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PREFACE

This Fourth Volume of the Studies in the History of Psychology and the Social Sciences is published later than usual. This doesn't mean, however, that the tradition of publishing the CHEIRON annual Conference Proceedings as quickly as possible will be discontinued.

We regret that in this volume as in the previous ones it has not been possible, for various reasons, to include all the papers presented at the 1985 Conference in Paris.

One essay in this volume, the last one, was presented at the 1984-Conference in Rome but was omitted from the Third Volume due to an editorial error.

We regrouped the essays according to their subject matter, but not in exactly the same order as the Conference Programme. This Programme is included in the book.

We owe our thanks to Adry van Es and Neline Ritsma of the Psychological Institute of Leiden University for typing the many pages besides their daily duties.

Leiden, January 1987 The editors.

CHEIRON - EUROPE
European Society for the History of behavioral and social Sciences
IVth annual Meeting
11-14 Septembre 1985

Centre National de la Recherche Scientifique
15 Quai Anatole France
Paris 7°

Wednesday 11 September

09.00 - 10.00 Opening Adresses

10.00 - 11.00 **Models of the historiography in the human and social sciences**
Chair: Ingemar Nilsson (Göteborg)
P.J. van Strien (Groningen) and G.C.C. Dehue (Groningen): A multidimensional model of historiography in the social sciences
Siegfried Sporer (Regensburg): Historiography, psychology and the context of utilization

11.30 - 13.00 **Philosophy and theory in human and social sciences**
Chair: Manfred Thielen (Berlin)
Irmingard Staeuble (Berlin): Konzepte der Geschichtlichtkeit des Menschen und die Idee einer historischen Psychology
Istva Magyari-Beck (Budapest): Psychology and Technique
C. Tögel (Sofia): Wahrnehmung und Widerspiegelung: ein unklares Kapitel der Historiographie von Psychologie und Philosophie

14.30 - 15.30 **Controversies on methodology in sociology**
Chair: Kurt Danziger (York)
Jennifer Platt (Brighton): The SSRC restudy of Angell's cases from 'The Family encounters the Depression'
Rein de Wilde (Groningen): From normal to abnormal discourses: a sociological interpretation of methodological controversies in Weiman sociology

15.30 - 16.30 **History of Crowd Psychology**
Chair: Kurt Danziger (York)
Jaap van Ginneken (Leiden): Tarde, Dreyfus and public opinion
Gregory McGuire (York): Hypnotic suggestion and criminal responsability

17.00 - 18.30 **Professionalization and institutionalization of human and social sciences**
Chair: Jaap van Ginneken (Leiden)
Alain Drouard (Paris): Analyse comparative des processus d'institutionalisation des sciences sociales dans les facultés de droit et dans les facultés de lettres au XIXème et XXème siècle
Pierre Desmarez (Bruxelles): L'étude des relations collectives de travail et les spécialistes de sciences sociales: examen d'une controverse

Geneviève Paicheler (Paris): La professionalisation du savoir psychologique aux Etats-Unis (1865-1924)

Thursday 12 September

09.00 - 10.00 **Re-examination of "classical studies" in psychology and social psychology**
Chair: Ian Lübeck (Guelph)
Frances Cherry (Carleton): The development of a classic study in social psychology: Sherif's summer camp studies
J. Verster (Groningen): The enquiry set up by Heymans and Wiersma in the Netherlands in 1905 reanalyzed in 1985

10.00 - 11.00 **History of Social Psychology**
Chair: Ian Lübeck (Guelph)
Dominique Le Dissert and Henri Paicheler (Paris): Pour une histoire récente et encore à faire de la notion de créativité: lois psychologiques ou lois du marché?
Robert Pagès (Paris): Structure et fonction de l'expérimentalisme psycho-social chez Charles Fourier

11.30 - 13.00 **History of psychology I**
Methodology and investigative practices
Chair: Jennifer Platt (Brighton)
A.D. Lovie and P. Lovie (Liverpool): Presupposition in Clark L. Hull's learning theory: evidence from curve fitting and other related topics
Trudy Dehue and G.H. de Vries (Groningen): Social origins of psychological methodology in the Netherlands (± 1900 till now)
Kurt Danziger (York): Social context and investigative practices: german and american patterns in the early 20th Century

14.30 - 15.30 **History of Psychology II: Theories**
Chair: Larry Ray (Lancaster)
Andrew S. Winston (Guelph): The use and misuse of Aristotle four causes in psychology
François Dachet and Paul Henry (Paris): Associationism and utilitarism revisited

16.00 - 18.00 Business Meeting

Friday 13 September

09.00 - 11.00 **History of Psychology III: Theories (continued)**
Chair: Hans Rappard (Amsterdam)
Roberto Cordeschi (Roma): Purpose, feedback and homeostasis: dimensions of a controversy in psychological explanation
Mathias Petzold (Düsseldorf): Analyse eines psychologischen Konstrukts aus wissenschafthistorischen Sicht: kognitive Stile
Ingermar Nilsson (Göteborg): David Katz as a swedish psychologist

14.30 - 16.00 **The making of History**
Chair: Luciano Meccaci (Roma)
Michel Plon (Paris): De l'"hérésie physiologique" à l'"obscénité scientifique": l'innomable dans l'histoire
Martin O'Brien (Lancaster): Making History
Michael Shortland (Lancaster): Hypocrisy in England: some contours of an historical problem

16.30 - 19.00 **Psychology and films**
Chair: Tibor Balogh (Szeged)
Helmut E. Lück (Leverkussen): Gestalt Psychologie und ihre Filme
Barbara Epstein (Wien), Karl Fallend (Salzbourg) and Johannes Reichmayr (Klagenfurt): Psychoanalysis and films in the Twenties
Presentation: "Geheimnisse einer Seele" (UFA, Germany, 1926) - Regie: G.W. Pabst, co-directors: Karl Abraham and Hans Sachs. 60'.
Some remarks to the origin in and development of cinematographic projects on psychoanalytical issues. Some remarks on the psychoanalytical movement in the Twenties

Saturday 14 September

09.00 - 11.00 **Psychoanalysis and Psychopathology**
Chair: Andrée Tabouret-Keller (Strasbourg)
Helio Carpintero and Maria V. Mestre (Valencia)
The impact of psychoanalysis in contemporary Spain (1940-1984).
Willem van Hoorn (Amsterdam): Modernism and the semantics of psychoanalysis
Régine Plas (Paris): De quelques particularités de l'histoire de l'hypnose et de l'hypnotisme en France

14.30 - 16.00 **Biographical data in history of psychology**
Chair: Alain Drouard (Paris)
Wolfgang G. Bringmann (Mobile) and Herbert Uhlein (Passau): How to study psychology: advice of a Jesuit, A.D. 1903
Fernando Vidal (Genève): Et la psychanalyse perdit Piaget
Jacques Vonèche (Genève): De quelques analogies entre G. Stanley Hall et Jean Piaget.

A RELATIONAL MODEL OF THE HISTORIOGRAPHY OF PSYCHOLOGY ILLUSTRATED BY THE DEVELOPMENT OF METHODOLOGICAL THINKING IN THE NETHERLANDS

P.J. van Strien and T. Dehue
University Groningen, The Netherlands.

Summary

In the relational model presented in this paper elements of the internalistic approach to disciplinary history are combined. The central idea is that a discipline develops in interaction with four "reference systems", partly within science (the forum of the discipline; the broader academic community), partly without (society at large; specific client-public within it).

According to the model four factors determine the dynamics of the historical development: (a) **Problems** that arise within science (IN) and within society (EX); (b) The methods, models and theoretical notions used as **"intellectual tools"** in dealing with these problems; (c) The establishment of **institutional cadres** within which scientific and professional traditions can be developed and handed down; (d) **legitimation and demarcation** activities, aimed at getting the contribution of the discipline accepted by the respective reference system.

In the paper the model is first applied to the dynamics of the development of psychology in general, and next in greater depth to the dynamics of methodological thinking in the Netherlands. Special attention is given to frictions between normative methodology and practiced methods.

In the paper on the social history of a disciplinary methodology Dehue (viz. p. 19-30 of these Proceedings) argued, that methodology - conceived as the set of rules which regulates scientific inquiry and differentiates science from other cultural domains - is not culture-free, but is itself part of and influenced by the surrounding culture. Her description of the changes in methodological thinking in successive stages of the social history of the Netherlands, serves to substantiate this thesis.

In the present paper the development of methodological thinking in the Netherlands will be used as an illustration of the historical dynamics of (social) science in general. A relational model, in which the production and use of scientific knowledge are seen as a response

to the questions and expectations of the different internal and external "reference systems" with which a science and it s practitioners have to deal will be presented, as an explanatory tool.

In the first section of this paper a brief sketch will be given of the relational model as such. In the second section, the main components of the model will be elucidated with the help of the early history of psychology. In the third, the development of methodological views in psychology in the Netherlands, as recorded in the paper of Dehue, will be described and explained in terms of our model. In this section the model will also be used to explain special frictions which appeared in the course of this development between normative methodology and methods used in practice.

1. A relational model of the dynamics of scientific disciplines

Few historians of psychology still adhere to a strict "internal" historiography of their subject, in terms of the succession of theories and schools and the great contributions of the "great men" to this. A strict "externalistic" historiography, which sees the development of the social sciences as a function of the technical and economic dynamics of society, appears to be equally unsatisfactory. The problem with present "contextual" theories of history is that they remain rather vague, because either they don't specify the way science and society interact or they focus only on a part of the total context. In our model the general notion of society is replaced by the concept of four "reference systems" with which scientists interact in the conduct of their discipline. And the general idea of interaction is replaced by four specific factors in the interplay of science and society. In explaining special aspects of the development of a discipline, the model is further refined by the introduction of one or more specific factors.

A. Four "reference systems" - As a fully grown science and profession, a discipline has to deal with four "reference systems":

(1) the internal "forum" of its own scientific and professional community;

(2) the wider "academic community" (scolars from other disciplines, the intellectual elite of the contemporary society);

(3) the "society at large" within which psychology is practized;

(4) the specific clients who makes use of the findings and professional services of psychologists (including private and governmental sponsoring and funding bodies).

The existence of these four reference systems is the result of a historical differentiation process, the description of which would go beyond the scope of this paper. Suffice it to say that in the course of the past centuries the academic community has come to take an increasingly specific place in western society, as an elite offering services to sponsors and clients, and exerting not only intellectual but also spiritual and moral leadership within the society at large. With the further differentiation and maturation of separate sciences specific disciplinary communities developed, which serve as a disciplinary "forum" which sets the norms for the scientific and professional conduct of those belonging to the discipline.

B. Four determinants of the functioning in science in society -

(1) The emergence of a new discipline and its further development results from "stimulation" by intellectual and/or social problems, that cannot be solved in a satisfactory way by the already available knowledge and/or methods. Stimulation can originate from any of the four reference systems listed under A. As a science matures, the internal reference system (1) becomes more and more important in generating problems that stimulate the further development of the discipline. In a similar way, a fully-grown profession tends itself to define new social problems that demand scientific attention.

(2) Inspiring methods, models and theoretical notions, already available in the surrounding intellectual culture, and - as far as already developed - in the discipline itself, are used and further developed as intellectual tools for answering those problems.

(3) The scientific and professional traditions that come into being in this way, require institutional cadres for their continuation.

(4) The further growth of a discipline depends on the degree to which it succeeds in convincing those from whom the questions originate of the validity and usefulness of the answers given (legitimation) and of their surplus-value compared to answers given on a different basis (demarcation). The reference systems (2), (3) and (4) can be seen as external "publics" towards which legitimation and demarcation activities are directed. In a mature discipline the legitimation of the work of individual scientists against the internal reference system

of the disciplinary forum (1) also becomes an important factor. In this respect the forum also fulfills the role of a public.

C.Specific factors - For full comprehension of the development of a discipline it may be useful to pay special attention to critical historical events and determinants, and to other specific factors. Examples of critical events are great wars or economic crises. An example of a critical determinant is the influence of religion in specific phases of the development of a discipline. Examples of other specific factors are the friction between the demands of different reference systems, and the influence of related disciplines and professions on the development of the discipline studied. Personal influences on the way scientific and professional traditions and institutional cadres develop, also belong to the category of specific factors.

In fact the issues treated in this paragraph should not be seen as additional factors, as much as specifications of the interaction between the discipline and its environment, as it manifests itself in the different components of our model.

Which specific factors should be taken into account depends on the aspect of the historical development studied. As we will see in the next section, in the study of methodological thinking the interaction between the normative methodology of the discipline as a science and the methods-in-use in professional practice appears to be an important clue for good understanding. The student of the professionalization of psychology will not get a complete picture without also drawing the efforts at professionalization by other, rival, professions in the picture, as for instance Geuter (1984) did in his study of the professionalization of psychology in Germany during the Nazi-regime. In a study of the relationship of psychology to related disciplines by the first author (Van Strien, 1984b), the claims of each discipline on specific segments of the "market" for professional services appears to be also an important factor.

In other studies yet other specific factors will appear to be important, for instance the role of religion (an aspect of the wider society which, as Van Strien (1986c) has shown, has had, and still has a notable impact on the way psychology and other social sciences have developed and interacted with their respective reference groups). One of the first steps of a historical analysis should be to

decide which factors to concentrate on, and which factors to leave out or treat only as background.

D. Phases or periods - An important element in a plausible exposition of the development of a discipline, is the ordering of the continuous course of history into phases. It can be said that development enters a new phase when there is a significant change in the functioning of science in relation to one or more of the aforementioned reference systems.

In our approach elements of the internalistic and the externalistic approaches to history are combined. In paying attention to the methods, models and theoretical notions used in dealing with the problems that originate from the forum of the discipline itself or from the wider academic community, justice is done to the internal determinants. By also taking account of the interaction with both society at large and the specific "client-public" applying for professional guidance and advice, the external element gets its fair share. Because the development of institutional cadres is one of the determinants in our model, institutional history also forms part of our approach. At first sight the biographical element, which plays such a great role in the "great men" approach in historiography, is missing in our model. The absence of a specific role for great men from our model does not mean we intend to ignore their contribution. In fact they are the "historical actors", who initiate new scientific and professional traditions, found institutes, and legitimate the discipline in lectures and publications within the academic community and society at large. There is, however, no reason to give them a place as determinants in their own right in the development of history. Their greatness lies in the creative way they define the problems, use new intellectual tools and so on. In so far as particular persons appear to be of interest in the analysis of the developments studied, they can be treated as specific factors in the sense of (C).

To summarize our exposition, we have represented the elements of our model in a diagram (figure 1).

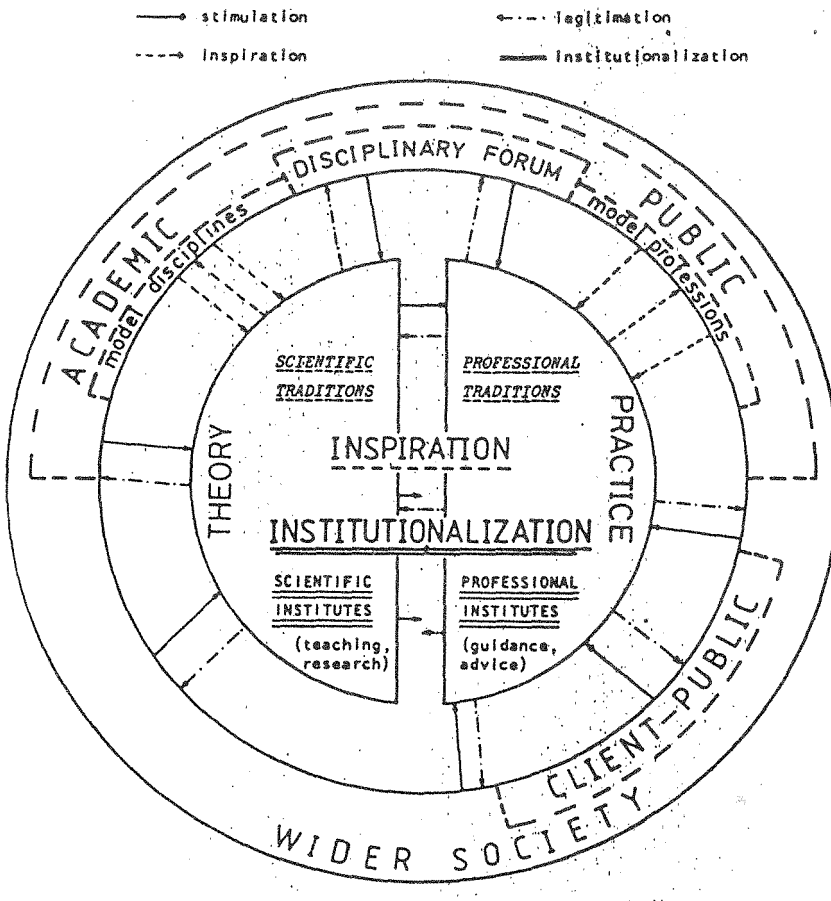

FIGURE 1: RELATIONAL MODEL OF DISCIPLINARY HISTORY

We have represented the wider society as the largest of a number of concentric circles surrounding the discipline. The academic community is represented as a smaller, narrower segment. The disciplinary forum and specific model disciplines and professions are represented as still smaller segments. In a similar way the "client-public" is represented as a segment of the wider society. In order to indicate that the border-lines between the four reference systems are fluid, we have used dotted lines to mark them. The four determinants are marked by different types of arrows and lines (explained in the heading of our figure). The discipline as a science is represented by the left part of inner circle, the discipline as a profession by the right-hand part. The inspiration by model disciplines evokes a number

of scientific traditions in the first, the inspiration by model professions a number of professional traditions in the latter. Institutionalization in both segments occurs, in the first in the form of scientific institutes, in the latter in the form of professional institutes.

2. The model applied to the history of psychology

To bring our model to life, we will now apply it to the development of psychology.

(1) Stimulation by problems. The new scientific problems that gave the impetus to the (gradual) formation of psychology as an academic discipline were broad "academic problems", that could not be solved by the already existing disciplines. So in the nineteenth century the new physiology of the senses and the nervous system appeared unable to give more than a partial answer to the old philosophical academic problems of the relationship between mind and body and between the contents of consciousness and the outer world. The psycho-physical paradigm, originally devised by physiologists and physicists, was further developed in the new psychological laboratories to explore the laws of mind and body from a psychological angle. New paradigms were added. As the discipline matured the broad academic problems of the first hour were gradually replaced by more specific disciplinary problems (e.g. the laws of learning and forgetting). Whereas at first it was a broad "academic public" who posed the questions and to whom answers were directed, the "forum" of the discipline itself eventually became the "reference system" with which psychologists primarily interacted. As we will see presently, the influence of other disciplines often remains an important factor in the way the problems are approached.

Beside the science-internal relationship to their own scientific community, and the relationships to the wider "academic community" of scholars in other disciplines, there is also the science-external relationship to society. The impetus for the take-off of psychology and its further development lay not only in intellectual problems, but also in moral and practical problems in the surrounding society and general culture, for which the available disciplines had no answer.

(2) Inspiration by models. To be able to answer the questions engaging the academic or the own disciplinary community, and the moral

and practical problems of the surrounding society or of specific clients applying for help and advise, psychology and its practitioners needed theories and methods that could serve as intellectual "tools" Especially in its early stages the discipline had few tools of its own at its disposal. It made use of the notions, methods and models available in the general academic and intellectual culture to guide its thinking. In his (1986b) article, the first author has shown how psychology has looked in turn to physics, biology and the humanities as model disciplines from which it could derive a promising approach to reality. In his (1986a) article, the exemplary function of model professions is discussed. The inspiration of these model disciplines and professions resulted in a number of competing research- and professional traditions in the laboratory and practice. In dealing with specific problems each tradition generated its own research- and practical "paradigms". The present "body of knowledge" of psychology is the product of this pluri-paradigmatix exploration of the human mind and of human and animal behavior. In experimental psychology the measurement culture of the science-inspired empirical-analytical tradition generated the psycho-physical paradigm and other laboratory-paradigms. In professional practice it generated the mental-test paradigm, which made it possible to predict performance on the basis of correlations between tests and criteria. In theoretical psychology the holistic "geisteswissenschaftliche" tradition generated phenomenological analyses of significant phenomena. In practice it gave birth to the projection test and to clinical psychodiagnostics. In their diagnostic and therapeutic activities psychologists were also influenced by the role-model of the medical profession. Of course psychology can itself also serve as a model-discipline, for instance with respect to pedagogics, but this is another story.

(3) Institutionalization and professionalization. The development and maintenance of research- and professional traditions in psychology involved not only a matter of individuals, but also of groups and networks of psychologists and practitioners who worked together, and influenced each other in institutional cadres: university departments, schools of scholars with master-pupil relationships, and professional institutes where the craft was practiced and handed down to newcomers.

Whereas intellectual questions, in the sense of the examples given under (1), stimulated the formation of psychology primarily as a scientific academic enterprise, moral and practical questions gave the impetus to activities directed at educating and enlightening the wider public, and to the founding of centers of professional services catering for the specific problems the client-public that gradually began to present itself.

Institutionalization is a precondition for the establishment of an independent discipline. The growth of academic psychology in Germany has been seriously hampered by the fact that up to the Second World War there were no independent chairs in psychology at the universities: nearly three quarters of a century after the founding of its first laboratory, psychology still had its institutional basis in philosophy (Geuter, 1984). The conduct of scientific inquiry demands institutional provisions, such as libraries, journals and conferences. Professional practice can prosper only when practitioners are able to build up a position, however limited, within the framework of institutes and bureaux. This institutionalized practice in its turn can win a recognized place in society only with the institutional backing of a professional organization. In the literature on professionalization the sociological processes and the means of giving a new type of services a firm institutional basis in society are described (see for instance Vollmer and Mills, 1966).

(4) Legitimation. The status of accepted, institutionalized science and profession can only be reached and maintained with the help of continuous legitimation and demarcation activities, which make the validity and usefulness of the methods used and the answers given clear to outsiders. As a new science psychology had to convince the academic community and the government of its right to an independent place in the spectrum of sciences.

As a profession psychology had to draw demarcation lines between psychological and commonsense knowledge, and to win the trust of the wider public and potential clients. Its professional organizations have been instrumental in this, for example by drafting ethical codes, which set rules for professional conduct and enact warrants for the quality of the services offered.

In an earlier Cheiron paper Van Strien (1984a) has given a picture of the various efforts at legitimation by Dutch psychologists in the successive phases of the development of the discipline (for

the content analysis on which part of this study has been based see Van Strien, 1983). Although it is now an established discipline, psychology still has to struggle for its share in the institutional means of the university and of the different research foundations. Within their own discipline researchers have to convince their colleagues in nomination committees, editorial boards, and so on, of the value of their work.

It is not the aim of this paper to enter further into the history of psychology in general. To show in more detail how our model can be used, we will focus now on one historical case: the development of methodological thinking in the Netherlands.

3. The social dynamics of methodological thinking

In our investigation methodology is studied not on the level of specific methods of measurement, testing, treatment, and so on, but on the level of scientific and professional traditions, which guide and regulate research and practice during several decades. In the Netherlands, and in Western Europe as a whole, the empirical-analytical and the "geisteswissenschaftliche" style, described by Dehue, form the two most prominent general research-traditions. In the first physics takes the role of model discipline, in the second organic-holistic biology and the humanities. Within each tradition several more specific approaches or programmes can be distinguished. Within the empirical-analytical tradition there was a shift from inductivism to predictivism. Within the "geisteswissenschaftliche" tradition the holistic understanding of "verstehende" psychology had to make place in the decades after the Second World War for existential insight and "encounter". In the internal historiography of psychology the controversy between the empirical-analytical and the "geisteswissenschaftliche" style of thinking is seen as a matter of varying academic climates. Changes in the course of time are seen as a matter of a changing "Zeitgeist" within the academic world.

Our relational model makes possible a clearer understanding of the successive changes in methodological thinking. As Dehue shows in her paper, Dutch psychology has drawn its inspiration in turn from the continental European and the Anglo-saxon intellectual climate. Internal factors, however, appear insufficient to account for the successive shifts in the style of methodological thinking. The influence of the "client-public" and of the wider society as

"reference systems" proves to be the leading force behind the changes in methodological climate, with professional practice functioning as the channel through which society exerts its influence.

The take-off of psychology as a separate discipline was a response to the problems that arose in the industrializing, rationalizing Dutch society during the first decades of the twentieth century, and to the resulting questions from the general public, and the specific client-public that applied for guidance and advice. The way the discipline and its practice developed appears to be influenced by the changing needs of the clients and sponsors of professional services, particularly in the psychodiagnostic area.

It is shown that these changing needs required different methodological tools and methodological rules justifying their use. In the psychotechnical period practitioners found their methodological inspiration in the empiristic measurement culture. When the selection of higher personnel required the appraisal of personal qualities, psychodiagnostics found their inspiration in holistic, "geisteswissenschaftliche" methodology. Until the mid sixties, the intellectual leaders of the discipline, who dominated the internal forum and made the methodological rules, were heavily involved in practice. Thus its demands had a substantial impact on methodological thinking.

The institutionalization component is not treated explicitly in Dehue's analysis. The powerful influence of the demands of practice on methodological thinking was only possible, however, because professional practice was for a long time the leading institutional force in Dutch psychology. When the universities became the dominant institutional force the roles were reversed, as we will show presently.

The influence of the need to win and to retain the approval of the external public (legitimation, acceptance) appears clearly from Dehue's exposition. So it is shown that at the end of the sixties a growing opposition made itself felt within the wider society against selection procedures in which "the souls rather then the achievements of applicants were examined". When the upsurge of democracy demanded "transparent" tests, developed within the "American inspiration", were the only diagnostic tools considered legitimate by the critical public (at least as far as decisions taken by persons others than those tested were concerned; as we will see next, clinical

psychodiagnostics remained the main method in situations in which clients approach psychologists for their own benefit).

So far we have focussed on the influence of practice on the methodological climate of the discipline as a whole. It can be shown, however, that there have also been pressures on practitioners from the side of the discipline to use methods which stand up to scientific scrutiny. These pressures became stronger when, with the influx of large numbers of students in the sixties the research staff at the universities became an institutional force in itself. For them the demands of practice were no longer important. Scientific recognition within the international disciplinary forum was their principal aim (see Van Strien, Takens and De Wolff, 1986).

In this new climate empirical analytical thinking became the dominant methodological view. Reflecting on the empirical foundation of psychodiagnostic statements, De Groot had already formulated his predictivistic principles in the fifties. In this view the "geisteswissenschaftliche" style of diagnosis is, as such, a subjective and authoritarian style. It can be made acceptable only when the truth, which may be contained in it, is put to the test by translating it into controllable predictions. If this is done, De Groot contends, authority is replaced by objective procedures: everybody can check with his own eyes! It is no longer the practitioner speaking, asking for a methodology which legitimates his work, it is the methodologist, with an eye on the critical forum of science, legislating diagnostic practice. After the publication of De Groot's book on Methodology (1961) his "empirical cycle" soon became accepted in psychological research methodology. The new empirical-analytical style was, however, at that time still out of tune with the current way professional practice responded to the needs of the client-public. De Groot's plea for objective procedures won broader support only when, first in personnel selection, the wider public began to demand matter-of-fact predictions, and nothing more.

In this new situation the phenomenological-existential tradition, which had provided the methodological legitimation for professional practice in the preceding period, was no longer satisfactory as normative methodology. From the discipline-as-reference-system came pressure on practitioners to use methods which could bear scientific scrutiny. From the external public came pressure towards transparent methods. In psychological practice the new empirical

style found its strongest response in practice in the area of personnel selection, especially when ethical protests arose against the old way of testing.

In clinical psychology, Barendregt, a younger colleague of De Groot at Amsterdam, professor in clinical psychology and psychology of personality, had started in the fifties a programme on scientific personality diagnosis, in which he applied the methods of empirical research to the single case (N = 1 studies), as Shapiro had done in the Maudsley clinic in London. In this way he hoped to give clinical practice a sound basis in empirical methodology. As Barendregt acknowledged later (1982), this programme failed because of the complexity of the variables involved. After this frustrating experience he started a new research programme on phobias.

In this situation a cleavage developed between theory and practice. The predictive methodology, to be sure, still leaves room for intuition and hermeneutical interpretation, but only as part of the "context of discovery". No methodological rules can be given for the "creative" generation of hypothesis. Students therefore are no longer trained in diagnostic interpretation. The only thing that matters in the new vision is that the resulting statements can be tested, with the result that the "clinical" thinking in the "geisteswissenschaftliche" tradition, while not prohibited, had been left in a methodological vacuum. When the dispute between clinical and statistical prediction produced in the feeling that the former was inferior to the latter, practitioners who could not cast their work so easily in an actuarial-statistical mould, found themselves in a legitimation crisis. Psychologists formerly proud of their interpretative achievements, suddenly found themselves looked upon as quacks by the scientific community which sets the methodological rules for psychological thinking. Young psychologists, raised in the new methodology, discovered not only that the tools used by the older colleagues in the field were suspect, but that the university had failed to provide them with a new kit. The only tools they had learned to handle were statistical devices to test hypotheses. How these hypotheses should be generated, however, remained an open question.

In clinical psychology, where Barendregt had left diagnosticians, hoping to be provided with decent tools to practice their craft, with empty hands, psychodiagnostics fell into disrepute. Quite

a few of them left the field of psychodiagnosis and turned to psychotherapy. The same happened in child psychology. In personnel selection a similar shift took place: testing lost its attraction and organization development and training became the favorite game. No doubt other factors have also influenced this development, but methodological respectability certainly has been one of the major reasons for this shift. Of course, from a methodological standpoint, therapy, organisation development, training, and other activities new for psychologists were no less, and perhaps even more suspect than diagnostics. The point, however, was that psychologists engaged in those new activities became part of the institutional networks of other professions - psychiatry, education, organization development etc. - with backgrounds less dominated by predictivistic methodology. Trends towards a new acceptance of hermeneutic and phenomenological methods within the broader academic climate could apparently permeate more easily into these disciplines than into psychology, possibly because of their more open view on the needs of practice.

Hofstee's "Research theory for the social sciences" (1980) did not make the situation easier for the practitioner. It left him a free hand in the diagnostic "context of discovery", and in what we will call "the context of communication" with the client. In the context of justification, however, it required the formulation of rival predictions on which diagnosticians could make a bet. Some empirical studies have so far been conducted on the predictive and therapeutic value of rival methods. Practitioners feel however that the opportunity for testing rival predictions about individuals is virtually absent in the clinical setting in which they work.

In figure 2 we have placed, in greatly simplified form, the development of the normative methodology within the discipline and the development of methods in practice along a time axis. The catchwords used in the columns should be understood within the frame work of the historical overview just given. The terms representing the "geisteswissenschaftliche" view are in italics. The names of the most important persons are in brackets. As the vertical curve shows, there has both in normative methodology and in the methods used in practice always been support for both the empirical-analytical and the "geisteswissenschaftliche" tradition. In the first phase, however, the former was dominant, in the second and third phase the latter. In the

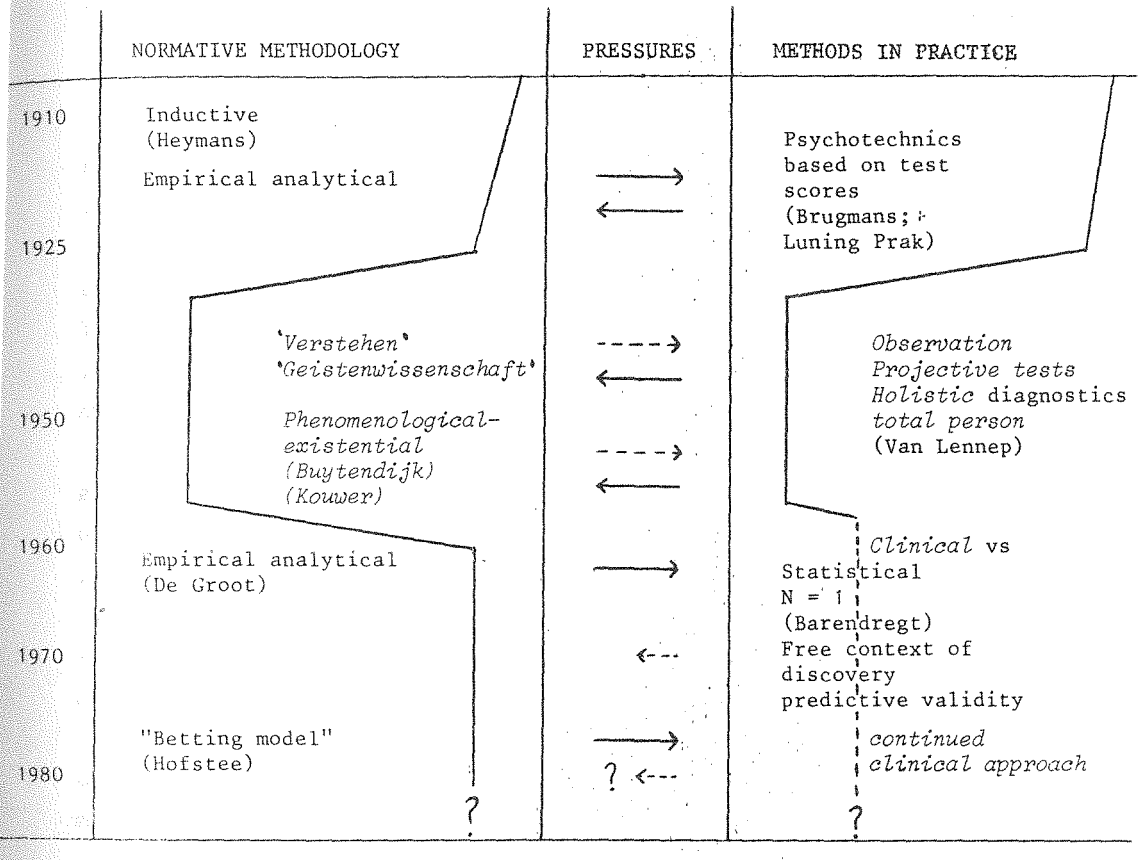

FIGURE 2: CONVERGENCE AND DIVERGENCE BETWEEN NORMATIVE METHODOLOGY AND METHODS IN PRACTICE

fourth phase the empirical-analytical tradition again became the dominant normative methodology. The methodological foundation of practice, however, has in the course of the sixties gone into limbo: the legitimation crisis we just sketched.

Practice has been characterised in the last decades by a mixed use of statistical and clinical methods with, however, a preponderance of the clinical approach. We estimate that at least 80% of the work of practitioners continues to be based on intuitive, clinical ways of reasoning. This applies not only to clinical psychology in the narrow sense, but also to work and organizational psychology, educational psychology, etc. This is partly due to difficulty in reducing different cases to a common denominator and partly to the previously mentioned liberal attitude that empirical-analytical thinking has towards the "context of discovery" of psychological statements. Most practitioners working on clinical lines seem to have accepted the empiristic normative view. They defend their way of working as a methodologically inferior, but inavoidable procedure, given the insufficient psychometric hold in the individual cases with which they have to deal. The consequence of this situation is that the majority of applied psychologists work on lines that have a dubious standing in the eyes of methodologists. Others have drifted away form the community of their own discipline. Discovering that this is the case may be one of the reasons that some representatives of the scientific part of the community have started to look for other methodological models, more propitious to practice and its legitimation.

In our table the disparity between the official methodological rules that are taught at the university, and the way practical cases are handled is apparent from the incongruence between the curves indicating the place empirical methods have in normative methodology and in professional practice.

Our figure represents only the dimension of the interaction between the normative methodology of psychology as a science and methods used in practice. It would have been possible to add other columns, representing the way the other components of our model have exerted their influence in the course of time. We have refrained from doing so in order not to make the figure too complicated.

In fact the analysis of the tension between theory and practice given in the last few pages is an example of the elaboration of a

specific factor in the sense of paragraph (C) of the exposition of our model. In terms of our model the tension between normative methodology and methods used in practice is ultimately the tension between the relationship with antagonistic reference systems. The orientation of the normative methodology is a research orientation, which hopes to gain the respect of the leading minds within the disciplinary forum and the academic community; the orientation of practice is a professional orientation, directed towards the client-public and society at large as the principal reference systems.

In other studies conducted within our programme, we hope to demonstrate the heuristic and explanatory value of our model on other themes pertaining to the history of psychology.

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THE SOCIAL HISTORY OF A DISCIPLINARY METHODOLOGY
PSYCHOLOGY IN THE NETHERLANDS, 1900-1980

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Summary

In this article the history of methodological development in Dutch psychology serves as a case study. It illustrated that the historical development of methodological rules is not purely a matter of accumulation of logical insight. In order to preserve the demarcating function of methodological rules, Dutch psychology appears to have readjusted its methodology in interaction with general social changes. The cultural context was, as one could say, assimilated in the methodological rules. Assuming (for the time being) that the findings of this case can be generalized, the received view on methodology as a 'sieve' sifting value-loaden chaff as much as possible from empirical wheat, has to be readjusted.

Introduction

Until the 1950s, Dutch psychologists saw neither place nor purpose for predictions in the methodology of their discipline. Heymans, the founder of academic psychology in The Netherlands, for example wrote in 1911 that psychologists yet would have to collect and analyze facts for centuries before they would be able to predict (Heymans, 1911, p.30). Again, at the 8th International Psychological Congress in 1926, he contended: "We should abstain from prophesying and learn to labour and to wait" (Heymans, 1926, p.366). A 1938 Dutch Dictionairy of Psychology (Van Essen, 1938) related prediction to visionary perception, divine inspiration and soothsaying, rather than to scientific psychology.

A few decades later however Dutch psychologists had completely changed their minds. Prediction had become a central notion in their methodological views, and it has remained so till the present day. Since the early sixties, most students of psychology in The Netherlands have learnt their research methods and philosophy of science from De Groot's Methodology of the Behavioural Sciences (De Groot, 1961), which tells them that every serious empirical investigation

starts with a precise prediction of its outcome, and that the main aim of science is to predict. De Groot's statement: "If one knows something to be true, one is in a position to predict; where prediction is impossible, there is no knowledge", has become a much quoted dictum. There is little doubt that prediction is still the key-concept of methodological thinking in Dutch psychology. Among the most radical predictivists is Hofstee, until recently president of The Netherlands Institute of Psychologists. In his Research Theory for the Social Sciences (Hofstee, 1980), Hofstee proclaims that all statements in the social sciences should be conceived as intentions to predict. Even after testing statements continue to be no more than merely predictions. Moreover, in Hofstee's view, scientists' reputations ideally should depend on the precisely calculated accuracy of their predictions.

In this paper, I shall analyse this historical development from radical inductivism to radical predictivism. That however is not my main purpose. First of all, I will explain and illustrate some reasons for research on the history of disciplinary methodology. Dutch psychology serves as a case-study to exemplify the reflections to be presented at first.

The philosophical interest of historical research

For the relevance of historical research on the development of a disciplinary methodology to become clear, the function of methodology must be considered. This function is twofold. Firstly, methodology serves to differentiate science from non-science. It expresses what is special about the approach of the scientific sector. Particularly in the social sciences great stress is laid on methodological prescriptions. This is so because the research subjects of these sciences are also the subject of many other social sectors, such as journalism, literature, philosophy, theology, social guidance and politics. The existence of the scientific sector is legitimated by its special methodological guarantees. For this purpose of legitimation methodological rules must be, or must be made, understandable and acceptable to the clients of the scientific sector involved and to its broader public (see Van Strien & Dehue, this volume). Methodological rules cannot be subscribed to or abandoned by the scientific community completely independently. As one could say, differentiation demands integration.

Second: methodological standards serve as rules of conduct for the members of the scientific group. They tell them how to do their job in order to contribute to the group's common aims. In the 'received view' of philosophy of science and in most contemporary scientific groups, the production of empirical knowledge is considered to be thé aim of science. The concept 'scientific' is used almost synonymously with 'empirical'. So, compared to other sectors, the scientific sector is considered to be particularly the one that specialises in the production (the 'discovery') of empirical facts, unadulterated as much as possible of personal and social preconceptions, and not the one for instance that delivers special interpretations and experts' opinions on value matters. Methodological rules are devised with this purpose of pure empirical knowledge in mind. Methodology is thought to function as a sieve, sifting value-loaden chaff as much as possible from scientific (i.e. empirical) wheat.

In this way not only is science thought to be shielded from social influences, but also is society thought to be protected against a politically powerful scientific sector. By the methodological separation of science and society the social role of science is confined to the provision of information. The distinction between fact and opinion guarantees that science does not interfere in democratic decision-making. Socially speaking science is just a supplier of knowledge; it does not meddle with policy-making.

The function of historical research on the development of methodology can now be explained. Closely connected with this 'received view' on methodology as a sieve of opinion from facts, is a special view on the development of methodological codification. As far as it is conceded at all that methodology changes over time, it is taken for granted that its development is a progressive process, i.e. a process of growing philosophical insight into thé rational methods of scientific inquiry. The historical development of methodology and its contemporary outcome are conveived as being a matter of progressive discovery of the one best way to contribute to the growth of empirical knowledge. The construction of the sieve is supposed to be improved step-by-step. This view on history for instance is clearly expressed by De Groot in his previously mentioned book 'Methodology': "The methodology of the empirical sciences has been evolved in a slow process of growth over the centuries. In this process an open system of criticism has always performed an important

productive function. Through the continuous interchange among scientists of ideas and information, it has become possible to formulate commonly accepted standards and to develop corresponding methods and techniques for scientific thought and inquiry" (1961, p.24).

According to the received view on methodology, methodological development indeed must be an accumulative discovery of standards and cannot be connected with contingent factors such as social and cultural changes. For in the latter case methodology would seem not to serve its sifting function properly: the screen of the methodological sieve would not (only) depend on logical arguments, but would (also) be modelled after the temporary social and cultural demands. As a consequence neither the output of the scientific sector nor its social role would ever be as neutral as it is believed they can be, given detailed methodological legislation and scrupulous researchers. The scientific product would inevitably bear the mark of the period of its production. Not even in principle would pure empirical knowledge be obtainable.

It is this idea of the historical development of methodology as an accumulative discovery of timeless principles, that will be contested in the following, using Dutch psychology as a case. It will be shown that in order to preserve the demarcating function of methodological rules, Dutch psychology had to readjust its methodology in interaction with general social developments.

The psycho-diagnostical background of prediction

At first view the emergence of predictivism in Dutch psychological methodology might seem to be nothing other than the general falsificationist turn in philosophy of science. However, a closer look reveals quite idiosyncratic traits in this Dutch psychological methodology (which need not to be further explained here). As a matter of fact its predictivism did not emerge from general philosophy of science. It stemmed primarily from reflection on the foundations and methods of one of the branches of applied psychology itself: that of psycho-diagnostics.

Psychology in The Netherlands mainly professionalized through its involvement in psycho-diagnostics, i.e. in the investigation of individual qualities and capacities, mostly for purposes of personnel selection and vocational guidance (cf Eisenga, 1978). This also holds good with regard to the most explicit predictivists, De Groot and

Hofstee. De Groot's Methodology for the behavioral sciences, as well as Hofstee's Research theory for the social sciences grew directly from reflections about psycho-diagnostics. De Groot makes this clear in the preface of his book: originally planned as a discussion of methods in psycho-diagnostics, the book turned out to become instead a treatise on general methodology. The attentive reader has no problem tracing the main lines of Hofstee's 1980 theory back to his book Psychological statements about persons, published six years before (Hofstee, 1974).

This is an important clue. It suggests that the issue of the acceptability of methodology to the public outside the discipline, can be focussed on the history of psycho-diagnostics and the changing views and demands of its specific public.

The developmental history of methodology in Dutch psycho-diagnostics can be devided into four periods: ca 1910 - ca 1925, ca 1925 - ca 1945, ca 1945 - ca 1965, and ca 1965 - ca now. In these four periods two methodological styles opposed and influenced each other, one a mainly Anglo-saxon oriented empirical-analytical style, the other a mainly Continental-European oriented 'geisteswissenschaftliche' style. The first style led to analyses of occupations in separate sensoric, motoric and cognitive abilities, the construction of tests for these abilities and the establishment of correlations between test scores and occupational behaviour. The second stressed interhuman understanding, relying on intuition of the experienced and gifted psychologist. In this methodological thought-style not individual achievements, but the 'inner self' of the unique person was the object of investigation.

From the take-off of psycho-diagnostics until about 1925, the empirical-analytical style was dominant in the Netherlands. In the second period, until about 1945, the 'geisteswissenschaftliche' style held the reins. The period between 1945 and 1965 is characterized by internal developments of both styles and a vehement confrontation between them. In the end the empirical-analytical style won and it has continued to dominate since then.

An important question that must be answered now is, how these changes could be made acceptable socially, i.e. how they were linked with view of the users of psycho-diagnostics. Of relevance here is that, in contrast e.g. to the United States, the main users of psycho-diagnostics in The Netherlands were for a long time neither

the private industries nor the army, but a conglomerate of confessional and public institutions set up for social guidance. These institutions had, as the head of the 'Amsterdam Municipal Bureau' expressed it, an "economical-pedagogical" task (Van Det, 1930). Their slogan "the right man on the right place" was considered to express humanitarian as well as economic requirements. They aimed at supporting the working people in their mental adaptation to the industrializing and rationalizing society, where general moral standards were dropping, and where neither one's place in a small community nor one's occupation any longer provided personal identity. Society increasingly stressed individual differences, individual choices, responsibilities, and rights. The methodological changes in psychodiagnostics were carried through by their association with this so called individualization process, which in its turn was linked up with economical developments.

The social history of psycho-diagnositical methodology

Psycho-diagnostics emerged as a profession in the beginning of the twentieth century when rationalization and mechanization of business and industry rendered inadequate the traditional, mainly classbound, ways of allocating workers to jobs. Society asked for selection of personnel based on individual differences. Increasingly common sense judgements were replaced by professional assessment of abilities.

Like their collegues in the U.S., the academic psychologists in the Netherlands saw their chance. Their elaborate equipment and statistical techniques for research of laws into human functions, could also be comployed for the measurement of individual differences. Experimental psychology's image of man as 'a bunch of motoric and sensocric capacities' fitted well with the needs of mechanized industry.

The so-called 'psycho-technicians' analyzed tasks and made up lists of required skills for which simple tests were devised. For example, to test their skills in concentration, applicants for a job as telephone-operator had to write down the alphabet for one minute, while simoultaneously saying aloud the series of odd numbers. In spite of Heymans' warnings against prediction, these 'psycho-technicians' soon came to formulate the aim of their trade as the prediction of later achievements. They also started to check the predictive ability of their tests empirically, mostly by testing a trial group

of already practized employees and by using the test-results as predictions of the employer's judgements on the acievements of the workers.

During the twenties, rationalization became the big issue in trade and industry. White collar jobs and managerial functions increased. The secretariat, foreign trade, accountancy etc., all now needed personnel with specific mental qualities, rather than motor and sensory skills. Charactertraits such as 'trustworthiness', 'punctuality', ambitiousness' and 'personal stability' had to be determined by professionals.

This time the American way was not followed. In the U.S. traits were most often investigated by questionnaires, and correlations between scores and achievements were calculated. However, because of the pedagogical part of the "economical-pedagogical task", Dutch psycho-diagnosticians were now avoiding these 'instrumentalistic' methods. The character traits to be investiged were considered to be moral faculties, and the pedagogical aspect of psycho-diagnostics became increasingly stressed. The psychologists dominating the scene in Dutch psycho-diagnostics of the 1930s and 40s looked to Germany for inspiration. The former ways of testing were increasingly abandoned and their place was finally taken by interpretative approaches, that aimed at examining the person's 'inner self' instead of predicting future achievements. Techniques were designed to bring a person to 'self-disclosure'. Interhuman understanding and intuition became the main routes to psycho-diagnosis.

This image of man fitted well with the continuing cultural individualization process. Personal goodness and badness no longer had to be seen as a matter of following fixed ethical or religeous rules. Individual personality traits were stressed instead. Rigid standards for behavior were no longer useful in modern society, which needed more flexible people able to maintain their ground in wider social networks. People had to acquire self-images as independent individuals. After the Second World War this 'progressive' process continued, with psycho-diagnostics as active participant.

After the war the economy had to be rebuilt. The 'Human Relations Movement' was imported from the U.S.: hearts and minds of the employees should be invested in their work. Central notions of the Movement were, for instance, 'personality', 'responsibility', 'motivation' and 'democratic leadership'. The employee had to become a

fellow-man, and Marshall-plan money was spend on social-scientific accompaniment of this process.

The achievement of personal independance was the new moral aspiration in Dutch psycho-diagnostics. The industrial psychologist Rutten for instance contended in a lecture to a Catholic association of intellectuals that Catholic teaching often misused the concept of personality by associating it with virtuousness, and education of humans to religious beings and children of God. What is crucial however, stated Rutten, is an independent way of life; independance of conventions and social background. Becoming a personality demands what Gabriel Marcel calls "une fidelité créatrice" (Rutten, 1947).

'Geisteswissenschaftliche' as well as empirical-analytical psycho-diagnostics changed its foundations and methodology. Increasingly the 'geisteswissenschaftliche' approach stressed personal freedom. Character-traits became considered as changeable existential choices. Psychological judgements of character, that made traits appear fixed, were now considered as veiled behavioural norms. It was stressed that psychologists should refrain from giving directions for behaviour and should offer only insight into the client's being, which should lead to his personal responsibility for his becoming. In that case prediction of behaviour was impossible. A new notion: 'the encounter' was introduced in psycho-diagnostics just after the war. By an encounter, one understood a completely open and equal dialogue between psychologist and client. The client's self-disclosure should serve nobody but himself, and should not be reported to any other party interested.

In the same period empirical-analytical psycho-diagnostics also opposed value judgements and paternalistic relationships in professional practice, but in quite another way. Prediction was proclaimed as the central methodological notion. De Groot introduced it mainly because of ascribed shortcomings of the 'geisteswissenschaftliche' style. The main idea was that a psychologist, however gifted and experienced he might be, remains a human being open to being misled by his prejudices. A psychologist's judgements can only be right, and this justifiable, if the psychologist has considered them to be testable predictions and has submitted them to severe empirical control.

Having been raised in the empirical-analytical tradition, De Groot applied to the 'geisteswissenschaftliche' style a

methodological idea, which had its roots in very early empirical-analytical test psychology. He proclaimed this idea to be the central notion of psycho-diagnostics (and later of methodology in general). In a "Statement of Policy" he defended his predictivism with the position that "social, yes democratic" ideals were at stake (De Groot, 1952).

The Human Relations Movement aimed at equivalence and at diminution of paternalism only as far as this was thought to contribute to a bigger productive capacity. The employers asked for reports on applicants and not for encounters. So the 'geisteswissenschaftliche' assessment psychologist restricted himself to his old methods and tried to live with the dilemma of filching the self-disclosure from his fellow-man for the benefit of somebody else.

However, the period after 1965 would release him from his sorrow. By the early 1960s the economic rebuilding had been accomplished; the workers could turn their grievances into claims. Individual responsibilities were felt to have their counterpart in individual rights. Instead of consultations, negotiations were conducted between employers and employees. The workers began to fight for their interests as far as the social framework would allow. Given the inevitability of selection, they would rather have their achievements examined than their souls.

Again the pedagogical background of psycho-diagnostics asserted its influence, albeit in quite another way: for the first time in the history of assessment-psychology its most important interlocutors were not the employers but the workers. In newspaper-articles and interviews the applicant was told to stand up for his rights. For instance, in a popular magazine in 1969 Van Strien advised the applicant to claim his right to privacy and good treatment, instead of showing up in his Sunday best and awaiting modestly. Employers moreover should not be given information about somebody's inner self. They should make do with a report on expected occupational behaviour. De Groot introduced the criterion of the 'acceptability' of tests as a moral criterion. The interests of the subject should be taken into account. He considered predictive ability of tests and respect for a persons privacy to be part of this criterion of acceptability, and he contended that "procedures in the need of a human judge" therefore should be discarded (De Groot, 1970).

The trade union for higher officials was the first to act upon his message. The State Psychological Service (the RPD), that selected all top-officials, was severely attacked by the unions, the Netherlands Institute of Psychologists, journalists and politicians in concert. Questions were asked in Parliament, and eventually a Governmental Commission was set up to investigate the desirability of legislative settlements for the protection of the interests of applicants. In this way 'geisteswissenschaftliche' selection procedures were increasingly brought into discredit.

So, it were not methodological considerations as such, but social and political arguments that finally were decisive. Contemporary predictivistic methodology owes its leading position to its greater affinity with the changed cultural circumstances.

Conclusions

In this paper it is shown methodological rules in Dutch psychology are bound up with images of man and with general moral standards. Parallel with changes in the economic and social structure of Dutch society, the images of man and the moral standards in society at large changed. To preserve the differentiating and legitimating function of methodology, Dutch psychology had to keep in touch with its clients. It attuned its methodological codification to the progressive needs of society. The currently predominant methodological style in Dutch psychology is not the outcome of a pure accumulation of timeless methodological principles. Methodology thus is not a matter of context free logic. The good reasons contemporary Dutch psychologists may have for their predictivism are good reasons given the contemporary cultural context. In other social circumstances there were, and most probably will be, other good reasons for other rules.

This implies that methodology does not fulfil the sieving function in the way accredited to it in the received view. The cultural context is, as one could say, assimilated in the methodological rules. The assumed frontier in fact is a smuggle route. Nevertheless, as the case suggests, this does not necessarely mean that methodology does not filter at all and does not in fact differentiate science from neighbouring sectors. The history of Dutch selection-psychology suggests that it is not methodology's guarantee of greater verisimilitude that legitimizes the existance of the scientific

sector, but its painstaking elaboration of the changing cultural demands on problem-definitions and -solutions. In its specific sector it sieves out socially and historically inadequate means and ends. Predictivistic methodology in Dutch psychology for instance offers more intersubjectivity then its predecessors and more detached information.

So social science appears not only to specialize in explicating and integrating the existing social demands, but also in adapting them when they are socially outdated. In other words: social scientific disciplines seem to apply themselves to the exact definition, and if necessary, redefinition, of problems, and to the design of work methods which meet the new criteria as precisely as possible. Now, if this description of methodology can be made plausible for more cases, it does have important consequences for the social role of the (social) sciences: it implies that they inevitably play an active part in the making of policy.

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HISTORIOGRAPHY, PSYCHOLOGY, AND THE CONTEXT OF UTILIZATION

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Introduction

Historiography in the history of science in general and in the history of psychology in particular has primarily dealt with the context of discovery and the context of justification of knowledge. Traditional philosophy of science, i.e., logical positivism and Popper's falsificationism, has restricted itself to the context of justification. Neither philosophy of science nor historiography have concerned themselves much with a third context, the context of utilization, i.e., the potential and actual application of knowledge as well as the effects of knowledge, intended or unintended, upon society (Verwertungs- und Wirkungszusammenhang, Autorenkollektiv Leipzig, 1968, cit. in Friedrichs, 1973; cf. also Autorenkollektiv Wissenschaftspsychologie, 1975).

However, more recent approaches to the study of science (or psychology) as a social enterprise, e.g., critical history, sociology and psychology of science, have emphasized the context of utilization of knowledge as well as the context of discovery. (Table 1 summarizes in tabular form the relative areas of emphasis of these various approaches. The plus signs indicate the respective contexts of knowledge traditionally investigated. This paper will deal primarily with the right-hand column of this table, the context of utilization).

The purpose of the present paper is to argue that philosophy of science, history, sociology and psychology of science could all benefit from and contribute to this additional distinction, the context of utilization of knowledge. The distinctions of these three contexts are abstracted from an analysis of the philosophy of science underlying much of recent psychology. They provide a schematic framework that could aid communication and cross-fertilization among these various disciplines.

Traditional philosophy of science has not concerned itself with the utilization of knowledge. It does not consider this a legitimate subject for "proper" science, at least not for scientists in their

role as scientists, but at best in their role as concerned citizens. However, these traditional views, as exemplified by logical positivism and Popper's falsificationism, have not gone unchallenged (e.g., Adorno, 1976). I will introduce some of these challenges for discussion, and show how they relate to the business of science (or psychology) as a social enterprise.

Therefore, my aim in this paper will be four-fold: (1) To briefly review the philosophy of science underlying much of psychology, and how this view has been surpassed by traditional approaches to philosophy of science. (2) To show that these more sophisticated approaches have restricted themselves to a discussion of the context of justification; and that they should be supplemented by a reflection of both the context of discovery and the context of utilization, all three of which are inseparably intertwined in the research process. (3) To introduce criteria of research beyond the ones generally considered by philosophy of science, and to argue for the necessity of a rational reflection upon their relative weighting. (4) To sketch some of the general implications of the consideration of the contexts of discovery, justification and utilization for an understanding of science (or psychology).

Before we discuss these specific points we should briefly note what philosophy of science can do for us. It is controversial whether philosophy of science can equip us with the norms to conduct research properly or whether it is primarily a rational reconstruction of science as practiced, serving both descriptive and critical functions (cf. Groeben & Westmeyer, 1975; Opp, 1970; Reichenbach, 1938). Either way, we have become increasingly disillusioned - as much of the recent "crisis-talk" in social psychology and other sub-disciplines indicates (Mertens & Fuchs, 1978) - with the prescriptions that have been handed down to us from a philosophy of science that has been abstracted from a reflection of the natural sciences only. A philosophy of science appropriate for psychology as a social science should also reflect abstractions of the specific problems and peculiarities the social sciences deal with.

The "naive empiricist view" of science

A cursory inspection of psychology textbooks reveals two preferred modes of dealing with philosophy of science issues: these issues are either ignored altogether, or the occasion is used as a welcome

opportunity to indulge in lavish praise of "the scientific method." For example, consider a description of the so-called scientific method taken from a representative socail psychology textbook (Wrightsman, 1977; cf. also Graham, 1977; both adopt a scheme from Kemeny, 1959; see Figure 1). The scientific method is described as a cyclical process with induction, deduction, and verification as its three stages.

As neat and plausible as this scheme may seem, it is outdated and in blatant disagreement with practically any philosophy of science view held today (from the Vienna and Minnesota Circles to critical rationalism and critical theory). Step 1 - to start with observed events and arrive at theories via induction -, despite its popularity among psychologists (Holzkamp, 1970b), is characteristic of the "narrow inductivist conception of scientific inquiry" which is untenable (Hempel, 1966). Step 3, verification, has also come under attack by Popper's falsificationism (Popper, 1959, 1965). Taken together, these criticisms unmask this model as a "naive empiricist view of science" (Holzkamp, 1970a), long surpassed by traditional philosophy of science views.

Context of discovery, justification, and utilization

How has it become possible that we psychologists have come to diverge so much from the norms of natural science that we have so fervently emulated? Partially, of course, the fault lies with our philosophical naiveté and lack of concern with the advances in philosophy of science. On the other hand, it could be argued that traditional philosophy of science has provided us with a truncated model that omits per definitionem many of the issues especially pertinent to the social sciences. Popper (1959), for example, has argued quite explicitly that philosophy of science is to concern itself only with the logic of knowledge underlying the deductive method of testing, but not with the act of conceiving and inventing a theory. To use Reichenbach's (1938; cf. also Herschel, 1830; Losee, 1972) apt distinction, the philosophy of science should be concerned with the context of justification, but not the context of discovery. As a consequence, the issues constituting the context of discovery are not subjected to rational reflection and critical analysis. These issues include the selection of the problems to be investigated, the choice of explanatory theories, the external determination of research

interests through funding, and many others (cf. Friedrichs, 1973; Holzkamp, 1972; Moscovici, 1972; Ravetz, 1971; Spiegel-Rösing & Price, 1977).

A critical analysis of the context of discovery indicates the societal embeddedness of the enterprise science, but is nevertheless an insufficient supplement to the time-honored context of justification considerations. As Horowitz (1967) has noted, "the question of who sponsors research is not nearly as decisive as the question of ultimate use of such information" (p. 37). It is therefore suggested that Reichenbach's dichotomy of the context of discovery and the context of justification be supplemented by the context of utilization. The latter entails a reflection upon the social functions of science, such as the potential and actual effects of an empirical investigation (cf. also relatedly, but narrower in scope, Singer and Glass' (1975) suggestion of a 'context of advocacy').

Criteria of research

The classic philosophy of science traditions have restricted themselves to the context of justification, and have consequently excluded questions of content and application. However, this laissez-faire attitude does not seem adequate anymore in the light of the exponential growth of potential research topics (Groeben & Westmeyer, 1975; Price, 1963). This attitude ought to give way to more rational reflection on the criteria of problem-selection - the so-called criteria of external relevance (Holzkamp, 1972). Habermas (1965, 1971), in his analysis of Knowledge and human interests, has deveoped several criteria, and Holzkamp's (1972) critical theory has extended them into the domain of psychology.

One such criterion is 'technical relevance', an Erkenntnisinteresse that deals with the control of nature, and by extension, of humankind (Habermas, 1965, 1971; McCarthy, 1978). It is an important criterion both with respect to the context of discovery and the context of utilization. But it also has its dangers. In conjures up the image of Orwell's 1984, and therefore needs to be balanced by 'emancipatory relevance' that postulates the need for research that makes our societal dependencies transparent, and contributes to emancipation from them (Habermas, 1965, 1971; Holzkamp, 1970a and b; cf. also Adorno, 1976, Frisby, 1972, 1974, McCarthy, 1978; Radnitzky, 1968; Ray, 1979; Wolff, 1975; for critiques, see Albert & Keuth,

1973; Apel, 1970; Böhler, 1970; Brocke, Röhl, & Westmeyer, 1973; Dallmayr, 1972; Herrmann, 1976).

These external criteria will have to be reconciled with the internal criteria, and their weight relative to each other determined (cf. Heckhausen, 1971). This weighting procedure as well as the individual criteria should be subjected to critical rational discussion.

Implications

A reflection on philosophy of science - both in its traditional and its more controversial versions, including history, sociology, and psychology of science - could improve theory and practice of any discipline through the development of explicit criteria of research. Recognizance of the pluralism in philosophy of science could also free the practicing scientist (or psychologist) to venture bolder theoretical and methodological approaches.

Besides these potential payoffs for the practitioners of science (or psychology), the proposed distinction into the contexts of discovery, justification, and utilization of knowledge offers a schematic framework that philosophy, history, sociology and psychology of science could fruitfully employ to further discussion across their disciplines. This cross-fertilization could help us to recognize and fill the gaps in areas that have been relatively understudied. The goal is a deeper and more comprehensive understanding of the enterprise science (or psychology) through the consideration of the contexts of discovery, justification, <u>and</u> utilization.

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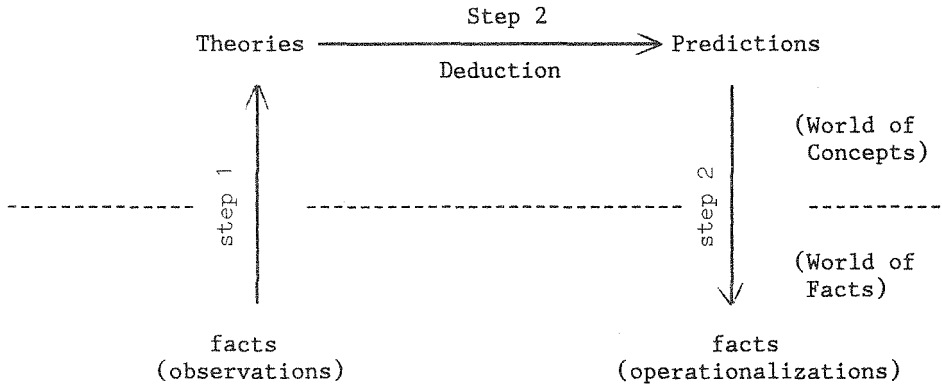
Table I.

Approach	Areas of emphasis		
	Context of Discovery	Context of Justification	Context of Utilization
Philosophy of science	-	+	-
History of science	+	+	-
Sociology/psychology of science	+	-	+

+ = traditionally dealt with
- = not traditionally dealt with but should receive more emphasis

Figure I. The "naive empiricist view" of science[a]



[a] From Wrightsman, 1977; the heading is mine.

CONCEPTS OF THE HISTORICITY OF MAN AND THE PROJECT OF A HISTORICAL PSYCHOLOGY

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Summary

While historians of psychology have increasingly become aware of the historicity of the discipline extending from the constitution of problematics to concept formation and theorisation, the historicity of human subjectivity itself is still widely neglected by psychologists in favour of a view of 'ahistorical man'. Approaches to a historical psychology thus deserve some considerations concerning the problems of a conceptualisation of 'historical man'.

Departing from Horkheimer's essay on 'History and Psychology' (1932) and the task Horkheimer suggests for the project of a historical psychology, this paper argues that renewed approaches to a historical psychology would need to review the currently competing paradigms of critical social theory in order to make a justifiable option with regard to the implicit views of history and subjectivity. Three competing paradigms - production, communication and power - are outlined and discussed in terms of their implications for the project of a historical psychology.

Scientific psychology is as much of a historical phenomenon as human subjectivity. Recently, historians of psychology have developed a sophisticated consciousness of the historicity of psychology. The naieve naturalistic view that psychologists simply find their objects in the world as raw givens, has given way to the view that psychologists construct their objects within a specific social context, a specific understanding of science and with particular knowledge-constructive interests (Danziger, 1984).

This view of the historicity of theory formation in psychology contrasts strangely, however, with the fact that the object area of psychology - human subjectivity - is not regarded as a historically changing object. The presupposition of 'ahistorical man' is still prevalent.

The question of why psychologists stick to an ahistorical view of their object and why they are not concerned with men as historical subjects is, I think, also an important issue for historians of psychology. First, it raises the question of why competing projects of a 'historical psychology' have not, so far, succeeded. Second, if the history of psychology is conceived as a critical self-reflection of the discipline and a means of clarifying perspectives for future research, we cannot behave indifferently towards the most basic decisions about knowledge-constitutive interests in the discipline.

The idea of a historical psychology had been around since the days of Dilthey, who not only formulated the need for a psychology which would be of use for historical research, but also outlined some proposals for a conceptualisation of men as historical beings. In the face of rising fascism and Hiroshima the need for an historical psychology was urged repeatedly - by philosophers like Horkheimer and Anders, sociologists like Mannheim and Gehlen, historians like Febvre and Mandrou - though not by psychologists. Since the turn to social history and the history of mentalities gained influence after World War II, there has been much debate on the relationship between history and sociology, while the 'dark continent where psychology struggeles with history' (Febvre) remained rather dark, despite the many impulses which came especially from the Annales group. As early as 1941 Lucien Febvre had drawn up a list of desiderata, ranging from a history of hatred, of anxiety, of cruelty to a history of odour, sight and hearing. The list was recently acclaimed and prolonged by Duby (Duby/Lardreau, 1982) and it seems that by now we got nearly all of the desiderata fulfilled. The last two decades have seen a veritable boom of historical studies on particular aspects of subjectivity, the titles extending from the history of perception, desire and collective fears, to the history of childhood, youth and death (Ariès 1960, 1978; Corbin 1982; Delumeau 1978; Lowe 1982; Schivelbusch 1977). And yet, these fascinating studies do not seem to offer an easy may of relating the fragmented aspects of subjectivity to some theory of subjectivity in history. Not only do they work with different concepts of history and often with eclecticist collections of concepts borrowed from various psychologies. More importantly, there has been no systematic tackling of the question of how the fragmented aspects of subjectivity relate to each other in the process of change.

To be sure, there is Elias' comprehensive study on the sociogenesis and psychogenesis of civilisation (Elias 1939). But apart from his problematic notion of a rather linear history, his conceptualisation of the link between psychogenesis and sociogenesis in terms of 'conditioning', 'shaping' or 'social coinage' is hardly satisfying as it suggests a merely external superformation of some ahistorical psychic substance.

The main reason for the conceptual problems indicated above is, I think, that the interest in a historical theory of subjectivity touches on key concepts of the self-image of modern society which have remained controversial in philosophical discussion up to the present day, while they have been cut out of social science in the course of its separation from philosophy and its particularisation into different disciplines. Approaching the question of concepts and theories that may guide future research in the history of subjectivity, I would like, therefore, to discuss some broader theoretical problems concerning the project of a historical psychology.

I will depart from Horkheimer's essay on 'History and Psychology' (1932) which in my opinion gives a rather convincing formulation of the social relevance and rationale of a historical psychology, while the aimed-at amalgamation of historical materialism and psychoanalysis would seem to need some radical review. Trying to justify some junction of historical research on subjectivity and critical social theory, I will then outline three competing paradigms in the current debate on critical social theory and discuss some of their implications for the project of a historical psychology.

1. Horkheimer's essay on 'History and Psychology' was published in the first volume of the Zeitschrift für Sozialforschung. Taken together with his Editorial and his introductory remarks on 'Science and Crisis', the essay may be regarded as a key to the project aimed at by this journal. In the face of rising fascism a comprehensive theory of the social dynamics of the period was to be developed by way of a reintegration of empirical social research and theoretical reflection. In this context the Editorial puts special emphasis on the task of developing a social psychology suitable for historical research. The essay outlines this focus of the project more closely.

Claiming to assess the role of psychology "in the context of a theory of history able to meet the advanced standards of social

science", Horkheimer first justifies his option among the competing concepts of history, then formulates the task assigned to psychology, and finally indicates some reasons for his option for the psychoanalytic concept of subjectivity.

Four concepts of history are dismissed as inadequate:

- The liberalistic concept of history would not allow a conceptualisation of the relationship between history and psychology, being itself based on a psychologistic reduction of socio-historical phenomena;
- The concepts of history prevailing in philosophical discussion of the period would not allow for a reintegration of empirical historical research and a theory of history, the Neo-Kantian concept reducing itself to a methodological reflection of the historical sciences, the Heideggerian concept of an 'inner historicity' of 'Dasein' remaining confined to ontology;
- Dilthey's concept of the history of ideas presupposes the mental unity of a period and thus underrates the problem of social antagonisms. Moreover, his view of subjectivity equalises men with their views of themselves and thus lags behind the psychoanalytic insight into the problem of systematic selfdeception.
- These concepts being dismissed, there remains the Hegelian concept of history as the process of the self-constitution of humanity and its Marxian materialistic elaboration, which is read by Horkheimer in a rather economistic way. His option for this concept is justified in terms of the dialectic of material reproduction and the development of human consciousness and of the diagnosis of a disproportion between the increasing human capacities and the limits imposed upon them by the social structure. Taken as assumptions expressing modern experience, this diagnosis could guide attempts at a conceptualisation of the historicity of human capacities as well as empirical investigations.

For Horkheimer, a psychology based on historical materialism would no longer take "man as such" as its subject matter; rather, it would analyse historically grown subjective capacities in terms of their contribution to the maintenance or change of the social forms of life. The task thus assigned to psychology is to differentiate, in any given period, between "the potential of subjective capacities, the forces underlying men's manual and mental performances, the psychic factors enriching the social and individual life process",

and "the relatively static dispositions of individuals and groups... which are determined by the total social structure" (Horkheimer 1932, p. 133; my underlinings).

He regarded psychoanalysis as a viable basis for working on this task and though he referred only to Freud it may be assumed that he also had in mind Fromm's proposals for a psychoanalytic social psychology published in the same volume. Since the later Frankfurt School contributions to the authoritarian personality are widely known, I do not want here to enter into a discussion of the aimed at amalgamation of historical materialism and psychoanalysis. Rather, I will focus on the more general implications and consequences of the task suggested by Horkheimer.

With such a task, a historical psychology would be conceivable only in the context of critical social theory, and the objection could be raised that a junction with critical social theory would unnecessarily overburden the project of a historical psychology. If this objection is taken seriously, the idea of differentiating between those aspects of subjectivity which may enrich the individual and social life process, and those aspects which only serve to reproduce a given level of development, would have to be given up. Such a differentiation cannot be made by describing and interpreting subjectivity as one "finds" it. It is a theoretical construct which inevitably depends on the wider construct of a differentiation between reproductive and enriching/transforming tendencies in society.

If, on the other hand, the function of historical psychology and critical social theory is accepted, the project of a critical social theory would also have to be reconsidered. Not only has the paradigm of production allegedly become obsolete, there have also emerged competing paradigms which claim to be more adequate for an updated critical theory. I will briefly outline the paradigms of production, communication and power, and discuss their implications with regard to the project of a historical psychology.

2. Now that the end of a society in which work has a predominant place is in sight, social research pays decreasing attention to 'working society' and the paradigm of production seems to lose its substantial meaning for a critical social theory. As will be shown, even Habermas, outstanding heir of the Frankfurt School, makes short

work with it. However, there are also serious proposals for its radicalisation.

Marx used the concept of production to denote the unity of instrumental labour and the reproduction of the social relations of production. The paradigmatic character of the concept refers to (a) production constituting the material and social life world of men as well as their views of this life world, (b) the organization of production implying unequal access to the means of subsistence and hence social conflicts, (c) a culture depending on the productive forms of the appropriation of nature, and (d) men changing their own nature along with their transformation of natural resources into human products.

The paradigm of production gives a new meaning to the Hegelian concepts of intersubjectivity and subjectivity. Intersubjectivity is for Marx the sum total of the social objectivations of previous human activities. For each generation of humans this objectivity constitutes the conditions of their activities. But the social objectivations can become lasting 'social relations' only insofar as they are retransformed by individuals into subjective capacities and reproductive activities. Along with the notion of intersubjectivity the notion of 'subject' changes its meaning: "Autonomy and creativity - the constituents of the notion of subject - are no longer thought of as metaphysical characteristics, but as historical possibilities. In any given period, they must be redefined, in relation to the existing 'limits of nature' on the one hand, and to the socially created needs and interests of the individuals involved" (Markus 1980, p. 19).

In the Marxist tradition, the paradigm of production has been decomposed into notions of 'instrumental labour' and vague notions of 'praxis', thus dissociating the dialectic of subjectivity and intersubjectivity into theories of history as a 'process without a subject' (Althusser) and theories of a 'collective subject' (e.g. Lukacs, Gramsci).

As George Markus has convincingly shown, these dissociations are based in ambiguities of Marx' original project, which would thus need a radical reformulation (Markus 1980). Marx explains and interprets social reality in terms of an analytic differentiation between the objectivations of men's relations to nature on the one hand, and the materialisations of social relations on the other. The dichotomous categories of the paradigm of production - material content and

social form, concrete and abstract labour, productive forces and relations of production - serve to define the realm of radical critique and possible social change. For Markus, the above analytical differentiation can only be made with reference to empirical radical needs, unless one takes recourse to some illusionary metahistorical standpoint. A radical updating of the paradigm of production would thus have to depart from the constitutive role of empirical radical needs for a critical social theory. As radical needs cannot be proved, critical theory cannot construct the 'subject' of radical change, it can only evoke it by suggesting a better alternative to the existing forms of social organisation and by stimulating further discussion of better social models. Moreover, the radical historicism of critical social theory would have to be applied to critical theory itself: it cannot claim to be THE critical theory as long as mankind consists of a plurality of peoples with different radical needs.

It seems to me that a reformulation of the paradigm of production along these lines could well meet with the task Horkheimer assigned to a historical psychology. Not only would the radical historisation of subjectivity urge an anlysis of the modes of cognition, feeling and action in terms of the technological and social conditions which brought them about.
The emphasis on radical needs would also draw attention to the manifold expressions of discontent and reluctance, which would have to be analysed and interpreted in terms of the desires and claims they might conceal, as well as to the disproportions between cognitive and emotional capacities, which Günter Anders regards as the key to the pathology of human adaptation to the atomic threat (Anders, 1956).

3. Habermas' approach to critical theory departs from his reading of the paradigm of production as reduced to 'instrumental labour'. He thus feels urged to add a supplementary dimension of interaction/communication, until he finally drops the paradigm of production in favour of a paradigm of communication. The motivation for doing so is that he aims at a critical social theory which would be able to justify its own normative yardstick of critique.

For Habermas, communication is the central activity in social evolution, and the rationality requirements which are presupposed by communication are the true measure and goal of human emancipation.

Communication is placed central because the productive cooperative activities need to be coordinated through communicative action. More exactly, Habermas assumes that the participants in a cooperative endeavour share a common life world (beliefs about the physical and social world, views about who is to do what and how) which may be disturbed by disagreements or misunderstandings and then needs communicative action to avoid the break-up of cooperation. So far, of course, there is no real difference to the paradigm of production which would never exclude the use of language and communication.

Habermas' stronger claim is that communication and only communication presupposes requirements of rationality which may reveal the fundamental norms of human existence. The paradigm of communication is based on the assuption that the telos of understanding one another is inherent in human language. In every speech act the actor makes a threefold validity claim - for the objectivity of facts, the legitimation of norms, and the sincerity of self-expression - which could be challenged by the addressee and then would have to be justified by the speaker. As everybody communicates, everybody is tacitly committed to fulfil these validity claims. With the threefold validity claim, communicative rationality proves to be a comprehensive concept of rationality, as it refers to the world of facts, the world of norms and the subjective world. In contrast to this comprehensive concept, the concept of instrumental rationality and the vague notions of 'the other' of instrumental rationality may be regarded as mere derivatives.

What does this have to do with social evolution: "If we begin from the fact that mankind preserves itself by means of the coordinated action of its members and that this coordination must be established by communication directed at agreement - then the reproduction of mankind demands also the fulfilling of the rationality conditions inherent in communicative action" (1981, I, p. 532). In modern Western society, communicative rationality has been released as well as distorted. The core of the paradox of rationalisation may be seen in the fact that the self-regulating systems of money and power which serve to manage the complexity of modern society tend to impose their standards of regulation onto a life world which depends on communicative ways of reaching agreements.

So far, Habermas has not elaborated the notions of system and life world which constitute his dual concept of society. As long as

these notions remain vague and the function of material reproduction and the symbolic reproduction of the life world a mere assertion, it would seem difficult to evaluate this model of a critical social theory. Apart from the claim to justify the fundamental norms of social critique, his critical social theory does not claim to offer any substitutes for the interrelated Marxian concepts of reasonable practice, diagnosis of crisis and solution of crisis (Habermas 1985). In practical terms, he even leaves it unclear whether the life world is to be defended only against further colonising imperatives of systemic regulation or whether communicative rationality could lay claims on capitalist economy and bureaucracy.

For the project of a historical psychology, Habermas approach to critical social theory would seem to offer two unrelated and equally problematic routes. One route may be seen in his comparison of the coming of age of humanity with Piaget's and Kohlbergs's accounts of the development of rationality and moral judgements in human individuals. Such parallelism of individual and generic development would still radicalise the Diltheyean problematic of an assumed mental unity of whole periods. The other route would depart from socialisation as one of the aspects of the symbolic reproduction of the life world. In this context Habermas tends to use neopsychoanalytic concepts of identity, which, I think, would reduce the historisation of subjectivity to some flat sociologism. If there is a third and may be more implicitly suggested route, I must admit that I haven't yet been able to discover it.

4. While the paradigm of production and the paradigm of communication may be viewed as efforts to radicalise and thus preserve the enlightenment notions of emancipation, the radical critique of the enlightenment in the tradition of Nietzsche dismisses altogether the notions of emancipation, self-determination and rationality in history.

Foucault's paradigm of power takes social struggle as the central activity in social evolution. Departing from a critique of the weaknesses of traditional and Marxist political theory, he defines power negatively as something one does not possess or lose or gain. Rather, power is conceived in terms of a strategic model of everchanging (results of the) constellations of social forces, the model remaining somewhat ambiguous as it sometimes refers to the

constellations themselves, sometimes to the results. Strategies of power may, of course, be based on knowledge. But Foucault simply equalises power and knowledge, and making the notion of truth dependent on power he gains Power as an Absolute.
Both concepts of power are systematically confounded, so that the empirical analysis of power strategies is always at the same time an account of the anonymous and timeless works of Power.

Foucault's social theory is conceived in terms of a dichotomy of power strategies and subjected bodies. In 'Discipline and Punish' he analyses in detail the power strategies which in modern society make the bodies productive and docile, and the arrangements of a power-knowledge over individuals as provided by the human sciences. There seems to be no escape from the networks of power. Where, then, does the emphasis on subversion come in? Who would ever be able to undermine what?

Genealogical historiography is to unmask the discoursive practices in which the will to power and the will to knowledge converge. As an 'antiscience' it is directed at the undermining of the pseudo-scientific status of the humanities. But what about the genealogy of genealogical historiography?

These brief remarks must here suffice to indicate the aporetic consequences of the paradigm of power. With the option for this paradigm the history of subjectivity would have to be totally dissolved into an account of the disciplinary practices working on the bodies:

> "It would be wrong to say that the soul is an illusion, or an ideological effect. On the contrary, it exists, is has a reality, it is produced permanently around, on, within the body by the functioning of a power that is excercised on those punished... On this reality-reference, various concepts have been constructed and domains of analysis carved out: psyche, subjectivity, personality, consciousness... But let there be no misunderstanding: it is not that a real man.... has been substitued for the soul, the illusion of the theologians. The man described fur us, whom we are invited to free, is already himself the effect of a subjection much more profound than himself. A'soul'inhabits him and brings him to existence, which is itself a factor in the mastery that power exercises over the

body. The soul is the effect and instrument of a political anatomy; the soul is the prison of the body" (Foucault 1977, pp. 29-30).

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PSYCHOLOGY AND TECHNIQUE

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1. The future of psychology

The main thesis of my contribution is, that the creativity paradigm will be the first really psychological paradigm in psychology. This thesis is, as it were, partly a historical and partly a program thesis. It is historical, because it can be understood only if we take into consideration its historical antecedents, but at the same time it is a program thesis because it concerns the future of psychology. Here we are interested first of all in the history of psychological ideas. That is why we shall turn to the problem of psychology and technique and try to explain why the technical approach which is predominant even in contemporary psychology could not grasp the essence of psychological phenomena. To accept this introductory sentences we must suppose at least in the beginning that technical and creativity paradigms are two contradictory ones. I hope my contribution will be able to outline the nature and the field of the proper application of the abovementioned two ways of thinking. The discussion will be not so easy, we shall have to go into some philosophical problems as the roots of both mechanical and creativity paradigms can be find in philosophy.

2. The problem of psychology and technique

The problem of psychology and technique containes in general at least two sub-problems. One of these concerns the place and the role of concrete techniques in the psychological investigation and practice. The second and more important for us question concerns the impact of the general technical paradigm on psychological thinking. In this brief study we shall try to outline the second problem. Let us see first the technical and heuristical (or creativity) principles separately:

3. Technical and heuristical principles

a. First of all what is technique? J. Ellul cited H.D. Lasswell's definition in his book: the technique is "the ensemble of practices by which one uses available resources in order to achieve certain valued ends".[1] We can cite a great number of definitions like this.

But if we use the content analysis of the texts about techniques we shall find an entirely different and much narrower notion of technique. According to this notion the technique is simply routine performed either by using tools or machines or without them.[2] And this is the real meaning of the word "technique".

As J. Ellul wrote the image of technique had been originated from machines by generalization and then technique itself originated machines everywhere it penetrated.[3] It is also known that the spirit of sciences came from technique as well.[4] In a recent work by L.N. Cooper we can find the following sentence: "Science has always constructed little machines or mechanisms "behind immediate experience" that connect various phenomena".[5] Notice, from the point of view of such epistemology any entity is a fact producing machine. In this picture the prototype of creative persons is the engineer, the hero of the past century, because he acts as the nature acts creating machines, which function like the nature functions.

b. On the other hand we have every reason to suspect that a big realm of reality has a different, the so called heuristical structure. As if we would take a machine together with an engineer that works on the former. The general structure of this constellation is that of the problems, main constituents of which are the aims and possibilities. Here the determining power comes not from laws or rules but from the lack of something that can be called solution after finding it. This is the first, and presumably the main difference between the two principles in question.

Now the question arises: what is the extension of these two opposite principles in the real world. Here we can point at two theories that have been worked out in the history of philosophical reasoning.

The traditional view was put forward by Descartes. According to this view the technical principle is appropriate only for the material world. It means that the border between the two abovementioned principles runs just within human beings, as Descartes himself stressed. From the beginning of this century most of the programs for the scientific psychology aimed at wiping this border and applying the technical principle in question to the psychic world as well. This movement is responsible for the technicism in psychology in the sense that most of psychologists tried and try to regard human personality as a kind of machine. There are numberless writings in

psychology which search after partial machines of perception, fantasy, intelligency and so on.

However I think that the Cartesian border is not set at the proper place. Let's set out from the Teilhard de Chardin's ideas. Teilhard de Chardin extended the evolutionary model far beyond the biological level to the level of dead nature on the one hand and to the level of societies on the other one as well.[6] One of the most prominent features of this model is the distinction between entities which are in deadlock and entities that do have wide perspectives. So in Chardin's ideas entities in deadlock and entities with wide perspectives belong not only to the biological level but both to dead nature and societies as well. For example crystal is an entity trapped in deadlock because it does not give rise to a new and more advanced entity. It is because the crystal represents a too rigid system. Likewise there are societies from which no new way takes its source.

In this model the border between technical and heuristical principles does not go "horizontally" as in the Cartesian system of thinking: here it goes "vertically". Because we are able to understand by the unchanged repeating machinery laws, that is by the technical principle only entities which having repeated itself can be found just in deadlock. On the other hand whatever entity which is in the process of development could be understood only by heuristical principles. It means that some societies are explainable in the manner of scientism: societies that only repeat themselves without any development, and some natural entities are explainable by heuristical principles. In this respect the Bohrian principle of complementarity in the physical world is of outstanding importance,[7] though we have not read yet writings on such interpretation of this principle. Here we can say that paradoxically a society trapped in deadlock is nearer to natural entities which also can be found in deadlock, than to a society, which is in the process of development.

This last picture of reality assigns the role of the traditional science and the creativity paradigm in the studying of phenomena. This will be our next subject - matter.

4. Science and the evolutionary deadlock

It follows from the above train of thought that our traditional scientific thinking is able to describe adequately only things which

are trapped and is able to construct things for the evolutionary trap.

I think we must evaluate the idea of the so called "scientific society" exactly from this angle. We have no room here to go into details. We only mention things like bureaucracy which is the manifestation of rationality in Weber's writings,[8] and - as we know - one of the main obstacles of whatever development.

Or turning from societies and organizations to the "details" of human activities, the so called scientific organization of physical or intellectual work led to extremely routine structures of the formers. Concerning physical work this was still depicted by Ch. Chaplin. As for intellectual work, contemporary science - for example - is overorganized on the basis of its method. The present-day scientific method seems to be a general obligatory technology of whatever kind of scientific problem-solving. There is no incomprehensible miracle in the attack "against method" by Th. Kuhn[9] and Feyerabend.[10] In the arts a number of new artistic methods remind us of a serial production system especially in painting or music. See the organization of Vasarelly's workshop and those of his followers. Although it is an open question whether this kind of art has its perspectives or not. We would get into great trouble if this question had to be solved by democratic voting.

Briefly speaking the generalization of the methods of traditional science onto entities which were and are in development is not only a great mistake but a dangerous step from the point of view of the same development as well. Now we shall continue our discussion not with the general heuristical model which is applicable for developing entities but with the narrower problem of psychology. What was and is the situation in psychology in this respect?

5. Psychology and the technical and creativity paradigm

Well let's turn to the problems of psychology. Unfortunately we became the victims of mechanical thinking in this field, too. This state of affairs appeared not only in the history of our psychological reasoning but in our everyday life as well.

In everyday life a great many people identified themselves with some kind of machines. Here I would like to mention only one consequence of this identification; namely: having identified themselves with some kind of machines these people regard their life problems -

which as we know form the inevitable essence of human reality - as mere anomaly. That is why they strive to eliminate their problems as the signs of illness sometimes by drugs. If it is true we can find a good and positive correlation between the technical attitudes of personality and his or her inclination to narcotics. Empirical investigation must control this hypothesis. By the way the use of narcotics can be regarded as a way of the pseudo catharsis which is contradictory to the real catharsis that appears only after the real problem-solving by personality.

And now about psychology. Well, we can notice that the large part of the so called scientific psychology was worked out in its history as the mere ideology of this person - machine. That is why one must turn to philosophy or belles-lettres if one intends to learn something really important about human beings. On the other hand the abovementioned hard psychology, that is the "technical-psychology" has given rise to its opposite in the realm of psychology, too. This latter can be called soft psychology. This kind of psychology has dealt with topics which were left aside by the hard, "technical" psychology. These topics are as follows: emotions, highly individual data, value aspects, intraspection, and so on. Now the hard and soft psychology together can cover the psychological subject-matter. The cost of this "double psychology" is the tearing to pieces the unity of psychological facts. A good example of the soft psychology is the Rogers' person centered approach. Though in his writings Carl Rogers stresses the importance of total personality in interindividual communication[11], for example, the writer of this study was present at a conference on Carl Rogers' theory where a sharp intolerance could be observed towards participants who expressed not their emotional feelings but rather intellectual ideas.[12] This intolerance marks the actual gap between the hard and soft psychology.

What is the way out of this contradition of psychology?

One could say that this question is not a historical one. But we study history just because of the future problem-solving. Historia est magistra vitae. So let me finish my contribution trying to answer this question, although my answer will be only a hypothetical one. One of the leading Hungarian psychologists of the 60th L. Kardos said that for a long time there have not been existing a science like psychology: there were instead a number of kinds of psychology. I think this statement holds true of today as well. Till these days

psychology has not found its basic paradigm. Any approaches which have taken place in psychology have remained particular. It is because our science has been imitating sciences established for the things trapped in evolutionary deadlock while human personality belongs to the main current of evolutionary development. Just because of this creativity must be the basic paradigm for psychology. Empirical investigations discovered a long time ago that creativity characterise all human beings, but these investigations failed to acknowledge that creativity is not only a simple feature of the personality but it is the essence of the latter as well. I think this is quite understandable, because no empirical investigation is able to qualify such discovery theoretically. Coming from the above theoretical considerations we can establish, that creativity is not only one of the ordinary psychological themes, but it is the notion about the antimechanical and antitechnical essence of any psyche[13]. That is, creativity is not only a partial psychological problem, but it is an alternative approach to whichever problem of psychology. As I. Abinun wrote citing L. Hudson: "This odd word - creativity - is now part of psychological jargon, and covers everything from the answers to a particular kind of psychological test, to forming good relationship with one's wife. "Creativity in other words, applies to all those qualities of which psychologists approve" says Hudson. Indeed, one who looks closely at the subject of creativity will be amazed at its extent and range and may well ask: Is there anything which is not creative? Is there any subject sphere, matter, person, to which the term creativity does not apply?"[14] I think that creativity will be the first psychological paradigm which is able to synthetize different psychologies into one psychology. And here I must finish my contribution, because the further discussion of this topic belongs to the history of the future.

Notes

1. J. Ellul: The Technological Society. London: Johathan Cape 1965, p. 18.
2. I. Magyari-Beck: Lehetséges-e alkotási technika? (Does creative technics exist?). In: Alkotáselméleti (kreatológiai) tanulmányok. Budapest: Akadémiai K., 1982.
3. J. Ellul: The Technological Society.
4. P. Rossi: A filozófusok és a gépek (I Filosofi e le Macchine). Budapest: Kossuth K., 1975.
5. L.N. Cooper: Source and Limits of Human Intellect. J. Leonardo, 1984. 1. p. 43.

6. Teilhard de Chardin: Az embri jelenség (Le Phénoméne Humain). Budapest: Gondolat K., 1973.
7. T. Toró: Kvantumfizika, Müvészet, Filozófia (Quantum Physics, Art, Philosophy). Bukarest: Kriterion K., 1982.
8. M. Weber: Gazdaság és társadalom (Wirtschaft und Gesellschaft). Budapest: KJK, 1967.
9. Th. Kuhn: The Structure of Scientific Revolutions. Chicago: University of Chicago, 1970.
10. P.K. Feyerabend: Against Method. Verso Edition, 1978.
11. In: "Person to Person". Background material to the Cross-Cultural Residential Workshop, Szeged. Organized by the Center for Cross-Cultural Communication and the Hungarian Psychological Association. Szeged, Hungary, Juli 1-7, 1984.
12. Cross-Cultural Residential Workshop, Szeged. Organized by the Center for Cross-Cultural Communication and the Hungarian Psychological Association. Szeged, Hungary, July 1-7, 1984.
13. I think it can be proved that animals also are "opened" just at their psyche.
14. J. Abinun: Creativity and Education: Some Critical Remarks. J. Leonardo 1984. 1. p. 36.

FROM NORMAL TO ABNORMAL DISCOURSE
A SOCIOLOGICAL INTERPRETATION OF METHODOLOGICAL CONTROVERSIES IN WEIMAR-SOCIOLOGY

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Summary
Once we stress the collective character of science, it is easy to see that one of the main problems a scientific community is confronted with, is how to settle differences of interpretation in a 'rational' way. The function of methodological reflections is to deal with these kinds of problems of co-ordination. In the paper the solutions that Max Weber and Karl Mannheim gave of those problems are compared. It is argued that, as a result of several political developments in Germany, in the late twenties Karl Mannheim could no longer solve problems of co-ordination on the methodological level, as Weber had done. For him the problem of 'objectivity' became a hermeneutical problem. In the paper some consequences of this shift are pointed out.

I.

Karl Mannheim's notion of a socially unattached intelligentsia has never been popular among sociologists. In the context of Mannheim's sociology of knowledge, as many critics have argued, the idea of socially unattached or free-floating intellectuals has only an ad hoc function, viz. to prevent total relativism and nihilism. (1) In this paper I want to give a more constructive interpretation of Mannheim's thoughts on this point. Moreover I will argue that Mannheim did not simply fall back into a position which just before him Max Weber and tried to overcome. Most critics who held this opinion defended at the same time a more or less positivistic view of science. But as soon as we drop this view, it becomes clear that Mannheim faced the same problems as Weber did, but in a different situation. My argument runs as follows.

Science is a collective activity. The intention of individual scientist to produce 'true' knowledge does not guarantee the 'objectivity' of their knowledge. The possibility of intersubjective

justification within a scientific community is relevant, but science is not only a collective endeavour, it is also an activity of interpretation. As Richard Rorty has argued, the notion of the mind as a mirror is a misleading metaphor. Knowledge is never a reflection of 'reality'. This means, that we have to give up the positivistic idea of scientific knowledge as accuracy of representation.

If we stress in this way the social and interpretative character of science, it is easy to see that one of the main problems confronting a scientific community, is how to settle differences of interpretation in a 'rational' way. If 'objectivity' is not an illusion, it should be possible to reconcile the different interpretations, which scientists give of -pieces- of reality. One function of methodological reflections is to deal with this problem of co-ordination.

This view of the function of methodological reflection might be used to illuminate why Max Weber was so intensively engaged in methodology. Weber saw himself confronted with two rival schools in economic thought: the younger historical school (Schmoller) and the theoretical school (Menger). In his well known essay "'Objectivity' in social science and social policy", Weber tried to bridge the gulf between the two schools, or more precisely: Weber thought that it could be possible to reconcile the interpretations of both schools on the basis of a new methodological framework. In order to salvage the idea of 'objectivity' Weber sought methodological rules, which could constitute a normal discourse in socio-economical science. (2)

While Weber could still hope for a normal discourse, several developments in Weimar-sociology undermined this expectation. In the late twenties Karl Mannheim saw himself confronted by a scientific community, which not only lacked a theoretical consensus, but which was also marked by an absense of any methodological consensus. In this situation of abnormal discourse, i.e. in a situation in which discussion proceeded without an agreed-upon set of rules about what counts as a relevant scientific contribution (3), the problem of co-ordination of interpretations could no longer be solved on the methodological level. In a word, the problems of 'objectivity' became a hermeneutic problem. Mannheim's theory of 'relatively free-floating intellectuals' can be seen as a solution of exactly this problem.

II.

When the problems of co-ordination becomes more complex, that is to say, when it changes from a methodological into a hermeneutic problem, other demands will be made upon those who have to accomplish that co-ordination. In entering the scientific community Weber and Mannheim thus ask for different professional skills. In Weber's case a methodological schooling is sufficient, for Mannheim an intellectual education is necessary as well, because Mannheim sees that what Weber assumed, has still to be achieved: the formation of one sociological community which has a homogeneous intellectual attitude.

One of the relevant changes that took place was the appearance of Marxism on the academic scene after World War I. During the Wilhelminian era members of the Social Democratic Party were excluded from university-positions and in the early days of the German Sociological Society - founded in 1910 - they played no prominent role. But after their entering the academic community it became more difficult to realise Weber's ideal of - at least - methodological consensus. (4)

The German Sociological Conventions held between 1910 and 1930 can illustrate this shift. According to Käsler, the first two conventions, held in 1910 and 1912, had a different character than those held between 1920 and 1930:

> The first two conventions were dominated unequivocally by the sociologists whose spokesmen were Weber and, to a lesser extent, Tönies. With the third convention, the representatives of 'Beziehungslehre' advanced with von Wiese as their social-intellectual leader; similary, Marxist sociology advanced under the leadership of Max Adler. (...) German sociology at the end of the fourth convention (1924) appeared but little changed in comparison with the third (1922). The central issue was still the controversy, increasingly sharp in tone, over 'Beziehungslehre' vs. 'Marxist sociology'. (5)

Naturally there were differences in opinion between Weber and Tönnies, but the point is that they did not stress them within the Sociological Society. After the war, however, the intention to reach consensus disappeared.

Simultaneously the difference between scientific and non-scientific knowledge became less clear than it was for Tönnies' and

Weber's generation. As soon as a 'normal discourse' changed into an 'abnormal discourse', the distinction between value-free knowledge and ideological opinion could no longer be taken for granted. The principle of value-neutrality functions solely within a coherent sociological community as a threshold against political and ideological influences.

In this context it becomes understandable why a paragraph in the regulation of the German Sociological Society about value-freedom, which was included in its 1910 charter, was deleted on the third convention in 1922. Only on the basis of a social condition, viz. the homogeneity of the scientific community, can this methodological rule be effective. (6)

During the twenties within the German Sociological Society the value-freedom problem shifted to the background and in its place the relation between theory and practice became a central problem. In this new dispute the question of whether sociology should be a social technology, as von Wiese advocated, or whether it has a critical task, as Adler argued, became the principal issue. In such a debate the principle of value-neutrality could no longer function as a means to reconcile the different interpretations. Maybe von Wiese realised this when he noted in a retrospective view on the third and fourth conventions, that happily "the yoke of the value-judgement police is thrown off." (7) When the 'Weltanschauungen' of the participants are completely different, no methodological rule can streamline the discussion.

The increased co-ordination problems within the sociological community have consequences for the self-image of this community. So the image of the sociologists as scientists is challenged when their scientific community, elevating itself on the basis of methodological schooling above culture and politics, appears to be an ideologically divided community within culture. (8) This shift expressed itself as a feeling of scientific crisis that many sociologists experienced during the twenties. It is possible to place the differences between Weber and Mannheim in this context.

III.

To solve the growing problem of co-ordination, a 'free-floating' methodology is insufficient for Mannheim: he needs a 'free-floating' intellect. Together with this change, the sociologists is given a

different place in society. He (male or female) is no longer the deliverer of value-free knowledge outside the pluriform culture, but becomes a scientist standing between ideological and political conflicts.

In this way it becomes clear that methodological points of view are closely connected with ideas about the social function of sociology. Weber sets value-polytheism against the idea of a single scientific sociology. It is the task of the sociologist to map out the social landscape, but he cannot function as a political guide. He is free to comment on the means, but should refrain from formulating goals. In "Science as a Vocation" Weber defends this difference on the basis of cultural-philosophical considarations. As a scientist, the sociologist can only supply 'technical' knowledge; as an intellectual, he can only promote the freedom of the individual to choose his own goals. However, to give the sociologist this role presupposes that it is possible to create a sociological community above culture and politics, i.e. a community in which research can take place from different culturally value-attached points of view (Wertbeziehungen), whilst nevertheless connected by one set of methodological rules.

Mannheim did not accept the gulf between theory and practice which Weber thus induced. In his startling speech "The meaning of competition in social thought", held at the sixth sociological convention in 1928, he points out that even the methodological rule of value-freedom has an ideological background. As a consequence, methodological rules can no longer be seen as the only means to reach 'objectivity'. A special group, the "freischwebende Intelligenz", is needed which has the hermeneutic competence to compare the different 'styles of thought' (Denkstile) with each other. In an essay Mannheim wrote in the early thirties, we read that the intellectual, by virtue of his training,

> is <u>equipped</u> to envisage the problems of this time in more than a single perspective, although from case to case he may act as a partisan and align himself with a class. His acquired equipment makes him potentially more labile than others. He can more easily change his point of view and he is less rigidly committed to one side of the contest, for he is capable of experiencing concomitantly several conflicting approaches to the same thing. (9)

Mannheim's argument is not, as some critics have stated, that to be an intellectual is a sufficient condition for overcoming the limitations of a given social location, but that the exercise of intellectual recourses is a necessary condition for making the attempt to do

so. According to Simonds, Mannheim's faith in the intellectuals "is a faith in the powers of the intellect to overcome the limitations of this or that personal experience as a ground of knowledge, to expand the self by engaging in authentic communication with others, to aspire to a more comprehensive view of our shared human condition by virtue of the communicative ability to gain access to context of thought other than the one into we are born."(10)

To get the communication between different styles of thought going, is also the -often unnoticed- motive behind Mannheim's sociology of knowledge. Mannheim was not interested in causal relations between thought and social positions or classes. The claim be made was that we do not fully understand certain ideas if we leave their social genesis unconsidered. In the beginning of his book "Ideologie und Utopie" (1929) Mannheim made his intentions quite clear. In the original German version we read:

> Es ist die Hauptthese der Wissenssoziologie, dass es Denkweisen gibt, die solange nicht adäquat verstanden werden können, als ihr gesellschaftlichter Ursprung im Dunkeln bleibt. (11).

So for Mannheim the sociology of science is in the first place a hermeneutical instrument. It helps to get a better understanding of different styles of thought. If we want to reconcile interpretations of -pieces of- reality which are formulated within divergent thought-styles, such an understanding is a necessary condition.

While Mannheim could not appeal to the methodological consensus which Weber hoped for, he is able to credit especially the sociologist of knowledge with a practical function in solving cultural and political conflicts. That is to say, Mannheim gives the sociologists of knowledge a cultural task that is unwanted and superfluous from Weber's point of view. As an intellectual he must be a guide in the disordered social landscape, because for Mannheim the lack of internal cohesion within the academic community can be traced back to a lack of cultural cohesion outside that community. Therefore Mannheim sees the persuit of cultural syntheses as a scientific task.

Both Weber and Mannheim allow sociologists to participate in public debates. But according to Weber they have no special role within these kinds of debates. Weber is able to dismiss such a role, because in his opinion a collision between 'Weltanschauungen' in the public domain does not necessarily influence the proces of communication in the scientific domain. On the other hand, while Mannheim does not accept the Weberian gulf between internal and external debates,

he attributes especially to the sociologists of knowledge the task to promote communication between styles of thought within as well as outside the scientific community.

IV.

When we compare not only Mannheim with Weber, but also Weber with Rickert, we can trace an increased tension between what can be called 'subjectivation' and 'objectivation'. As a Neo-Kantian, Rickert pointed out that scientific knowledge is never an image of reality, but always a value-attached framing. This 'subjectivation' did not undermine the ideal of 'objectivity', because according to Rickert scientific research is guided by some objective -formal- values. Weber went one step further. He gave the Neo-Kantian concept of 'Wertbeziehung' a subjective interpretation. This subjectivation of values did not undermine the ideal of 'objectivity', because in Weber's case a set of methodological rules took over the function that Rickert ascribed to objective values. Mannheim went another step further. Methodological rules are not context-free, as Weber maintains, but are connected with styles of thought. According to Mannheim this 'subjectivation' implies no relativism, because 'relatively free-floating intellectuals' can take over the function that Weber ascribed to methodological rules.

This trend does not continue after World War II. Within the positivistic view of sociology dominant in that time, the tension between 'subjectivation' and 'objectivation' is strongly reduced. A normal discourse is re-established and at the same time sociologists re-assume a technical role. Already therefore it is unlikely that the trend from Rickert to Mannheim has a mere internal character. A look at the development of the early American sociology only corroborates this conjecture. The ideas of successively, Small, Thomas, Park and Ogburn reveal a trend just the opposite of that in Weimar-Germany. While in Germany before World War II the technical role of the sociologist is slowly exchanged for a more cultural role, in America it is the other way around.

However, the thesis that external factors have caused the development of Weimar-sociology is at once too strong and too vague. Frequently it remains unclear which developments are connected in what way with which factors. Let it therefore be clear that in this

case the explanandum is precisely the transformation of normal into abnormal discourse. But what can count as an explanans?

As soon as methodological reflections are seen not as individual endeavours of scientists to pinpoint the demarcation between scientific and non-scientific knowledge in an 'objective' way, but as a collective endeavour to co-ordinate the different scientific interpretations in an 'intersubjective' way -with demarcation as un unplanned social consequense-, then it becomes possible to see increasing co-ordination problems within the sociological community as a part of increasing co-ordination problems in the society as a whole. This claim will now be briefly elucidated.

After 1914 the ideals of bourgeois culture become more illusionary in Germany. Following the war, the Spartakus-revolt and the Kapp-putch, it seemed as if, together with the established Prussian order, all order had disappeared. In this situation a well functioning public debate should have been of essential importance for the new democratic order to be built up. Only on the basis of a healty public opinion could discussion proceed without violence. Then it is possible to co-ordinate dissenting ideological and political points of view in a peaceful way.

It was precisely the formation of such a public forum that stagnated. In a country where public consciousness could not develop for a long time, there were many who looked askance at the new constitution. Gay puts it as follows:

> When the democratic Weimar Constitution opened the door to real politics, the Germans stood at the door, gaping, like peasants bidden to a palace, hardly knowing how to conduct themselves. (12)

After the establishment of the Weimar-republic the chances to create a democratic public debate were few. These chances were further reduced during the twenties. What Musil has called a "babylonisches Narrenhaus", came into being, in which a dialogue between political and ideological groups was no longer possible. This experience, as Sloterdijk remarks,

> ..hat man heute unter einer Grabplatte mit der Aufschrift 'Pluralismus' beerdigt. Doch damals, als die Massen noch keineswegs darauf eingerichtet waren, alles gelten zu lassen oder vielmehr alles mit Gleichgültigkeit zu betrachten, war 'Pluralismus' noch etwas, was den Zeitgenossen weh tun musste (...) Längst hatten sich die Widersprüche noch nicht in blosse Verscheidenheiten entspannt, sondern wurden in voller Härte empfunden, und zugleich begann doch schon die gleichmacherische Tendenz, alles, was sich als Gegensätze gegenüberstand, in ein mannigfaltiges Einerlei zusammenzurühren. (13)

This ambivalence can be found back in Mannheim's work on the sociology of knowledge. On the one hand Mannheim argued that intellectuals can re-establish the public dialogue between conservatives, liberals, socialists and communists. On the other hand however, he thought that intellectuals could create a cultural synthesis, a "mannigfaltiges Einerlei", as Sloterdijk calls it. In this way Mannheim put public discussion aside in favour of an intellectual élite.(14)

To conclude, a generally accepted framework in which ideological and political conflicts are regulated could not be built in Germany between the two World Wars. Public consensus increasingly diminished. This meant that co-ordination problems in the society as a whole increased. The change from normal to abnormal discourse within the sociological community can be seen as a part of exactly this development. When there is no generally accepted framework on which basis attunement of interpretations is possible, there are also no generally accepted boarderlines between the scientific, the political and ideological domain. What is counted as a scientific contribution by one person, is counted as an ideological statement by an other. In such a situation of abnormal discourse the ideal of 'objectivity' is far away. Mannheim realised this. Behind his faith in intellectuals laid the purpose to identify and overcome the obstacles of intersubjective communication.(15)

Notes

1. See e.g. Von Schelting (1934), Merton (1945), Stuart Hughes (1961), Coser (1971).
2. 'Normal discourse' and its opposite 'abnormal discourse' are concepts which I borrow from Richard Rorty. The dinstinction between these two forms of discourse generalizes Kuhn's distinction between 'normal' and 'revolutionary' science, i.e. Rorty's distinction cuts across the distinction between science and nonscience. A 'normal discourse' presupposes no more than a common set of communication-rules and can therefore be found not only in science but also in politics and theology. For detail, see Rorty (1980, p. 315-33).
3. Rorty (1980, p. 320).
4. Weber of course was not opposed to Marxists entering academic life, but nevertheless he claimed universal validity for his methodological ideas. When he saw that Marxists within the German Sociological Society challenged the rule of value-freedom, Weber took the consequences: he left the Society.
5. Käsler (1983, p. 237 and p. 246).
6. In which way this homogeneity is reached is a second question here. It can be a result of rational consensus, but also -as it in most cases is- a result of ideological dominance.
7. Quoted in Käsler (1983, p. 240).

8. Compare De Vries and Harbers (1985).
9. Quoted in Simonds (1978, p. 131).
10. Simonds (1978, p. 131).
11. Mannheim (1929, p. 4), underlining by me.
12. Gay (1968, p. 72)
13. Sloterdijk (1983, vol 2, p. 880).
14. On this ambivalence in Mannheim's sociology of knowledge, see De Wilde (1984).
15. Simonds (1978, p. 114).

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PRESUPPOSITIONS IN CLARK L. HULL'S LEARNING THEORY: EVIDENCE FROM CURVE-FITTING AND OTHER RELATED SOURCES

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Summary

It is not our intention in this paper to join the anti-Hull league; instead, we undertake what can be termed a presuppositional analysis of a central part of his scientific programme. Specifically, we investigate Hull's curve-fitting procedures in the light of his prior commitment to models of exponential growth. We also discuss aspects of his rather complex philosophy of science in relation to these presuppositions. Where his adoption of empirically realizable postulates as part of his elaborate deductive systems implies an operation which started by assuming what was subsequently demonstrated (via curve-fitting) to be true.

Hull's presuppositions are investigated in a range of his curve-fitting studies. Since most of these curve-fits involved extensive interpolation and extrapolation, we consider the implications that such practices have for a presuppositional analysis. Examples of missing data and outliers are discussed in the same context

In our discussion of Hull's curve-fitting method, we note that presuppositions could influence particularly those stages which require the curve-fitter to choose specific mathematical functions to either linearise or represent the data. We conclude that, in general, Hull and his students ignored the more substantively neutral methods widely available to researchers of the period.

"And the deep lane insists on the direction
Into the village ".
East Coker, T.S. Eliot

The poverty of empiricism

It is an easy task to find unashamedly positivist and empiricist positions adopted by the learning theorists of the 1930s and 1940s. The centrality of the experiment, the fitting of empirical data by a range of mathematical models and the frequent use of the term "objective" to describe such methods and their results were all clear signs, at least on the surface, of the capture of significant areas of psychology by late nineteenth century physics. Although the most explicitly converted of these workers was the young B.F. Skinner (see Smith, 1984), the influence of hard-nosed empiricism is easy to detect in the writings of many other workers. In particular, the learning theorist Clark L. Hull exhibited all the verbal habits of the most committed of positivists.

This shows itself, for example, in his explicit espousal of both inductive and, even more prominently, deductive processes of theory development. Indeed, most text books credit him with the introduction of the hypothetico-deductive method into psychology. However, the uneasiness that we feel about Hull's curve-fitting work, at least to the extent that it provides an unproblematic illustration of a positivist programme, also extends to his rather contradictory philosophy of science, since he preached a rather peculiar inductivism and an even stranger deductivism.

Indeed, it is the rather ambiguous status of certain key elements in his philosophy that we feel lies behind his attempts at fitting elaborate mathematical models to data. Specifically, we argue that Hull's deductive systems, which included among their postulates many experimentally examinable or behaviourally expressible statements (for example, 1940, postulate 1; 1943, postulate 5; 1951, postulate 7; 1952, postulate 12), were not really deductive systems at all since, in the presence of confirming results, the argument from postulate (and derived theorem) to data has become too narrowly circular (see the parallel comment on Watson by Titchener, 1914). More importantly, however, such postulates allowed Hull to claim that they were both true (or at least assumed true) a priori and hence

must be true (or could be demonstrated as being true) empirically. In other words, the status of such experimentally expressible/testable postulates was apparently greater than ones arrived at purely empirically. The relatively unproblematic and even metaphysical form of the postulates in the more usual variety of deductive systems, such as Euclid's geometry and Newton's Principia (cited by Hull as partial models for his own work, 1940, pages 3-4; 1943 pages 6-7), contrasts markedly with the theoretically loaded and question-begging nature of Hull's behavioural axioms.

Our comments on Hull's deductive systems may be compared with the apparently inductive strand of his thinking. For example, his insistence that observation and theorizing progress in parallel (1940, page 1; 1943, pages 1 and 2), or that observation may outstrip theory (1952, page 353). Unfortunately, his inductivism did not crucially extend to an endorsement of inductively generated theories (1943, pages 2-5). Indeed, at one point he entertains the possibility of scientific development entirely independent of any empirical observations (1943, page 5).

One could, of course, conclude from our remarks that Hull had little knowledge of philosophy of science and leave it at that. Our thesis, however, is that Hull's manoeuvering was really designed to satisfy the contradictory pressures that inevitably arise when attempting to synthesize deductive and inductive approaches within the strongly empirical and non-metaphysical context of the learning theory of the 1930s and 1940s where positions had to be demonstrated experimentally or at the very least translated into an experimental statement. It follows from this, therefore, that what Hull did was essentially both to assume the truth a priori of an empirically realizable set of propositions and to arrange his experimental tests and their analysis and interpretation so as to support these presuppositions. The purpose of the empirical work was, therefore not to test either the postulates (or their consequences) but to demonstrate their essential truth. As George Miller pointed out in a historical commentary on Hull's work "Hull ... began his own theory by postulating what others thought he should have ended by proving." (1964, page 135).

Thus, as well as showing that in Hull's case such pre-existing notions can direct the experimental work of the scientist, this finding also provides evidence for the two major propositions of

post-positivist philosophies of science, viz. observations and theories cannot be separated and hence observations are theory-laden, and theories are undetermined by data, thus theories can be fixed so as to accommodate contradictory or negative evidence.

Curve-fitting, or the royal road to learning theory

Although Hull was probably the most enthusiastic curve-fitter amongst early learning theorists, there were many instances prior to 1939 of psychologists (and even non-psychologists) deriving and testing mathematical models for learning. The range of models included the hyperbolic (Thurstone, 1919, 1930; Gulliksen, 1934), the so-called monomolecular autocatalytic function, extensively used in studies of biochemical reactions (Robertson, 1908, 1923; Culler, 1928; Chaisson, 1930; Woodrow, 1940), and the conceptually related exponential growth function (Schükarew, 1907; Henry, 1911; Spielrein, 1925; Ettlinger, 1926; and Hull himself, 1940, 1943, 1950, 1952, etc.). Hull, therefore, operated within an intellectual climate where the comparison of data with a specific mathematical model, including that for exponential growth, was a recognized scientific procedure (see also Lewis, 1960).

We must confess to a problem here, however, in that Hull, at least in his published writings, never referred to this earlier psychological work. Instead, he repeatedly emphasized the value of exponential growth models in either biological research (1943, page 111), or in a variety of less well defined contexts (1940, pages 43-45). Hull, in other words, appeared to be impressed by a rather larger and more diffuse body of applications of exponential growth models than that found in psychology up to 1940. It is to this extent, therefore, that his views were the product of a pre-existing conceptual context.

Hull's later remarks (Gladstone et al., 1947) on the value of exponential growth functions are also worth including at this point. They imply that Hull was doing no more than articulating a preexisting (but unexpressed) collection of widely held beliefs about behaviour at asymptotic stimulus conditions. When discussing a range of fitting criteria (see our later remarks), Hull suggests that one of them should be the existence of a finite upper limit to simple learning:

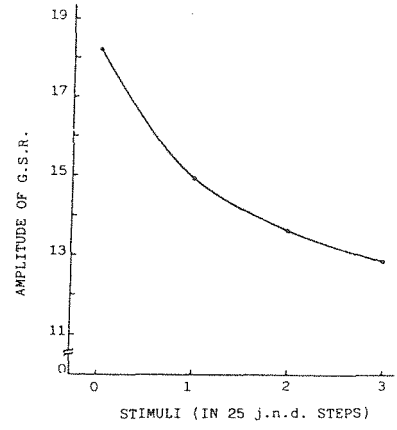
> While this is apparently in general harmony with most opinion in the field, as well as with available evidence,

> no one seems ever to have committed himself specifically on the subject. On the other hand, the criterion does have the support of the concept of physiological limit which is not known to permit infinite functions in any other fields. (1947, page 522).

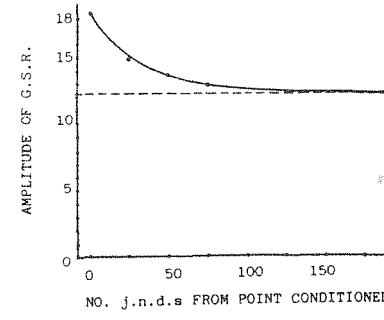
The earliest mention of exponential growth functions in Hull's published work is in 1939 in a paper on stimulus equivalence, although he had, in the preceding year, proposed the use of an exponential function to model goal gradients. It should be noted, however, that the latter function was not chosen on theoretical grounds, rather its selection was designed to overcome certain unfortunate mathematical properties of an earlier (1932) logarithmic formulation of goal gradients (1934, pages 159-160). In 1939, however, Hull justified his exponential function (also re-expressed as an exponential growth function on page 20) on theoretical grounds, saying that 'it must not be confused with any of the numerous curves of learning plotted directly from empirical data' (1939, page 16).

The data analysed in the 1939 paper came from a somewhat convoluted study by Hovland (1937) (Figure 1a). Hull's graphical version of this data, as distinct from Hovland's, reveals one of the major problems with his approach, specifically the excessive extrapolation of the function (Figure 1b). In fact, the equation is extended over more than double the range investigated by Hovland, thus making the result appear stronger and more general than is justified by the original data. (Note that extrapolation assumes a specific model of the situation to be true for both investigated and untested values of the independent variable.) Furthermore, there is no indication of the process whereby the parameters of the function were estimated. Again, like most of the later curve-fitting exercises, the mechanics of the process seems much less important than the result. It is worth pursuing Hull's treatment of Hovland's data a little further.

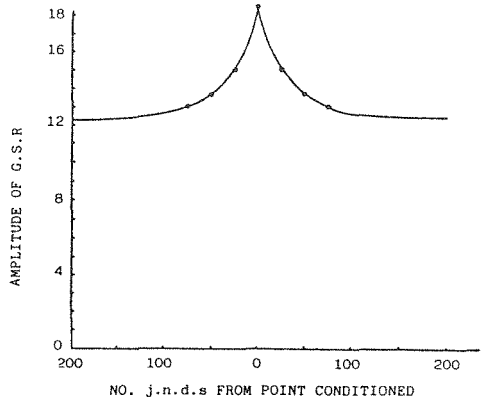
In his 1943 book, Hull recast his 1939 version of the Hovland graph into a symmetric back-to-back form (Figure 1c), thus implying a function that could be generalised not only to the right of Hovland's original data, but also to the left and below the standard stimulus. Whilst it is true (although impossible to infer from the plot) that Hovland looked at stimulus generalisation both above a low valued standard stimulus and below a high valued one, all he actually presented was a composite graph of points plotted in the positive quadrant only. In fact, unbundling the data and replotting separately



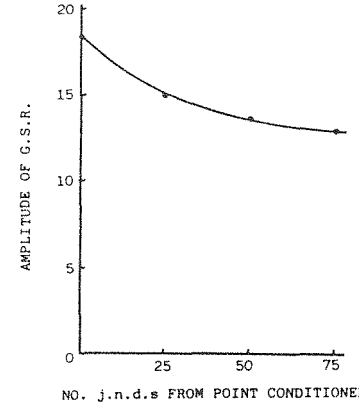
a. FROM HOVLAND (1937)



b. FROM HULL (1939)



c. FROM HULL (1943)



d. FROM HULL (1951)

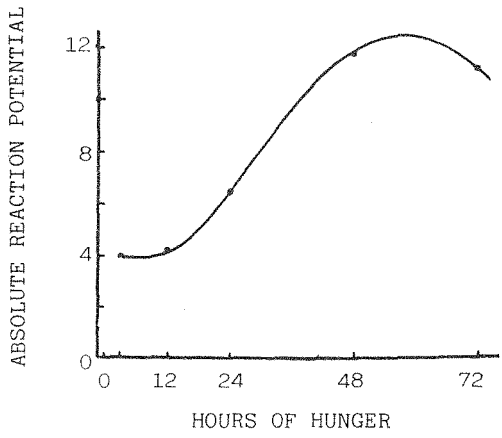
FIG. 1 HOVLAND'S STIMULUS GENERALISATION GRADIENT DATA AND ITS SUBSEQUENT TREATMENT BY HULL.

shows a marked difference between the generalisation gradients above and below the standard stimuli.

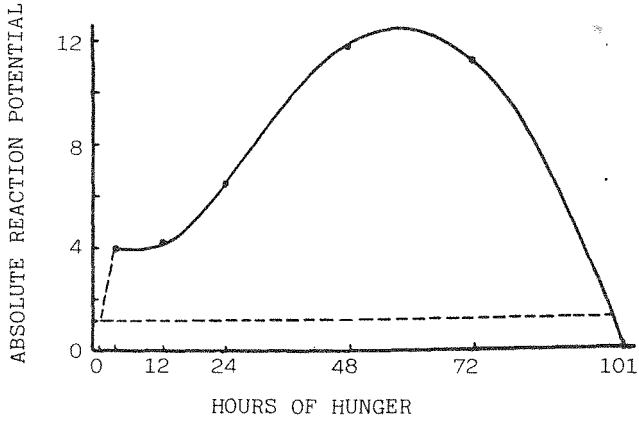
In a review of Hull's 1943 book, Ritchie (1944) pointed to the conflict between the back-to-back plot and Hovland's original. Consequently, in his 1951 book, Hull presented only Hovland's original data, although still fitted by the 1939 exponential function (Figure 1d). These successive changes, additions and deletions to the graphs show that Hull was driven by his commitment to the exponential function rather than the data itself. Moreover, some of these changes were due to the intervention of an outside critic, hence the direction taken by the work was determined by an ongoing, if indirectly and slowly pursued, debate about the interpretation and representation of the data. It is worth noting that in his 1943 book, Hull also undertook the same substantive extrapolation of Hovland's results for stimuli above and below a standard stimulus for extinction trials (page 264). Again, the symmetric curves are based on a single composite curve from Hovland (1937, page 143).

Hull's first major book, Mathematico-Deductive Theory of Rote Learning (1940), contains several curve-fitting examples, but again with no hint as to how the fitting was performed. In addition, one can observe difficulties with interpolation (often over two-thirds of the range) in the functions (pages 70 and 136), too few data points (pages 70 and 136), and a willingness to detect theoretically acceptable patterns in rather uniform or contradictory data (pages 157, 160 and 162). With interpolation of the kind practised by Hull, the final curve is overdetermined by the single data point. Such interpolation clearly requires a strong commitment to a specific mathematical model.

It was, however, in his second and third texts. Principles of Behavior (1943) and Essentials of Behavior (1951), that curve-fitting, as distinct from algebraic manipulation, became the major vehicle for theoretical development. All that we can do is to select examples of curves that appear to offer much stronger support for Hull's theoretical views than a less committed position might have accepted. All show the same excessive interpolation and extrapolation as was noted earlier, and the same tendencies to detect confirming results in unpromising data (see 1943, pages 106, 125 and 139; pages 45, 50 and 106).



a. FROM YAMAGUCHI (1951)



b. FROM HULL (1951)

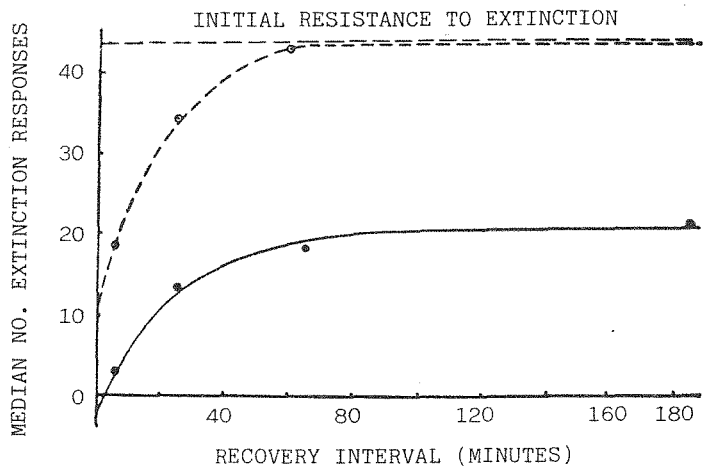
FIG. 2 YAMAGUCHI'S ABSOLUTE REACTION POTENTIAL AND HUNGER DATA. REWORKED BY HULL (1951).

One further comparison is worth a slightly longer treatment. This is between the curve generated by Hull's student Yamaguchi (1951, page 112) and Hull's version (1951, page 36). In Yamaguchi's study, a complex double exponential equation was fitted to data from an experiment on the effect of hours of hunger (Figure 2a). In his discussion of the curve, he points out that the empirical maximum of the function varies considerably (between 36 and 120 hours) over the three other studies that he cites. Hull's response to this work (Figure 2b) was to extrapolate Yamaguchi's curve to the zero value of the dependent variable (absolute reaction potential). This yielded a hunger score of 101 hours, some 19 hours earlier than the upper range of values cited by Yamaguchi. In other words, Hull's extrapolation discounted the contribution of earlier work. This clearly weakens the generalisability of the result and undermines the validity of the extrapolation, except, of course, for someone already committed to a particular growth model (and convinced of the superiority of Yamaguchi's data).

There are also instances of Hull missing out data from the original study, but still assuming it when drawing the curve (1943, page 271) and also of ignoring outliers which conflict with the fitted exponential (1943, page 166). Both curves are displayed in Figure 3.

Interestingly, in only one paper (Gladstone et al., 1947) did Hull directly compare other functions with the exponential growth equation. Although this function failed on one of the three fitting criteria (sum of least squares deviations), it did come out best on the other two (realistic intercept and a finite asymptote). A more detailed analysis of this paper, however, reveals an inconsistent application of at least one of these criteria on an earlier data set, where an unrealistic intercept is ignored in order to fit an exponential growth function (page 519). Figure 4 shows the three functions fitted to the data. Notice that the intercepts imply a negative reaction potential.

Only in 1947 (Felsinger et al.) did Hull explicitly describe his curve-fitting sequence in detail. It is worth going briefly through this process, since it is clear that there are key stages in this routine where Hull's presuppositions, especially his commitment to exponential growth models, could enter and termine the final shape of



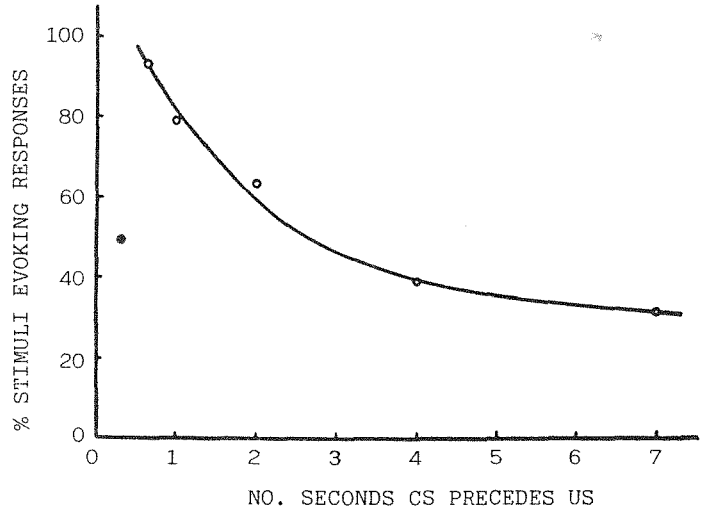
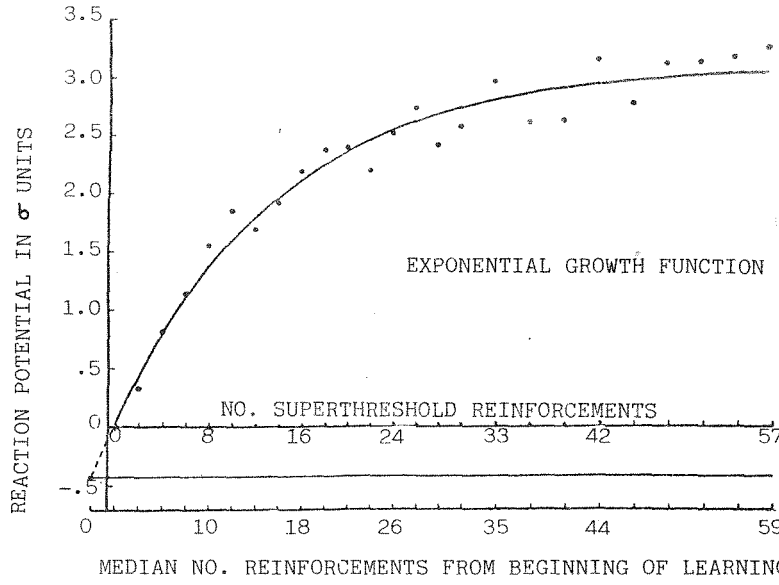


FIG. 3 MISSING DATA AND OUTLIERS. FROM HULL (1943).



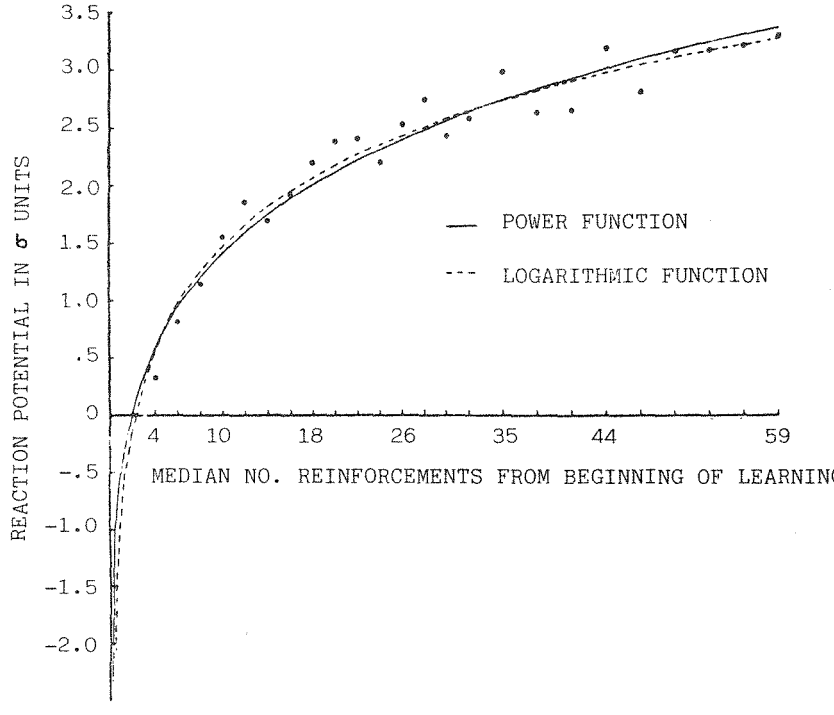


FIG. 4 COMPARING CURVE-FITS. FROM GLADSTONE ET AL. (1947).

the function. Thus step one consists of plotting the data and fitting a smooth hand-drawn curve through the averages of the points by eye. The second step involves the selection of a suitable class of functions to fit this curve, the class being chosen from the analyst's general knowledge of the graphical forms of such formulae. In step three, the data is linearised using a transformation suggested by the choice of function from step two. Hull only mentions semi-log and log-log transforms at this stage, or, to be more precise, he says that the raw data is plotted on either semi-log or log-log graph paper to see if a linear fit appears suitable. Note that only certain functions, most prominently those involving exponentials, may be linearised by such transformations.

In step four, the parameters of the linear model are read off the graph when a straight line had been successfully achieved by eye-fit. The parameter values are then substituted into the original equation from step two and sample theoretical values generated. These are then plotted with the original unlinearised data. The final step consists of an adaptive try out of different parameter values around the estimated ones. This fine-tunes the theoretical curve so as to achieve a minimum sum of average squared deviations of the data from the theoretical curve.

We believe that the analyst's presuppositions could influence most if not all of the fitting stages, mainly because of the highly informal, non-mechanical nature of the fitting process. We would, however, argue that steps one, two and three (the last because of the limited choice of transforms) are particularly susceptible to such contextual pressures, although even the informal nature of the graphical parameter estimation procedures in step four could lend itself to a directed eye-fit of the linear function.

It is worth contrasting this sequence with the much more sophisticated least squares technology available to experimenters over the period 1920 to 1950. In particular, a number of standard texts of the period confirm that least squares methods were sufficiently well developed to be applied directly to both linear and non-linear functions, although linearisation was often recommended for non-linear fits. Moreover, goodness of fit was routinely measured by chi-squared, eta-squared and ANOVA, as well as by residuals (Merriman, 1911; Rietz, 1924; Daniels, 1928; Peters and Van Voorhis, 1940; Deming, 1943). In fact, the Rietz book (1924) is of particular

interest since it was widely quoted by Hull, his students and research associates, but not on the topic of curve-fitting. In general, therefore, one is tempted to conclude that Hull deliberately ignored the more substantively neutral methods available to him in favour of the more subjective approach outlined earlier.

It is also worth noting the likely source of the curve-fitting sequence. This was contained in Daniels' 1928 book (particularly pages 229-239), which was used by Lewis as the text for his 1930s graduate course in statistics for psychology at Iowa State. The contacts between Hull's department at Yale and Iowa State were numerous and longstanding. For example, Hull's great disciple and evangelist, K.W. Spence, was Lewis' colleague at Iowa State at least until the time of Hull's death.

Final thoughts

Students of Hull's work will have recognized that several of our points have been made earlier, particularly in the brilliant and devastating critique mounted by Koch in 1954. What we argue, however, is that whereas Koch started from a positivist scientific base and accused Hull of philosophical and scientific inconsistencies, we begin by believing that Hull was consistent towards what he knew was the truth (in 1951 he referred to his exponential growth functions as "the laws of behavior") and that his methods of data analysis were directed by that commitment. Furthermore, a major point of our paper is that this commitment was prior to his main experimental programme of the middle 1940s. Indeed, one finds that even as late as 1943 his major curve-fitting exercises were performed on data generated mainly by workers unconnected with Hull or with his views.

We also maintain that such presuppositions operated through his apparently "objective" curve-fitting methods, leading him and his associates to push their statistical techniques beyond what would have been acceptable, even in the 1940s. Finally, we have not argued that Hull's case is typical of all research in psychology, although there is accumulating evidence for the strong role that presuppositions play in behaviourism generally (see Lovie, 1984). However, we would urge historians (and philosophers) of psychology to consider the influence that the intellectual context has had on the direction of developments in their subject.

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SOCIAL CONTEXT AND INVESTIGATIVE PRACTICE: AMERICAN AND GERMAN PATTERNS IN THE EARLY 20TH CENTURY

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For an understanding of the historical development of 20th century psychology it is important to pay attention, not only to differences of theoretical orientation, but also to differences in investigative practices. During the present century it has been these practical activities, often in the laboratory, which have actually generated the products that counted as psychological knowledge. Investigative practices are far from being theoretically neutral, merely technical, instruments. On the contrary, they embody ontological commitments and are extremely sensitive to the social context in which psychologists operate. Thus, they are a key mediating factor between the social and intellectual roots of the discipline and the empirical facts which the discipline generates.

Among the major changes in investigative practices which marked the history of psychology during the first half of the 20th century, there is one which is particularly striking and also of quite fundamental importance. This is the change from methods dedicated to the direct analysis of psychological processes in individuals to methods dedicated to the analysis of the distribution of psychological characteristics in groups or populations. With very few exceptions all studies during the first three decades of scientific psychology produced results which were attributed to individual subjects. It was the responses of specific individuals under various conditions which were analysed and presented in published reports. By contrast, a glance at the psychological literature in more recent times will show that the vast majority of studies do not report any findings attributed to indivdual subjects, but only report results that pertain to groups, often of course subjected to different treatments. Any conclusions about processes taking place in individuals now have to be mediated by rather problematical techniques of statistical inference.

The question is how and why this large scale and fundamental change came about. A comparative analysis may contribute something to

an answer to this question. In particular, I would like to compare the American and the German case during the earlier part of the present century. To do this I analysed all empirical reports published in certain key American and German journals at ten year intervals, sampling three consecutive volumes at each point (1914-16, 1924-26, etc.)

Such an analysis reveals that while both the German and the American literature show a very similar pattern of investigative practices at the beginning of the century, they soon begin to diverge. Broadly speaking, the practice of concentrating on group results has obtained a firm foothold in American psychology by the second decade of the 20th century and then gradually gains in acceptance, until it becomes the predominant form of research practice in the years just prior to World War II. By contrast, the German research literature continues, in the main, to use the practical approach that was in vogue early in the century. However, in order to understand the nature of this divergence it is necessary to adopt a more differentiated view of the research literature.

In particular, it is necessary to make a distinction between the practices reported in journals with an orientation towards the applications of psychology and practices reported in journals devoted essentially to "pure" reserarch. In the American literature the preference for group data clearly originates in the context of applied psychological research. From its first volume in 1910, this pattern of investigative practice dominates the pages of the Journal of educational Psychology. For the period 1914-16 three quarters of all the empirical studies published in this journal reported virtually no data on individual subjects at all while only 12% of these reports limited themselves to individual data. By contrast, during this same period, over two thirds of the studies reported in the American Journal of Psychology were of the individual type. In subsequent years the characteristic pattern for the Journal of Educational Psychology become even more pronounced. By the mid-nineteen twenties the percentage of studies essentially devoted to the production of statistical data on groups of subjects exceeded 90 and by the mid-nineteen thirties it reached close to 100%.

The pattern for the Journal of Applied Psychology, which begin publication in 1917, was very similar. From the beginning, three quarters of the empirical studies reported there were devoted to data

on groups rather than on individual subjects, and by the mid-nineteen thirties that figure had exeeded 90%.

By contrast, the American research literature outside the applied area was relatively slow in adopting the new investigative practices. However, a tendency to move in this direction emerges in the nineteen twenties and accelerates in the nineteen thirties also in this literature. By the period 1934-36, over half of the research reports in the American Journal of Psychology (54%) and the Journal of Experimental Psychology (60%) are essentially devoted to the reporting and analysis of group data. Thus, during a twenty year period there has been a clear reversal of the preferred style of research published in these journals from a predominance of studies on individual subjects to a much heavier reliance on group data. What is worth noting is that the applied literature leads the way in this respect and that the so-called "pure" research literature follows. Thus, by the late nineteen thirties the American "pure" research literature has adopted on a large scale certain aspects of investigative practice which were always characteristic of the American applied research literature.

When we turn to the German literature during the same period we find a pattern that is similar to the American pattern in some respects but different in others. The most important similarity lies in the distinction between the preferred research style of the "applied" and the "pure" research literature. During the second decade of the 20th century virtually 100% of the research reports published in such journals as the Zeitschrift für Psychologie and Archiv für die gesamte Psychologie are devoted to analysing data from individual subjects. However, during the same period a little over half of all the empirical research reported in the Zeitschrift für angewandte Psychologie (56%) and the Zeitschrift für pädagogische Psychologie und experimentelle Pädagogik (54%) is essentially devoted to the analysis of group data rather than data on individual subjects. Thus, the overall nature and direction of the difference in predominant investigative style between the "pure" and "applied" research literature is rather similar in the American and in the German case.

However, this degree of similarity makes the differences all the more striking. First of all, the German applied literature never shows the overwhelming preference for group studies that is so

characteristic of the American applied literature. Not only is this preference less marked at the beginning, but is does not grow during either the 1920's or the 1930's, so that as time goes on the discrepancy between the German and the American applied literature increases. While the latter becomes almost exclusively devoted to group data, the German literature continues a pattern established early in the century, where only a little over half of the published empirical studies are of this type.

Secondly, the more purely academic German literature does not show the same degree of adaptation that has been noted for the equivalent American literature. When one examines journals like Zeitschrift für Psychologie, Archiv für die gesamte Psychologie and Psychologische Forschung the pattern of investigative practice remains overwhelmingly committed to studies based on individual subjects in the 1920's. By the 1930's a trend is detectable which points in the same direction as American developments, but it is much less marked. Even at this time, studies which are no longer based on the analysis of individual responses but on the analysis of group data only account for 20% of all empirical studies in the Zeitschrift für Psychologie and 23% of those in the Archiv für Psychologie and 23% of those in the Archiv für die gesamte Psychologie. At that time the corresponding American journals have already established a clear preference for group data.

In attempting to account for these patterns it is appropriate to begin with the most striking element of similarity in the two cases. Both in the American and in the German case the original impetus for the development of investigative practices that generate group data appears to emerge in the course of producing a kind of psychological knowledge that is regarded as practically useful. This is hardly surprising when one remembers that the one kind of application of psychological knowledge which was professionally successful involved placing such knowledge in the service of established social institutions. That meant producing a kind of knowledge which would be useful to those who had to make administrative decisions within an existing institutional framework. More specifically, psychological data was seen as potentially useful in the rationalizing of two kinds of administrative decisions - those pertaining to the selection of individuals and those pertaining to the selection of programmes. But data relevant to such decisions had to be in the nature of

statistical information based on groups or populations, otherwise there would be no payoff for the institution and its administrators. From the point of view of long term effects, the most important single set of institutions which provided a market for psychological knowledge of this type were educational institutions.

The differences between the German and the American case were probably due partly to certain crucial differences in the market for psychological knowledge and partly to certain differences in the professional project of the discipline. First, a brief indication of the most relevant difference in the education market.

In America, the initial success of an applied psychology based entirely on group data appears to have been due to a professional alliance formed between certain psychologists and a new group of professional educational administrators dedicated to the rapid and ruthless rationalization of the educational system to meet the requirements of corporate industrialism. These administrators were devotees of a clearly formulated "cult of efficiency" which was based on the explicit transfer of the principles of industrial management to education. Data on the distribution of psychological characteristics in populations and on the relative efficiency of programmes were regarded as useful in the pursuit of these aims. This provided American psychologists with their first really large scale market, on condition that they produced the kind of information that this market demanded.

In Germany such a market only existed in a localized and rudimentary way. The educational system was much less adaptable and continued to rely on traditional methods of organization and selection far into the 20th century. There was no group equivalent to the American educational efficiency experts with whom psychologists could enter into a mutually beneficial professional alliance. One German group that was interested in certain products of psychological research were reform minded teachers, but their means were very limited, and they certainly did not share the administrators' preoccupation with purely statistical data based on populations. Thus, the impetus for the production of such data never became as powerful as in the American case.

But one must also see these external factors in relation to the professional project of psychologists, especially academic psychologists. In America, the conviction that psychology as a discipline

could progress only in so far as it could produce knowledge useful to the administrators of social institutions can be documented as early as William James. Such convictions were widely held among academic psychologists, and when, in the second decade of the 20th century it was practically demonstrated what kind of knowledge was needed to achieve this sort of usefulness, there were relatively few obstacles to the gradual adoption of the new research style by the majority of American psychologists.

By contrast, German academic psychologists found themselves in a situation where their enterprise was obliged to legitimate itself in terms of the values represented by a well entrenched traditional intellectual elite, if it was to prosper. On the level of research practices there was no chance of accomplishing this by means of methods that were seen as a kind of superficial census taking which negated human individuality as well as the traditionally recognized mandate of psychology to explore the individual mind and personality. In any case, these judgements were characteristic not merely of groups completely outside the discipline, but of academic psychologists who had complex external and internal ties to these groups. Thus, even those, like William Stern, who acted as spokesmen for the new methods often did so in an equivocal and half hearted manner.

Professional commitments to different styles of investigative practice can therefore be seen as mediating between the social context in which psychologists operate and the kind of knowledge which they produce.

THE USE AND MISUSE OF ARISTOTLE'S FOUR CAUSES IN PSYCHOLOGY: A CASE OF OBSCURUM PER OBSCURIUS

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Summary

Descriptions and interpretations of Aristotle's Four Causes by psychologists are varied and conflicting. A careful examination of the Oxford translation and subsequent analyses by philosophers indicates that "cause" is an inappropriate translation of "aitia", and that the four causes are not equivalent to any modern version of cause. Instead, the four causes should be understood as four "reasons why" something is the way it is. In the most basic sense, the four causes are answers to: What is it? (formal cause), out of what is it made? (material cause), by what agent? (efficient cause), and for what end? (final cause). However, such an analysis oversimplifies the obscure aspects of certain Aristotelean notions, such as "form" and "essence". Attempts to mesh Aristotle's four causes with the Humean view of cause have lead either to distortion of Aristotle's concepts or to the view that Aristotelean notions are a barrier to scientific progress. Nevertheless, the issue of the four causes has, either explicitly or implicitly, played a major role in metatheoretical debate among psychologists. Two examples are discussed in which Aristotle's four causes are explicitly invoked to defend the adequacy of the Skinnerian and Piagetian systems. Although such use of Aristotle is viewed here as a distortion, the use of the four causes in modern writings suggests that notions of causality may be of fundamental importance in understanding historical trends in psychological theory and research.

At various times, psychologists of widely differing metatheoretical persuasions have invoked the Aristotelian tetrad of material, formal, efficient and final cause. Aristotle's four causes have been used to defend a particular position, criticize an alternative view, or to lend an air of tradition and gravity to an argument regarding causality. In the course of these discussions of Aristotle's causes, a most curious state of affairs has arisen: the accounts of the four causes given by different writers do not resemble each other, and, quite often do not resemble Aristotle's original description. Among

psychologists, misunderstanding or misinterpretation of Aristotle's four causes appears widespread. In this paper, I will attempt to clarify some aspects of the Aristotelian tetrad, and show that other aspects remain obscure. The issue is not simply one of historical accuracy; the vigorous debates that occurred during the 1960's and 70's between psychologists of radical behaviorist and cognitive perspectives were, in part, debates over the issue of what is acceptable as a "cause" in psychological inquiry.

Recent descriptions of Aristotle's four causes.

A few relatively recent examples will illustrate the diverse versions of the four causes that have appeared in the psychological literature. Griffore (1978) gives the following set of definitions:

> material causation: the matter of which something is made
> formal causation: the form in which something is made
> efficient causation: the agents which initiate or make something
> final causation: the purpose or end for which it is made (p.182).

These definitions are reasonably close to the original Aristotelian version, although the meaning of terms such as "form" or "purpose" is by no means a simple matter. Griffore's (1978) definitions appear to have little in common with the following version given by Frieze and Bar-Tal (1979):

> Material cause: Human theories of causality (attempts at identifying formal causes)
> Formal causes: Ultimate or true causes (which can never be knowable to humanity but which are continually being sought after).
> Efficient causes: Apparent physical causes.
> Final causes: The ultimate purpose of events (p.4).

Only the descriptions of final cause bears any resemblance to Griffore (1978). Interestingly, Frieze and Bar-Tal (1979) cite Lana (1969) as the source of their description of the four causes. But inspection of Lana (1969) reveals only the following definitions, which bear little resemblance to Frieze and Bar-Tal (1979):

> First is the material cause, which refers to the passive substratum out of which a theory comes to be and which persists. The second is the formal cause, which describes the ultimate nature of the thing, that is, the statement of the archetype or essence, usually given by a mathematical relationship of some sort. Third is the efficient cause, which indicates the source of motion involving an external compulsion affecting a body in the most general sense... The fourth cause is the final cause, which specifies the end or purpose or good of anything (p.7).

Clarke (1982) provides definitions of material and formal causes that appear quite different from all of the above versions. Clarke (1982) defined material cause as "the enabling conditions or powers that make something a possibility," and formal cause as "the shaping factors governing the form of something which other factors have caused to happen in some form or another." Still another version is found in the Encyclopedic Dictionary of Psychology (Harre & Lamb, 1983) where formal cause is defined as "a preexisting pattern of meanings" and final cause as the "selection of meanings that are to be achieved in action."

Some versions of the four causes are presented to show that a contemporary position is consistent with or supported by Aristotle's notions. For example, Moore (1984) argues that material, formal, efficient, and final cause are conceptually equivalent to the Skinnerian concepts of organism, discriminative stimulus, contingency, and reinforcer. Moore contends that "the two sets of uses, Aristotelian and Skinnerian, are entirely consistent" (Moore, 1984, p.88). Other writers have used the same Aristotelian causes to support positions which are clearly at odds with Skinner's radical behaviorism. Overton and Reese (1973) use the concept of formal cause, which they define in terms of pattern or structure, to argue for the central importance of "cognitive structures" in the study of development. In separate commentaries on a major critique of Piaget by Brainerd (1978), both Bates (1979) and Buss (1979) use the four causes to argue that Brainerd's criticisms are inappropriate.

There are several possible reasons for the great diversity in these renditions of the four causes, such as difficulties of translation and reliance on secondary sources. In addition, Aristotle's notions may be fraught with conceptual ambiguities and difficulties and thereby lend themselves to a variety of interpretations. The original translation of the four causes must now be examined.

What is the Aristotelian view of cause?

A first consideration in any discussion of Aristotle concerns the translation that will be used. For English readers, the "standard" translation is generally considered to be the Oxford Press edition of The works of Aristotle translated into English, under the editorship of W.D. Ross (1912-1952). A variety of other translations, particularly of specific works such as the Physica, have appeared in recent

years (e.g., Charleton, 1970; Hope, 1961). Although the examples used to illustrate each type of cause are quite consistent across translations, the exact English words used to define each cause are not. The issue of translation is often neglected in discussions by psychologists.

A second question is: to which of several discussions of cause in Aristotle's work should we refer? In fact, Aristotle discussed the four types of cause in at least three places: Physica, Book II. Metaphysica, Book Alpha and Book Delta, and Analytica Posteriora, Book II. Additional discussions of cause appear in De Generatione et corruptione. The variation in how the causes are defined in each work is substantial. In the Metaphysica alone there are two separate and slightly different sets of definitions (i.e., Book A.3 and Book Delta 2). Although no one of these treatments can be considered the "correct" one, the subsequent discussion will be based on Book II of the Physica in the Ross (1928) edition, since this passage is most commonly used in discussions of Aristotle.

The relevant section of the Physica begins with the notion that knowledge of things and changes in things requires knowing "why". The discussion then continues:

> In one sense then, (1) that out of which a thing comes to be and which persists, is called 'cause', e.g., the bronze of the statue, the silver of the bowl, and the genera of which the bronze and the silver are species.
> In another sense (2) the form or the archetype, i.e., the statement of the essence and its genera are called 'causes' (e.g. of the octave the relation of 2:1, and generally number) and the parts in the definition.
> Again (3) the primary source of the change or coming to rest; e.g., the man who gave advice is a cause, the father is cause of the child, and generally what makes of what is made and what causes change of what is changed.
> Again (4) in the sense of end or that for the sake of which a thing is done, e.g., health is the cause of walking about... This then perhaps exhausts the number of ways in which the term 'cause' is used. (Physica, Book II.3).

These few sentences define (1) material, (2) formal, (3) efficient, and (4) final cause. The analysis is much more complex than this passage suggests; Aristotle proceeded to classify the "mode" of causation along several dimensions (e.g., potential vs. actual) and then discussed the status of chance and spontaneity as causes (Physica, Book II.3, II.4). Formal, efficient and final cause often coincide, and the same thing may serve as one kind of cause in one case, and another kind of cause in a different case (see Charlton,

1970). Finally, there exists an enormous body of scholarly thought and debate on the question of what these four causes mean, ranging from ancient writers to St. Thomas Aquinas to contemporary philosophers (e.g., Hocutt, 1974; Gotthelf, 1976; Sorabji, 1980). The task of interpreting Aristotle must be approached with humility and trepidation.

Fortunately, Aristotelian scholars and philosophers have come to agreement about some general aspects of the problem. First, there is consensus that "causes" is not the best translation of the word "aitia" and that this unfortunate translation has created substantial misunderstanding. Randall (1960) in a very straightforward analysis, noted that "aition" means literally the "answer or response to a question" (p.123). Specifically, the word "aitia" is connected with the idea of "blame" or "accountability" (Charlton, 1970). The notion that cause is not the proper translation of "aitia" has been echoed more recently by Vlastos (1969) and Hocutt (1974). To say that the bronze causes the statue makes very little sense to us. Aristotle was after "explanation," particularly explanation derived by syllogistic reasoning, according to Hocutt (1974). Sorabji (1980), Randall (1960) and others have noted that only efficient cause in any way resembles a "cause" in the contemporary sense, and even this resemblance is deceptive. Thus the four causes should not be thought of as "causes" in the modern sense, but must be understood as the four "dioti's" or "reasons why," that are necessary for understanding.

Despite this general problem of the meaning of "cause", it is possible to have some idea of the meaning of <u>material cause</u>. When "cause" is taken in terms of "why is this object what it is?", then part of the explanation must lie in the answer to "of what is it made?" i.e., the material cause. While this notion makes sense for bronze of a statue or other objects, it is not clear how to discuss the material cause of a behavior in Aristotelian terms. Modern writers (e.g. Moore, 1984) speak of material cause in human behavior as the genetic make-up and physiological structure of the organism, but it is not at all clear that this is what Aristotle meant. A final complication is that material cause rests on the notion of matter as distinct from form. Aristotle's notion of matter does not correspond in any simple way to our modern notion of the term (see Wieland, 1975 for a discussion).

The concept of formal cause poses serious problems. What could Aristotle mean when he defined formal cause as "the form or archetype, i.e., the statement of the essence and its genera... and the parts in the definition"? One possibility is that by form, he may mean Platonic Forms. But Aristotelian scholars such as Owen (1975) have shown that Aristotle clearly takes issue with the Platonic concept of Forms. A second possibility, often adopted by modern writers, is that by formal cause Aristotle means the pattern, organization, and structure of a thing, or the "blueprint" (e.g., Rychlak, 1968). But Aristotle's question, for which formal cause provides an answer, was not "how are the parts put together?"; instead, the question was "what is it to be this thing?"

A third interpretation, which I believe comes closer to Aristotle's meaning of formal cause, can be derived from the example that Aristotle used. As indicated above in the exerpt from Physica, Aristotle's example is that the ratio of 2:1 is the formal cause of the octave. Formal cause is, in Aristotle's words, "the statement of the essence." Unfortunately, "essence" is an extremely troublesome Aristotelian concept, and Aristotle expressed and used the concept in a variety of ways. According to Sorabji (1981) the simplest sense in which we can understand the idea of essence is as "definition" of a certain kind. The essence of a thing is the statement of the genus, or category, to which it belongs. At times Aristotle also required that the statement of the essence must also include the "differentia", that is, the characteristics which make this thing distinct from others.

To return to the example, two strings which are in the ratio of 2:1 in length will produce notes an octave apart, as Aristotle well knew. Thus an octave is partially explained (or at least understood) by noting that it belongs to the general class of things which are in the ratio of 2:1, and a peculiar arrangement of steel girders in the middle of a park is partially explained by noting that it is a sculpture. Hocutt (1974) summarizes the issue nicely: "to understand what something is is also to understand why it is what it is" (p.394).

This appealing analysis greatly oversimplifies the issue. It is not clear how Aristotle means such terms as "archetype" or even "form" (see above, also Charlton, 1970). Nevertheless, the notion of "definition" is a useful way to get some idea of formal cause. We

should recall that Aristotle was deeply concerned with the systematic classification of plants and animals, and it is in this realm that formal causes (as definitions) are clearly of assistance in understanding what something is.

The concept of "efficient cause" appears equivalent to our contemporary notion of cause as "productive event." But there are some critical differences. As noted by Collingwood (1945), Hocutt (1974) and Sorabji (1981) the contemporary notion of cause, as used by most modern scientists, requires that causes precede effects in time, whereas Aristotle argues that any cause, including efficient cause, could be simultaneous with its effect. Moreover, Collingwood (1945) and Wieland (1975) maintain that efficient cause was a conception about things rather than processes or events. Finally, Aristotle did not specify an empirical procedure for determining the efficient cause. In contrast, modern conceptions of cause since Bacon and Hume (1739/1927) have been explicitly tied to a set of observations that must be made to establish something as a cause. In sum, we may be tempted to identify efficient cause with our modern conception of cause, but we must be cautious in doing so and recognize that this notion is part of an elaborate system of thought quite different from our own.

The notion of "final cause", "that for the sake of which a thing is done," presents even more serious problems for contemporary understanding. The question of teleology has been debated for centuries. Does the notion of final cause imply that conscious desires and purposes reside in all natural events, such as the development of an acorn into an oak tree? The answer is not so simple (see Rist, 1965; Sorabji, 1980). Aristotle attributed conscious desire to some natural objects, e.g., animals, but not others, e.g., plants (Sorabji, 1980). Nevertheless, conscious purpose or desire is not a necessary feature of Aristotle's final cause.

The second major difficulty with final cause arose from the widespread adoption of Hume's notion of causality by Western philosophers and scientists. In the Treatise of Human Nature, Hume (1739/-1927) contends that a cause must always precede its effect. For scientists, the problem was clear: if causes must precede their effects, teleological, final explanations must be rejected. The result is, according to Wieland (1975):

> Scientists today consider Aristotle's teleological interpretation of nature to be at best an interesting

> mistake, perhaps explicable in historical terms. They hold it responsible for delaying the progress of science some two thousand years. (p. 142).

The exclusive use of final cause to explain an object eventually made final cause the subject of satire. Indeed, scientists often delight in ridiculing final cause by quoting Dr. Pangloss' assertion that we have noses in order to hold up our spectacles.

For psychologists, the rejection of future events as causal is a difficult problem. Clearly; human behavior is bound up with goals and purposes in some inherently important way. In order to remain faithful to the Humean dictum, it was necessary to postulate preexisting wishes, desires, or instincts to substitute for goals or purposes. In modern terms, cognitive and motivational variables were used to transform unacceptable final causes into acceptable efficient causes.

If final cause is taken, like the three other causes, to be "what we need to know in order to understand things or events," the problem of temporal order and final cause simply vanishes. The clearest analysis of this view is given by Gotthelf (1976). The oak tree is not a cause of the acorn in the Humean sense, but the oak tree is a final cause of the acorn, in that we can only understand an acorn if we know that it will develop into an oak tree. Knowing "that for the sake of which," the final cause, thus makes the world intelligible. The future does not "cause" the present, the future allows us to understand the present.

Modern writers who use the four causes to describe human action generally commit a major ommission: Aristotle stated quite clearly in De Anima (Book II 4) that the soul is the formal, efficient and final cause of human activities. What Aristotle meant by soul, or "anima", is clearly very different from what is meant in Western religious traditions. The concept of soul was Aristotle's way of answering the question: "what differentiates living from non living things?" (see Hartman, 1977 for a full discussion). To use the four causes to describe human action without using the concept of soul is a major distortion of Aristotle's thought.

In summary, Aristotle's four causes must be understood as the four "dioti's" or "reasons why" something is the way it is: What is it? (formal), out of what is it made? (material), by what agent? (efficient), and for what end? (final). When we have the answers (aitia) to all four of these questions, we can be said to understand something.

The four causes and theoretical debate in psychology.

Despite the fact that contemporary versions of the four causes are often a distortion of Aristotle's original notions, the underlying issue raised by the four causes has served as a major source to theoretical debate in psychology. Any particular position, such as radical behaviorism (Skinner, 1953) or genetic epistemology (e.g., Piaget, 1952) emphasizes or excludes one or another form of cause, thus setting the stage for theoretical incompatability with other systems. Two examples of how the issue raised by Aristotle's four causes has played a major role in current debates will be examined. The first example arises out of discussions of whether Skinner's experimental analysis of behavior is deficient in its conception of causality. The second example arises from a recent debate over the explanatory value of Piagetian theory.

What is the Skinnerian view of cause?

The clearest statement of Skinner's position on cause is found in Chapter III of Science and Human Behavior (Skinner, 1953, p.23):

> The terms 'cause' and 'effect' are no longer widely used in science... The terms which replace them, however, refer to the same factual core. A 'cause' becomes a 'change in an independent variable' and an 'effect' a 'change in a dependent variable.' The new terms do not sugggest how a cause causes its effect: they merely assert that different events tend to occur together in a certain order.

Skinner then continued with a discussion of the problems inherent in structural, neural and mentalistic "causes." While granting that structural factors, such as genes or body type, are responsible for some differences in behavior, their role in a science of human behavior is seen as limited for two reasons: the problem of disentangling cause and effect from correlational data and the limited practical value of such "causes" in producing changes in behavior. Neural "causes" are also seen to be of limited practical value and often circular in their logic. The complete rejection of mentalistic of "psychic" causes is a basic and firmly argued tenet of Skinner's position. Skinner summarized this section of Science and Human Behavior with a clear statement of the goals of the science (Skinner, 1953, p.35).

> The external variables of which behavior is a function provide for what may be called a causal or functional analysis. We undertake to predict and control the behavior of the individual organism. This is our "dependent variable" the effect for which we are to find the cause. Our "independent variables" - the causes of behavior -

> are the external conditions of which behavior is a function. Relations between the two - the "cause and effect relationships" in behavior - are the laws of a science.

Clearly, Skinner views past and present environmental events as the causes that are suitable for the scientific analysis of behavior. In the experimental analysis of behavior, such events would include positive and negative reinforcers, discriminative stimuli, eliciting stimuli, and operations such as deprivation and satiation. These events correspond most closely to Artistotle's notion of efficient cause. That is, these events most closely resemble the idea of "the primary source of change or coming to rest" (see above), i.e. the efficient cause. There are important differences between the Skinnerian and the Aristotelian notions. As noted earlier, Aristotle does not require that a cause precede its effect, while Skinner clearly does. Causes for Skinner are to be established by finding functional relationships through a systematic process of empirical investigation, that is, through experimentation of a special sort. By implication, an event must be systematically manipulated in order to establish its status as an independent variable and therefore as a cause. Causes cannot be established through casual observation or syllogistic reasoning.

Although Skinner places the greatest emphasis on a notion of cause similar to efficient cause, he does not completely exclude all other forms of cause. He grants that the genetic make-up or the physical structure of the organism may function as causes of behavior. Nevertheless, these causes are seen as limited in value for the prediction and control of the behavior of individuals.

Moore (1984) contends that all four forms of Aristotelian cause, material, formal, efficient, and final, are entirely consistent with Skinner's analysis. According to this view, the Aristotelian notions have clear counterparts in Skinner's functional analysis of behavior. The issue is not trivial: what is at stake is whether or not the Skinnerian system is fundamentally deficient in its analysis of cause.

In Moore's (1984) analysis, final cause is said to be equivalent to "the reinforcing consequence achieved by the behavior," formal cause is identified with "the antecedent discriminative stimulus that sets the occasion for the response," material cause is taken to be "the organism," and efficient cause is identified with "the notion of the contingencies, both ontogenic and phylogenic". A full analysis of

this view is beyond the scope of this paper. The analysis of Aristotle given earlier, however, clearly indicates that Moore's translation of Aristotelian terms into Skinnerian terms is a gross distortion of one or both systems of thought. Overall, it is important to reiterate that none of Aristotle's four causes are a cause in any modern view, whether Skinnerian or not.

Despite Moore's (1984) contention, the Skinnerian system does not make use of any version of formal and final cause. Skinner's emphasis on a kind of efficient cause has enormous implications for both the kind of questions to be asked and for research methods to be used. As a consequence, experimental methods are to be preferred over other research strategies, environmental events are to be the major focus of research, and demonstration of functional relationships is to be the major goal of inquiry. Moreover, explanations of behavior from other theoretical systems based on internal structures (e.g. Piaget, 1952), personality traits (e.g., Cattell, 1965), or the future direction of the organism's development (e.g., Werner, 1948) are at best insufficient or at worst unacceptable. The Skinnerian viewpoint has similarly been attacked over the last 30 years for failure to analyze the structures presumed to underly behavior (e.g., Chomsky, 1957) or the organized course of behavioral development (see Overton & Reese, 1973). Thus the historic confrontation between mentalistic and behavioristic psychology was in part a confrontation between a modern version of efficient cause and modern versions of formal and final cause.

A second example of the importance of the four causes in recent theoretical debate in psychology arises from a strong critique of Piaget written by Charles Brainerd (1978) in Behavioral and Brain Sciences. A major thrust of Brainerd's argument is that Piaget's stage theory is deficient in explanatory power. According to Brainerd, a stage theory must specify precisely the antecedent conditions which produce the changes in behavior, or at least specify precisely how these changes can be measured. Brainerd contends that Piaget's stages fail in this regard, and for this, as well as other reasons, the stages do not explain changes in behavior.

Among the dozens of commentaries that were published in connection with Brainerd's paper, two are of most interest here. Elizabeth Bates (1979) and Allan Buss (1979) both argue that Brainerd's

critique is based on a limited notion of explanation. Both cite Aristotle's four causes as a more comprehensive view of explanation and argue that Brainerd places undue emphasis on material and efficient causation while ignoring formal and final causation. Bates continues with a discussion of the central role of formal cause in Piagetian explanations, while Buss emphasizes the major role played by final causes in Piaget's theory.

The reply by Brainerd (1979) is illuminating. He ridicules the use of Aristotle by Bates and Buss as "Thomist" (p.150) and views their comments as a curious historical anachronism. Brainerd repeats the old notion that Aristotle was a tremendous obstacle to scientific progress and notes that using Aristotle raises the "spectre of teleology." (p.151). As his authority on the matter, Brainerd cites Hume!

While both Bates and Buss have somewhat distorted the original Aristotelean notions, the issue they raise is fundamentally correct. Brainerd (1978, 1979) treats causal explanations that specify antecedent variables as the only acceptable kind of explanation. But the problem of cause and explanation has a much more complex history that Brainerd's argument suggest (see Bunge, 1959). At the heart of this debate is the question of what is acceptable as cause and the relationship between cause and explanation or understanding.

Conclusion

Many aspects of Aristotle's four causes are, and will remain, obscure. Aristotle's notions emerged in the context of the language, consciousness, and culture of Greece in the fourth century B.C.E., and no easy rendering into modern terms is possible. The various interpretations of the four causes offered by modern psychologists often seem even more obscure; hence the subtitle of this paper, "explaining the obscure by something even more obscure". Nevertheless, the basic issue raised by Aristotle nearly twenty four centuries ago, that is, the various senses in which we understand "cause," remains a critical point of contention. That psychologists attempt to resolve this metatheoretical debate through reference to Aristotle's four causes is indeed fascinating. That psychologists distort and misuse Aristotle's notions is, perhaps, an unfortunate legacy of the historic dissociation of psychology from philosophy.

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THE FOUNDATION OF MODERN ASSOCIATIONISM RECONSIDERED

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Summary

Most historians of psychology credit James Mill with the most complete and rigourous expression of associationism, elaborated by combining Hartley's conception of the associations of ideas and Bentham's utilitarism. Taking into account the ethical and epistemological gap existing between modern psychology and psychoanalysis leads one to reconsider that moment of the history of psychology. It is shown that, through the elimination of what Bentham called "fictions", the foundation of modern psychological associationism was already inducing psychology to take the way which actually opposes it to the outcomes of Freud's discovery of the unconscious.

Even if it has escaped detection or been mischaracterized for a long time, there is nonetheless a deep ethical and theoretical difference between psychology and psychoanalysis:

1) According to Sigmund Freud, human psychism is dominated by the nostalgia of the lost object: what reality can offer to satisfy human needs cannot, even potentially, bring happiness, since the "principle of pleasure" is relative to the unconscious wish and has no relation to the satisfaction of needs. Therefore, the pursuit of happiness on the one hand, and individual and social survival which depends on the satisfaction of needs in reality, on the other, are separated. Furthermore, the pleasure principle, it appears, can outweight biological necessities to the point where vital regulations are no longer taken into account.

Opposed to this, psychological theories are grounded on the postulate that, at least ideally, the same real objects are able to extinguish personnal dissatisfactions, to fulfil hopes (what can be asked for, an distinguished from the unconscious wish), as well as to satisfy vital necessities. From that point of view, the satisfaction of needs in reality can be taken as the basis, and eventually as the measure, of the psychological achievement of happiness. The two aspects are combined in the process of adaptation: the psychological

mechanisms are supposed to drive the subject in the direction of what is naturally useful.

2) This ethical gap has, of course, its epistemological translation. For psychology, the relation of the individual to reality can be completely accounted for through the scientific understanding of the organism and of its environment. It is not the same for Freud. Indeed, in order to account for the paradoxical character of the relation to reality as he conceived it, he introduced the theory a "psychic apparatus". But such a device has for Freud the theoretical status of a "fiction", constructed on the basis of biological and optical metaphors, but not reducible to the physical or biological value of the elements of these metaphors (Freud, 1899, VII, II).

The misperception of these differences between psychology and psychoanalysis found strong support in the confusion between

a) Psychological theories of the "association of ideas" and Freud's "method of free association".

b) The use of "utilitanian principles" in psychology, and the meta-psychological function of the "principle of pleasure" in Freud's theoretical elaborations.

According to the historians of psychology, utilitarism is associated with the name of Jeremy Bentham and is a revision of classical hedonism. Utilitarism was, according to these accounts, introduced to psychology by Bentham's pupil, James Mill, as a dynamic principle, in combination with associationism. But if we look more closely at Bentham's works (1), we can notice that what has been published by Charles K. Ogden (1932) under the title Bentham's theory of fictions has been largely neglected by psychologists and by historians of psychology, who stressed instead the pleasure-pain principle. Recognising the differences between psychology and psychoanalysis enables us to take into account Bentham's conception of fictions, to explain their neglect by historians of psychology, and to highlight a theoretical gap between Bentham and the psychologists who built upon his ideas, especially James Mill and John Stuart Mill. In this paper, we will explain how the combination of utilitarism with XVIIIth century associationism led to the elimination of Bentham's fictions, and everything connected to them.

In order to illuminate this point we have to look more closely at the history of associationism and utilitarism, to reconsider them, and to define more precisely the role played by Bentham's work and teaching.

From "associations of ideas" to the "laws of thought"

It is necessary to specify that the history of associationism cannot be reduced to that of empiricism. Our question is to determine what is constitutive of "that" associationism which offered the basis of misinterpretation with the psychoanalytic "rule of free association". That associationism is a description of thought, based on the postulate that laws accounting for the succession of ideas cover the totality of the thinking processes, and do this universally, without exception.

For us, these two features are brought together for the first time in David Hartley's Observations on man. Of course, Thomas Hobbes uses the terms of "consequence" (Latin, consequentia), "chaining" or "trayne" of ideas. But these terms have to be situated in the Galilean background of Hobbes. Laws of mind are not laws in the Newtonian sense. They are not internal to the process of thinking but, guided or not, their arrangement is dependent on the external reality of which they are an imitation (see Hobbes, 1651, I, 3). It is in quite the same way that we may understand John Locke's "connexion of ideas" which he calls natural, and which it is the task of the mind to "discover and preserve". And, when he introduces the expression "association of ideas" in the third edition of his Essays concerning human understanding, he opposes these "natural connexions" to "another connexion of ideas wholly owing to chance and custom". The description of the processes which establish this second type of "connexions of ideas" clearly indicates that it is only them which have been later called "associations of ideas" in its actual empirical meaning. For Locke only the "natural connexions" are involved in the normal, rational process of thinking, whereas the associations "made by custom in the mind of most men" are able to drive him to madness (John Locke, 1690, II, XXXIII).

The case of David Hume is even more problematic, and in fact quite variable, depending on which of his works is to be the reference. In the Treatise of human nature, Hume preserves the freedom of thinking: "Were ideas entirely loose and unconnected, chance alone would join them; and it is impossible the same simple ideas should

fall regularly into complex ones(...) without some bond of union among them, some associating quality, by which one idea naturally introduces another. This uniting principle among ideas is not to be considered as an inseparable connection; for that has been already excluded from the imagination: nor yet are we to conclude, that without it the mind cannot join two ideas; for nothing is more free than that faculty: but we are only to regard it as a gentle force, which commonly prevails..." (Hume, 1739, I, I, 4). The restriction concerning the freedom of the mind to connect ideas disappears in the Inquiry concerning human understanding published ten years after Hartley's work.

So, our problem is not to determine who is the "inventor" of "associationism" in its contemporary meaning. Many authors worked on this question in the first half of the XVIIIth century. But our purpose is to underline that, during the period when associationism so defined is being constitued, Hartley represents the first systematic expression of it, and has effectively, although with little convincing rationale, been recognised for that by the historians of psychology.

The "laws of association" considered the laws of thought have for Hartley a status similar to that which the laws of planetary movement had for Newton. Hartley refers to Newton in order to conceive of thought as a material process but, in fact, through Newton's theory of vibrations, it is Newtonian metaphysics and explanation of the order of the material universe which is pinpointed. This is true up to the point where Hartley explicitly says that the doctrine of vibration was not an essential part of his conception of association and of the thought process:

> And if the doctrine of vibrations be rejected, and sensation and muscular motion be supposed to be performed by some other kind of motion in the nervous parts; still it seems probable that the same method of reasoning might be applied to this other kind of motion. (Hartley, 1749, I, IV, conclusion).

The question of the materiality is not the real problem; rather it is ethical. For Newton, it is God himself as an intelligent being that maintains the material universe in the intelligible order he gave to it. As God has designed the universe so that it would be perfect, this "order" is perfection itself and all goodness (2). In this way, Hartley has given through his psychology a political translation of Newton's metaphysics:

> Malevolence may also be excluded in the following manner: if we suppose a system of beings to be placed in such a situation, as they may occasion either much happiness, or much misery, to each other, it will follow, that the scale will turn more and more perpetually in favour of the production of happiness: for the happiness that A receives from B, will lead him by association to love B, and to wish B's happiness in return: B will therefore have a motive, arising from his desire of his own happiness, to continue his good office to A: whereas the misery that A receives from B, will lead him to hate B, and to deter him from farther injuries. This must necessarly be the case, if we only admit, that every intelligent being is actuated by the view of private happiness, and that his memory and trains of ideas are of the same kind with ours (Hartley, 1749, II, I, 4).

But the support Hartley's Christian optimism found in Newtonian metaphysics met the contemporary ethical reflection from which associationism originated.

Individual happiness and the common welfare

It is not necessary to insist upon the fact that the notion of association was elaborated after the two English revolutions in the context of a political and moral question. How can individual happiness and the common welfare be associated? Or, in other words, how to reconcile the personal and common interests when the society is now conceived of as an association of individuals, and no longer as a social hierarchy corresponding to God's will, as in feudal society? This question is not raised as long as individual good and social harmony are conceived of as being in mutual accord in a natural order corresponding to God's will. Political utopias used such explanation during the revolutionary period (Gerard Winstanley) (3). From these utopias there arose the principle of the "greatest felicity for the greatest number" as being the natural order; from this order, humanity had departed because of the original sin, but would be able to come back by relying on Reason, as Christ in every human being, so that God's Kingdom be realised right on earth, here and now.

But how to maintain this principle outside of the influence of such ideologies? In particular, how to maintain this principle if, as Hobbes held it (while philosophically elaborating the revolutionary experience), the individual passions and egoïsm are the essential or even only motives of human action. Reconciling the private and common interest in order to progress towards a supreme Good cannot but involve understanding and reason in a completely new way. If there were innate principles of understanding, prior to experience, which

would lead human beings in the direction of that supreme Good which they cannot but wish to reach, how can we explain rationally, and without involving the original sin and its consequences, that they fail to do it? The general answer to that question si given by the application of the empirical postulate and by the critique of understanding its leads to.

According to Locke, was we have seen, we must appeal to reason to discover and the preserve those "connexions" of ideas which he calls "natural", while on the contrary we must discard the "connexions" of ideas which are contingent and are the causes of human madness (Locke, 1690, II, XXXIII, 5).

Similar ideas were put forward by Gay in 1730: Why are men unable to reach a general agreement if they all have pleasure for their goal and virtue as their rule? The answer was that pleasures are idiosyncratically associated to their causes; therefore every one believes that he is driven by a unique passion, unrelated to that of any other person. Thus associations, and therefore the thought process, disguise as idiosyncratic pleasure what is really a part of the common wealth.

It is about twenty years later that Hartley introduced his novelty. Instead of functionning as an impediment in the articulation between pleasure and happiness, or as an aberration, the proper mechanism of association, as defined by Hartley, always reinforces this articulation (Hartley, I, I, II,9). And, at the same time, it is made the general mechanism of thought. That coincidence enables us to formulate the hypothesis that associationism could be made a general law of thought only from the moment it ceased to be a principle of error and pathology and became, on the contrary, a factor contibuting to felicity. But, in order to reverse the articulation between ethics and associations in such a way, Hartley must rely finally on divine providence. Hartley's commitment to Christian optimism drives him to look for laws of nature whose mechanisms cannot but, in the long run, bring humanity back to the paradise lost. The laws of association, that is to say the laws of thought, as laws of nature, cannot but have "a tendency to reduce the state of those who have eaten of the tree of the knowledge of good and evil, back again to a paradisiacal one" (Hartley, I, II, 14, IX). His demonstration of this point relies on an implicit postulate: sensible pleasures are intrinsically more numerous than sensible pains: "Let the letters a, b, c, d, e, etc...

represent the sensible pleasures; x, y, and z the sensible pains, supposed to be only three in number; and let us suppose all these, both pleasures and pains, to be equal to one another. If now the ideas of the sensible pleasures and pains be associated together, according to all possible varieties, in order to form intellectual pleasures and pains, it is plain, that pleasure must prevail in all the combinations of seven or more letters; and also, that when several parts of the complex pleasures are sufficiently united by association, the pains which enter their composition will no longer be distinguished separately, but the resulting mixed and complex pleasures appear to be pure and simple ones, equal in quantity to the excess of pleasure above pain, in each combination. Thus association would convert a state in which pleasure and pain were both perceived by turns, into one in which pure pleasure alone would be perceived" (Hartley, ibid.). Hartley does not give any justification for his postulate but we can see that, given his "calculus of pleasure and pain" based on association, it follows directly from the fact that, if he did not make it, he would have been compelled to conclude that God has not displayed things in such a way that paradise could be realized on earth despite the existence of evil. His followers had to face a major difficulty: that of finding an equally satisfactory laïc principle which could be substituted for the involvment of divine providence.

Devine providence and Bentham's calculus of pleasure and pain

Here precisely, appeal has been made to Bentham and to his own "calculus of pleasure and pain", based on assumptions different from Hartley's. But, as we shall see, an examination from the perpective of Freudian work enables us to see that this was made at the price of a misunderstanding of Bentham's theoretical positions. This misinterpretation involved omission of what remained unsystematised in Bentham's work, that is to say, what we shall call his psychology, which corresponds to what Ogden called his "theory of fictions".

It may be recalled that Bentham was first a jurist and a legislator, not a philosopher. His concern was with the question how the law could permit private pleasure and common interest to be reconciled. He supported the thesis of the artificial identification of interests. Thus was he in the continuity which Hobbes and Hume.

For Hume, the state of nature should be considered as a "pure fiction". Only if we had an unthinkable quantity of goods could justice become useless, because only in that case could envy be suppressed. Justice is what results when virtue becomes identified with the longing for universal happiness, as a condition of individual happiness (Hume, 1739, III, II, 2).

For Bentham, as for Hume, vice and virtue are not natural; they do not result from the direct appreciation of reason. They are perceived as such only according to rules of justice which are artificial, which are the outcome of conventions between men necessitated by their limited generosity and their egoïsm. Bentham acknowledged that he borrowed the principle of utility from Hume, but he added:

> The difference bertween Hume and me is this: the use he made of it is to account for that which is, I show what ought to be (Letter to Dumont, 6.10.1822, Mss. Univ. Col. no. 10, in E. Halevy, 1901, 1, p. 6-7).

For Bentham the existing laws are effectively artificial, but they establish conventions which are not in agreement with reason in that they do not serve the general interest but only that of some people: politicians, clergymen, lawyers. Legislation should then be reformed by basing its decisions on reason, that is to say on an equitable appreciation of pleasure and pain, which supposes that they are calculable. This is possible only if we make a distinction between ideal happiness and actual happiness. The latter only, which is the sum total of pleasures and pains that can be compared to each other at a given moment in the circuit of exchange, can be the basis of a calculus: the quantity of pleasure I expect from an object or a service is the one I expect from the money I am ready to pay for it, or from the service I am ready to give in exchange. The common actual happiness is then considered as the sum total of avaible pleasures which, as in the market and its laws, is the totality in reference to which pleasures and pains can be determined by definite ratios. This calculus measures utility which, given that definition, is not a psychological entity. Thus, from the point of view of equity and reason, an action can be appreciated only in consideration of its consequences and not of its motives. A single motive can lead to different consequences. It is only habitually that a motive, having usually either good or bad consequences, can be itself qualified as good or bad, and that, not systematically. Thus there can be no measures of motives since there is no constant relation between them

and the calculable consequences of the associated acts. It is here, at this point, where a hasty reading influenced by objectivism and behaviorism will make it most easy to miss what is specific about Bentham's position. Such a reading was already begun by Bentham's pupils, especially by J. Mill. And as a matter of fact, Bentham's positions were open to such interpretations (4). For this reason, we must make a deeper analysis of Bentham's conceptions.

Motives and fictions

Bentham does not neglect motives; they can even help with a rational distribution of pains, especially in attenuating circumstances. But Bentham's notion of motives cannot be understood without reference to his notion of fiction.

At first glance it may seem that Bentham defines fictions in connection with the intent to abuse. Bentham speaks of fictions in reference to juridical categories as grounded on fictions and not on utility. In that sense he gives to the notion a connotation of abuse of discretion. He also uses it to speak about politics and religion. In such a case fictions can be related to Hume's "artifice" (5), adding to it a disparaging connotation.

Given that, we may believe that fictions, for Bentham, have been created by politicians, clergymen, and lawyers in order to abuse; it would thus be possible to eliminate them by revealing this intent. But this is not the case:

1) Bentham distinguishes carefully fictitious entities from nonentities which are separated by "a difference such, that when, with property and use, the one is, the other cannot be employed" (Bentham, in Ogden, 1932, p. 16).

2) He distinguishes between those fictions which are used with the intent to abuse --those of politicians, clergymen and lawyers-- from those which are used without this intent: the fictions of the poet, the logician and even the psychologist: The fictions of the logician "have had neither more nor less than the carrying on of human converse"; the fictions of the poet "are pure of insincerity". And:

> In the mind of all, Fiction, in the logical sense, has been the coin of necessity in that of poets, of amusement in that of the priests and the lawyers, of mischivous immorality in the shape of mischievous ambition; and too often both priest and lawyers have framed or made in part this instrument (id., p. 18).

Moreover, if Bentham makes a distinction between real and fictitious entities, it does not mean that fictitious entities are non-entities, or have no reality at all. Why should we not consider that fictitious entities are nothing but non-entities which can be detected and discarded as such?: "Answer - Altogether inevitable will this contradiction be found. The root of it is in the nature of language: the instrument without which, though itself it be nothing, nothing can be said, and scarcely anything be done". Fictitious entities are "... one of those sorts of objects which in every language must, for the purpose of discourse, be spoken of as existing -be spoken of in the like manner as those objects which have existence, and to which existence is seriously meant to be ascribed..." (id., p. 16). "To language, then -- to language alone -- it is, that fictitious entities owe their existence; their impossible, yet indispensable existence" (id., p. 15). The apparently contradictory aspects of Bentham's conceptions results from that fact that he distinguishes those "names of entities" which owe their existence to language only from those which have also an existence as "real beings".

Bentham developped his conception of fictions specifically in the direction of psychology:

> Of nothing that has place, or passes, in our minds can we give any account, any otherwise than by speaking of it as if it were a portion of space, with portions of matter, some at rest, others moving in it. Of nothing therefore that has place, or passes, in our mind, can we speak or so much as think, otherwise than in the way of "Fiction" (id. p. 17, Bentham's emphasis).

This can be applied to the consideration of motives. A motive is nothing but a kind of "disposition", a "fictituous entity", introduced for the purpose of discourse to express what is supposed to be a permanent disposition of the mind of a man. Thus a motive has two aspects: a real one, in relation to the real event it is connected with, and a psychological, fictitious one, whose value cannot be determined exactly because it has only a usual, non-systematic connection with the consequences of the act. This is the essential aspect of the motive, its psychological aspect. It is a "fictitious entity", an "ideal being", "passion" or "affection", which is supposed to induce the human mind to take a particular direction.

The reinterpretation of Bentham's utilitarism and the elimination of fictions

While elaborating his laws of "association of ideas", James Mill apparently proceeds as Bentham did in his dichotomizing classification of motives. But what was for Bentham only a classification becomes for J. Mill a genetic principle of construction, a way of chaining ideas one to the other for making complex ideas from simple ones. Bentham always tried to return empirically from fictions to reality in order to formulate applicable laws. But he also always maintained the theoretical difference between real objects (in the circuits of exchange) and fictions as motives of actions. Hence, his psychological principles were always on the side of fiction.

To the contrary, J. Mill tried to establish a constant connection between real and fictitious aspects of the motives of action in order to reduce all fictitious elements to real ones.

This was very well pointed out by Halevy, whose work on Bentham is to-day still a major reference. Let us quote him:

> What Bentham called the motive in the literal meaning of the word, is what James Mill called the desire (...). What happens to the motive in the figurative sense, the affection or passion which is supposed to push to action? Bentham just noted the existence of two different acceptances for a single word. But James Mill did not see an insuperable gap between these two ideas: there is, according to him, between the real event and the 'fictitious entitious entity' just the difference between the simple and the complex. From desire to affection, from affection to motive, fom motive to disposition, the principle of association gives the transition: for James Mill, defining the psychological phenomena is to generate them, or construct them (Halevy, 1904, p. 285 -our translation).

Thus, if we follow James Mill:

1) the scientific description of reality can be progressively reduced to psychological components; and,

2) consequently, the gap between fiction and reality, between ideal and actual happiness, disappears.

For Jeremy Bentham, the calculus of pleasure and pain enables us to reach the maximum "sum total" of possible common happiness, that is, the common good or wealth. His entire analysis is based on this assumption. But as this calculus applies to actual happiness, it excludes that happiness which is wished or desired, which can only be individual and fictitious. On the contrary, for J. Mill, the calculus applies to ideas as they are associated in the process of thinking;

and thinking is governed by the utility principle which is the measure of individual pleasure. The association of ideas spontaneously performs the calculus, but it is now transposed to an individual level. The calculus is interiorized and, realising the maximum of utility, thus takes the place of the divine goodness in Hartley's theory of association of ideas.

We can describe James Mill's psychology as the result of the combination of two elements:

1) Hartley's associationism, making of the laws of the association of ideas the laws of the thought process but without any consideration of their material status. With materiality removed, there was no longer a theoretical entry for divine providence, as there had been in the Newtonian perspective. Thus Mill's associationism eliminated both God and materialism in one fell swoop.

2) A transposition of Bentham's calculus of pleasure and pain:

- from the consequences of the act to motives as the determination of acts,
- from the distribution of the common good or wealth to the individual expectations of pleasure.

In these transpositions the pleasure-pain principle becomes a rule of rational thought, driving the individual spontaneously towards the increase of his own good. This mechanism thus takes the place of the divine goodness in Hartley's associationism. But it is at the cost of dropping the originality of Bentham's fiction.

With this elimination, there ocurred a complete reversal of perspectives, implying a thorough change of the criteria of truth. For Bentham, it is only the hypothesis of materiality that fictions implied which was deceitful. On the contrary, for the psychology which follows the ideas if J. Mill, it is the fictitious which becomes deceitful and unreal, and the materiality of the thought process framework becomes, at least ideally, the criterion of truth. From Bentham's own point of view, in other words, everything occurred as if psychologists had really taken "the scaffoldings for the building" (Freud, 1899, VII, II).

Towards a psychology of progress and a psychological explanation of progress

In conclusion, we may underline that this combination of Hartley's associationism with Bentham's utilitarism, as used by J. Mill and his

followers, enabled the constitution of a psychology of progress and moreover a psychological explanation of progress. The process of thought is no longer conceived as following the flow of fictions, but rather the harmonious order of the scientific description of reality at the very moment when humanity is beginning to look towards science as the source of its salvation and happiness. Later on, psychology would call upon physiology and biology to ground the pleasure-pain principle in a material framework. It must be noticed here that this appeal to biology places this later one, in fact, just where Hartley had situated divine providence. Such a biological interpretation of the pleasure-pain principle made it impossible to understand why S. Freud opposed the principle of pleasure to the principle of reality. It furthermore obscures the fictional status of Freud's theoretical constructions (including biological metaphors).

Our comparison between the works of Hartley, Bentham and James Mill characterizes the ethical and epistemological shift which has driven to modern psychological associationism. It must be stressed that this comparison is made possible only by the systematization of the status of fictions worked out by Freud through his study of dreams, lapsus, symptoms... In Bentham's work, fictions gain positivity because they are recognized as being necessarily involved in every language process. But this positivity is won by the exclusion of fictions from the scope of the pleasure-pain calculus and the field of rationality it defines. Because of this exclusion, the truth value of fictions remains undetermined. They are "indispensable" but they escape to the field of rational thought identified with that of calculability. On the opposite, it is this exclusion which for Freud gives truth value to fictions. If, for him, certainty can be grounded on fictions, it is because they are close to what can be called the "real" from the point of view of the unconscious: dreams, lapsus, symtoms are fictions, and they escape to what falls under everyday life rationality just as unexpected events in observational or experimental designs (6). This is where rational thought fails and where its truth value has to be re-elaborated.

So, fictions must be studied for the truth they support and not for the future they promise. This is why Freud, on the contrary to Bentham, never really turned from fictions to such utopias as that of the "panopticon". He declared "impossible" the art of governing, although, can we add by reversing Bentham's proposition,

"indispensable". The difference between Freud and Bentham gives a precise meaning to the choice made by psychology through the rejection of fictions. Its aim was to make psychology able to contribute to a panoptical organization of society under the flag of utopias, the common denominator of which is the promise of a happy future. In this we can recognize the political side of the belief in a possible paradisical state of affairs on earth which has dominated the rise of capitalism as well as that of modern psychological thought. There, too, psychology appears as having forgotten Plato's dictum recalled by Freud at the end of The interpretation of dreams: "The virtuous man is content to dream what a wicked man really does".

Notes

1) During his 1959-60 seminar entitled The ethic of psychoanalysis (unpublished), Jacques Lacan argued that Bentham's utilitarism means a rupture in the ethical tradition originated in Aristotle; and that this rupture can be perceived only under the light of Freud's position on the same problems. Given the place attribued to J. Bentham in psychology by its historians, that affirmation looks at first glance paradoxical because it comes from a thinker who has always underlined the irreducibility of the gap between psychology and psychoanalysis. Such was the starting point of the following analysis (for further indications, see Lacan, 1975).
2) The metaphysical background of Newton's system has been thoroughly analysed. Newton's epistemological empiricism was not conceivable outside of his metaphysical and theological conceptions (on this question, see Koyré, 1957).
3) See Christopher Hill, 1972.
4) It must be recalled that only some texts were effectively edited by Bentham, that a great part of his work has been edited by his pupils (Dumont, J. Mill, Bowring), and that Bentham himself disavowed partly the result of their work. In fact, he said at the end of his life about Dumont: "He does not understand a word of my meaning" (in Ogden, 1932, XXIV-XXX).
5) David Hume (1739), Book III, part II, section II "...those impressions, which give rise to this sense of justice, are not natural to the mind of man, but arise from artifice and human conventions".
6) Lacan's comments on the convergence between the ethical and epistemological approaches can be synthetically summarized by the following question: Wat is "happiness" but the "happening" of the unexpectable?

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PURPOSE, FEEDBACK AND HOMEOSTASIS: DIMENSION OF A CONTROVERSY IN PSYCHOLOGICAL EXPLANATION

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"Homeostasis does not suffice"
(R. Arnheim, Entropy and art.)

Summary

At the beginning of the 1940s, two essays were published which were to become a real hallmark in subsequent speculation on teleological matters: one was by W. Ross Ashby (1940), the other by A. Rosenblueth, N. Wiener and J. Bigelow (1943). They both tried, independently, to give a new definition of adaptive and purposive behavior, so that this became synonimous with behavior controlled by negative feedback. They both gave rise to a cluster of theoretical investigations, which can be summarized in the following question: is it possible to extend the concepts and methods of mechanical (physical) explanation to psychological explanation, in order to explain human (and animal) purposive behavior? In this paper a reformulation of this question is examined as it was discussed in particular in a number of essays in the Psychological Review throughout the 1940s and the 1950s. The above question was now becoming a more precise question on the very nature of feedback and homeostatic mechanisms in psychology: are they really explanatory of adaptive and purposive behavior, or rather are they tautological re-formulations of older notions?

An other notion of adaptive and purposive behavior is also examined, which was developed in that very period by Herbert A. Simon.

In a 1940 paper, W. Ross Ashby pointed out an idea which was soon afterwards to become fundamental: the concept of "stable equilibrium" might be substituted for the concept of "adaptiveness" in analyzing animal and human behavior.

At that time, the concept of a feedback mechanism had not yet been explicitly introduced in psychological explanation, and cybernetics was not yet officially born. Nevertheless, physiological homeostatic mechanisms which regulate the adaptive behavior of animals and men were well known on the basis of the investigations by biologists as Claude Bernard, Jacques Loeb, Walter Cannon, Jacob von Uexküll and others (for further details see Cordeschi, in press). Uexküll's "function circles" between animal and environment were explicitly mentioned by Ashby as supporting his own thesis. In confirmation of the pionieristic character of Ashby's 1940 paper, one might observe that his speculations on stable equilibrium and adaptation were based on the mechanical analogy with those servomechanisms (as the thermostat) which would have become one of the central issues of the cybernetic explanation of adaptive and purposive behavior. In further confirmation of this point, the explicit operationistic and mechanistic character of Ashby's thesis should be pointed out. This aspect also would have become typical of the whole development of classical cybernetics. As Ashby says, changing the concept of stable equilibrium for the concept of adaptation would mean changing an operational concept for a "vague", "not quantitative" and "subjective" concept. In conclusion, such an operational concept "avoids all metaphysical complications of 'purpose'" (Ashby, 1940, p. 483).

Not by chance, both these pionieristic ideas in Ashby's paper are found in the 1943 paper by Arturo Rosenblueth, Norbert Wiener and Julian Bigelow on "Behavior, purpose and teleology", which is usually considered the manifesto of cybernetics (as it is known, this word was introduced by Wiener in 1947). In such a paper, the authors tried to give a more "restricted" (as they say) but hence operationally more "precise" definition of purposeful behavior. Such a definition should not imply "the vague concept of a 'final cause'", that is a typical concept of old teleology, as contrasting with old mechanism. Indeed, from the new mechanistic point of view, "the concept of purpose and of teleology, (...) although rather discredited at present, are shown to be important". Therefore, they conclude, "'teleology' was used as synonymous with 'purpose controlled by feedback'" (Rosenblueth, Wiener and Bigelow, 1943, pp. 22 and 23).

Perhaps a difference between Ashby's 1940 proposal and that by Rosenblueth et al. might be pointed out. Ashby seems to conclude that operationistic concepts might substitute for teleological ones, which

thus might be in a sense eliminated. Rosenblueth et al. stressed the significance of teleological terms, and the utility of retaining them once one can prove their operationality. But it seems rather a question of a diference of emphasis.

Anyway, for Ashby the more precise concept of equilibrium should be substituted for the vague concept of adaptiveness. However, Ashby puts himself the question whether the two concepts are always interchangeable. This - Ashby admits - is still an "open question", even if an useful "working hypothesis" (Ashby, 1940, p. 482-483). Thus, if one reads between the lines, Ashby seems to put the question if there is, in his own words, just a "striking similarity" (an analogy) between the "essential features" (i.e. the objective features) of the two mentioned concepts, or rather a strict synonymy (an identity) between them. Anyway, the operationistic power of this "translation" of concepts from the psychological to the physical (or the cybernetical) is stressed by Ashby in the field of psychiatry too. In a further paper especially devoted to this subject, Ashby so concluded:

> Here is a field that might well attract the research worker who would like to develop psychoanalysis by making its basic concepts less metaphysical and more biological. Cybernetics offers the possibility of a psychology that recognises to the full the richness and complexity of Man's strivings, while being based on concepts that share the objectivity and precision of physics (Ashby, 1954, p. 122).

Ashby's proposal on adaptive and purposeful behavior as well as Rosenblueth's et al. were largely developed, both in cybernetics and in psychology and philosophy of mind. The concepts which are the foundations of these authors proposal (homeostasis and feedback) influenced independently psychologists and behavioral and social scientists between the 1940s and the 1950s. Their elaborations of the concepts of feedback and homeostasis were contrasting each other, and often rather vague and unclear. And both misconceptions and corruptions of these concepts died hard.

Take for example R.C. Davis's analysis of these concepts. Davis (1958) thought that two meaning of the term homeostasis might be distinguished: (1) the tendency to stability or constancy of a system reacting to a disturbing force, and (2) the specific mechanism, i.e. the negative feedback which acts in order to minimize the change, when a change occurs.

In fact this is a quite trivial and misleading distinction. Indeed, the meaning (1) cannot be given to homeostasis, for the

simple reason that it is the old dynamic meaning of an immediate restoration of equilibrium without a feedback action during the time. In this case it is sufficient to have an input force and a reacting, restoring force, which is a function of the input force (think to the behavior of a disturbed elastic body, in Davis's own example). Such a confusion is really puzzling, if you consider that Davis's paper is published in 1958, after a lot of discussions on the subject among psychologists and behavioral scientists and several analyses from different points of view. It is our purpose to review some of those analyses and discussions in order to point out certain misconceptions of the use of the principles of homeostasis and feedback on the one hand, and some possible more fruitful developments of such principles on the other hand.

Thus, let's come back to the 1940s. Now we see John Fletcher who stresses the thesis of homeostasis as an explanatory principle in experimental psychology and personality theory in a 1942 paper. Fletcher was rather prudent, anyway: he rejected the ideas of building on such a thesis "a whole new system of psychology", or of simply adding "another technical term" to the "already podded glossary" of the psychologist, or of introducing such a term "from mere reasoning by analogy" (Fletcher, 1942, pp. 83 and 81). But when he ventured to enumerate certain instances in which some known but as yet not well explained mental phenomena could be better explained through the concept of restoration of equilibrium or homeostasis (such as color vision, after-image of motion, etc.), he observed:

> We, of course, do not know much about the nature of the original organic status, not precisely how it is disturbed, nor yet concerning the process of recuperation. Yet we cannot but assume the existence of such a status, the disturbance of the status, and a process which brings about its restoration (Fletcher, 1942, p. 84).

As J.R. Maze rightly pointed out, "the 'process' which restores the states is just the homeostasis itself. But of course homeostasis is simply the fact of restoration, the word is only another name for the restoring, and so it cannot be appealed to as something which explains that restoration. (...) The alleged explanations then merely restores the fact to be explained" (Maze, 1953, p. 406). Presumedly, here Maze means 'explanation' as 'discovering of causes'. So homeostasis does not appear to him as the cause of the steady states, but as the effect of (at least) two causal sequences, opposing each other. We might conclude that Maze refuses the notion of homeostasis

as an end of organism as a whole, because in this case one might be tempted to describe it through a teleological language - a language which in his opinion reduces homeostasis to a not-explanatory and teleological concept, when not to a merely metaphorical one. In homeostasis there is no special case of intention or knowledge which cannot in principle be reduced to causal sequences of opposing forces: Maze rejects the idea that organism as a whole possess "a special kind of causality, namely, a teleological causality" (Maze, 1953, p. 407). In his opinion, the very existence of a physical analogy, as for example the thermostat, is an evidence for his own thesis: in fact, in order that such a mechanism works, "no foreseeing of ends is necessary" (ibid., p. 408).

Compared to Rosenblueth's and others' point of view, we might conclude that Maze's approach to teleology and purposeful behavior was radically different. Instead of describing the thermostat (or analogous servomechanisms) with an intentional terminology, Maze points to describe intentional behavior of organisms in terms of mechanical causality. However, we should stress the one-sidedness of Maze's examples of servo-mechanisms. In his criticism of teleological categories, he mentions only examples of servo-mechanisms without "predictive behavior", in Rosenblueth's et al. (1943) terminology. So, if in the case of thermostat and other mechanisms which Ashby (1950) called "self-directing", speaking of "foreseeing of ends" might be considered not necessary, in the case of "self-correcting" mechanisms (such as the classic self-aiming guns and missiles) to avoid such expressions seems to be hardly possible.

Christian Weber was perhaps thinking to something like that, when he seems to distinguish homeostasis from servo-mechanisms. He says that homeostasis does not explain conscious goal-seeking human activity. In fact, "homeostasis storage provides found of energy, but does not specify the directions of expenditure". On the other hand, he adds without further details, "in this respect servo-meachnisms lie closer to [human] behavior" (Weber, 1949, p. 239). Now in his opinion consciousness and goal-seeking activity upset homeostasis: as soon as homeostasis is restored as a state of equilibrium, it is broken down, because consciousness is the search of always new equilibria. As he picturesquely says:

> The [the ideal ends of consciousness] are really not ultimate goals at all, but only directions in which behavior can move (...). They provide for an ever-

> shifting equilibrium (...) attained only to be lost again (...). The attainment of stable equilibrium is another name for dying (Weber, 1949, p. 239).

If - in Weber's quoted words - "in this respect servo-mechanisms lie closer to [human] behavior", one might interpret Weber's last statement as supporting the idea that servo-mechanisms might provide an analogy of purposive activity directed towards ideal - thus not actually (physically) existent - ends or goals. Weber's statement is really a vague one, but its core idea seems to be interesting, if one considers Richard Taylor's objections to Rosenblueth's et al. 1943 paper (see Taylor, 1950). According to Taylor, once the equivalence between purposive behavior and negative feedback is assumed, it is not possible to explain that kind of goal-seeking activity which produces directions, or goals by definition never attainable - for example, ideal or not physically existent ends or goals. In fact - Taylor says - from the point of view of servo-theory it is required that the goal is some existing object or feature in the environment - an object which emits physical signals in order to have a feedback correlation.

Now the admittedly vague thesis stressed by Weber seems to be very close to the best rejoinder to the objections à la Taylor. Such a rejoinder was implicitly given by Donald MacKay. According to MacKay, the goal of purposive activity is defined as an abstract state, not as a physically existent object. So, in order to have a feedback controlled action, "there is no question of the goal emitting physical signals" (MacKay, 1952, p. 63; for further details see Cordeschi, 1986).

An other convincing objection to the use of the principle of homeostasis is raised by Weber. He maintains that homeostasis does not explain the existence of those features of human behavior which are not directed to the need satisfaction, namely those "priceless unessentials" stressed by Cannon himself (see Weber, 1949, p. 237). In order to contrast analogous criticisms, Ross Stagner was forced to conclude that, for example, climbing a mountain (sure not a need directed or survival directed activity) might be anyway an equilibrating activity, because "the humiliation of losing valued social status may be more upsetting"! (Stagner, 1951, p. 11.) In such a case, it is difficult not to be in agreement with Bertalanffy's conclusion that "the principle of homeostasis has sometimes been

inflated to a point where it becomes silly" (Bertalanffy, 1968, p. 210).

Anyway, homeostasis in his restricted notion of a mere restoration of equilibrium in the presence of a disturbing condition sure is not sufficient to explain motivated and intentional activity on the whole. Even if one is not interested to grasp by homeostasis (or servo-mechanisms) ideal ends, as directions towards which behavior moves without never attaining them, one should at least elaborate such concept in order to explain the power of organisms to anticipate the attainment of an actual end or goal.

Fletcher himself stressed this problem when he said:

> Physiological homeostasis manifests itself only when the disturbing conditions are physically present, whereas, thanks to the influence in the mental equation of that unresolved and possible unresolvable, factor of consciousness, it is possible to demonstrate the tendency to maintain status at the mental level of behavior even in anticipation of the presence of disturbing conditions (Fletcher, 1942, p. 83).

Clearly, homeostasis was here conceived by Fletcher as something more than a mere physiological self-correcting mechanism for the restoration, for example, of blood temperature.

Thus, Ross Stagner (who believes in the unifying, if not explanatory, power of homeostasis in psychology) speaks about "dynamic homeostasis". When is motivated by dynamic homeostasis, "the organism responds selectively to external stimuli". "Thus, the organism does not simply restore the prior equilibrium. A new, more complex and more comprehensive equilibrium is established" (Stagner, 1951, pp. 9 and 6). Accordingly it would seem that, in Stagner's opinion, the extention of homeostatic principle to psychology takes place at the level of the organism as a whole. At this level homeostasis is not the mere recurrent restoration of a pre-existing state, as it is prevalently at the physiological level. So, in replying to Maze's criticisms, Stagner points out the essential role of the perception of goal objects in the motivated human behavior: this "has far more the characteristic of an active searching for desired object (...) than the characteristic of a blind searching to struggling impulses" (Stagner, 1954, p. 206).

Nevertheless, when the concept of homeostasis is applied to organism as a whole, one might further wonder whether it might be extended to those cases in wich equilibrium and goals are states or

relations which were not experienced before: for example, in the cases of learning and individual development.

In studying "need" as a teleological concept, C.M. Mace tries to deal with this problem when he proposes to extend the concept of homeostasis "not only to the restoration of an equilibrium, but also to the discovery of new equilibria", in order to explain how "new goal-directed systems come into beings", as in the mentioned example of learning or in the case of individual creativity and problem solving (see Mace, 1953, pp. 202-205). Mace does not say how homeostatic explanation of those complex human activities is possible. And it is dubious whether the concepts of homeostasis and feedback can go beyond the explanation of some simple forms of learning.

We see that the notions of homeostasis and feedback, in order to be of some use, were submitted to more and more extensions, to such a degree that they lose their original meaning, becoming, on the other hand, more and more metaphorical. In particular, as we see from the above analysis, one ends up denoting various different processes with the word "homeostasis": (1) the end-restoration of a previous disturbed state; (2) the forestalling action which uses clues in order to anticipate the disturbe itself; (3) the selective establishment of more comprehensive equilibria and end-states; (4) the highly selective processes leading to alternative and never experienced goals. Indeed, when it is so extended, the concept of homeostasis becomes a mere metaphor rather than an explanatory tool of mental phenomena.

In order of being actually explanatory of purposeful behavior - and of the various mental activities mentioned by Maze -, the concepts of feedback and homeostasis did not need generic (metaphorical) extensions. On the contrary, they need a radical re-examination or, in a sense, a restriction: they need to be supported with other actually explanatory mechanisms.

Now some kind of feedback, which makes the organism-environment relationship rationally flexible, is no doubt present in purposive activity - i.e. in goal-seeking or motivated behavior, in learning, in problem solving, etc.; the end-attainment may no doubt be considered as an homeostatic activity; and such an activity is no doubt a process of adaptation to external environment. But in such a way only a very general level of description of the organism or system has been individuated, which allows to predict the activity of the system as concerns just its gross features: i.e. those features which are

relevant for establishing an automatic (rational) connection of actions (or means) to goals (or ends). In other words, the system shows not a generic flexibility, but an end-directed flexibility ensured by a feedback dependence with a changing environment. So the system rationally reacts to the environment - then its behavior is roughly predictable if we assume some general principle of rationality, which we might identify here with homeostasis principle.

Anyway, it was in the very period we are speaking about that the inadequacies of such a description of end-directed activity were pointed out. Between the 1940s and the 1950s it was Herbert A. Simon who stressed the relevance of introducing an other explanatory principle for predicting adaptive and end-directed activity: the principle of bounded rationality (see Simon, 1947; 1956). Simon's analysis had important and well known developments, both in social sciences and in computer science. As far as we are concerned with it here, we want to point out that Simon's description of purposive system goes beyond the generic notion of end-directed (feedback) flexibility. In fact, Simon's purposive system assumes more than the simple rationality of the system and the specification of its goals, in order to explain its behavior. These two factors are, in a sense, external to the system, and they are not sufficient, in general, for predicting the purposive behavior of the system, when this is relatively complex, or is in a complex situation. To exemplify with Simon's words:

> If we have a rat in a very small maze, with cheese at one branch point, and if we give the rat plenty time to explore, we can predict where he will finally go without any very deep knowledge of rat psychology. We simply assume that he likes cheese (a given utility function) and that he chooses the path that leads to cheese (objective rationality). If we now transfer the rat to a maze having a number of pieces of cheese in it, but a maze that is several orders of magnitude larger than the largest maze he could possibly explore in a rat's lifetime, then the prediction is more difficult. We must now know how a rat solves problems in order to determine where he will go. We must understand what determines the paths he will try and what clues will make him continue along a path or go back (Simon, 1963, p. 711).

The lesson we get from Simon's analysis is, shortly, the following. Purposive or adaptive behavior entails that the system must be flexible toward external environment, in order to attain or maintain a certain relationship with it. In the case of a changing environment, this flexibility is assured by homeostatic or (negative)

feedback meschanisms. This was "the classic insight of cybernetics", as Allen Newell more recently says (see Newell, 1980, p. 147). However, when we say that a system has the ability to fit itself to environment (or that exists an adaptation or equilibrating process), we have not yet explained how the system uses such an ability in fitting to environment. For example, we said nothing about its strategies of adaptation. The analysis of the way in which the system uses its adaptive ability (i.e. the ability to attain or to maintain a relationship with environment) shows that the system employs rational choice criteria which are bounded by its internal limits (which are limits of memory, etc.). Those limits strongly influence, for example, the ability of the system of learning or selecting clues in external environment in order to attain a goal, and in general the ability of foreseeing the features of a purposive behavioral sequence.

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COGNITIVE STYLES - ANALYSIS OF A PSYCHOLOGICAL CONSTRUCT FROM A HISTORY OF SCIENCE POINT OF VIEW

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Summary
Whereas in the 'Sixties research on cognitive styles flourished, interest in more specified investigations on the many dimensions of cognitive styles has decreased in recent years. However, in current textbooks, handbooks, and reviews cognitive styles are often described as a well established construct and the many critical theoretical articles and empirical refutations of numerous aspects of cognitive styles are often neglected. This paper reviews different attempts to provide definitions and centers on a historical evaluation of the theoretical construct of cognitive styles by analyzing the origin of the field and its socio-political implications. It is shown that cognitive styles do not represent a valid and useful psychological construct but are more of an attempt to overcome behaviorism in psychological theory discussion.

A typically shared characteristic of nearly all studies on cognitive styles is the mere empirical basis; theoretical explanations are rare or not given at all. With reference to cognitive styles in concept formation Kagan, Moss & Sigel (1963, p. 74) even speak of random discovery and concede that their first empirical studies were only "trial-and-error". Later, Kagan tried to build up an integrative theoretical view (cf. Kagan & Kogan, 1970); however, the results were too diverse and contradictory to allow theoretical integration. The same may be said for all other areas of cognitive styles (cf. Nickel, 1984; 1985; Petzold, 1985a).

1. Definitions of cognitive styles

A homogeneous definition incorporating the different cognitive styles has not yet been given. Goldstein & Blackman (1978) refer to S-R-models and S-O-R-models and discriminate between various cognitive styles by viewing cognition as the intermediating O-variable. They

list several definitions of cognitive styles and maintain that within all of these definitions structure is emphasized in contrast to content.

Harvey defines cognitive styles as "the way an individual filters and processes stimuli..."; Bieri denotes them as a "process of information transmission", Messick speaks of "consistent patterns of 'organizing and processing information'"; Coop & Sigel stress "consistencies in individual modes of functioning in a variety of behavioral situations" and refer to similarities in Gestalt psychology ("silent organisation"); Tolman uses the term cognitive maps, Bartlett and Piaget use the term "Schemata".

In Germany, Fröhlich approached the definition of cognitive styles by first investigating the indicators used for relative global particularities of experience and behavior. He then looked for criteria to give a general description of cognitive styles: "In general, with the term 'style' one denotes modes of self-expression or experience which are peculiar to a person or a group and can be described by use of special criteria. Such criteria for 'style' are the forms of behavior and/or experience, in the extreme the one unic form" (Fröhlich, 1972, p. 1024). On such a basis Frölich then agrees with the definition of cognitive styles given by Lazarus (1969, p.81):

> ...'cognitive style' refers to stable ways in which the person thinks, perceives, and looks at the world and his relationship to it. Implied here is that there are many individual differences in the ways these cognitive functions are carried out, and that these are stable over many different activities and situations. In other words, it is not the intellectual task or situation alone that determines the form of perception and thought, but stable properties of the personality.

However, in his conclusions Fröhlich left unanswered the question as to whether the contruct of cognitive styles is useful: "It is largely a matter of risk whether one gives statements on the level of 'cognitive styles' before having gained proof of the generality and persistence of stable properties and the thereby connecting empirical-rational test of the representativity of the selection of stimuli and situations" (Fröhlich, 1972, p.1025).

By far the most differentiated and detailed discription of various cognitive styles has been given by Messick (1976 & 1982) who specifically incorporated the differentiation of styles and abilities,

> Cognitive styles appear to serve as high-level heuristics that organize more specific strategies, operations, and propensities - often including abilities - in such complex sequential processes as problem solving and learning... They function in part as controlling mechanisms... ...abilities are enabling variables in the sense that they facilitate task performance in specific areas, whereas cognitive styles are organizing and controlling variables (Messick, 1982, p.10).

Accordingly, cognitive styles are rather general constructs, and "cognitive controls" on the one hand and "stylistic abilities" on the other hand are two subordinate concepts. This relationship is, however, reserved by Amelang & Bartussek (1981, p.385) who determine styles as one of several classes of cognitive controls.

This shows that in spite of decades of research psychologists have not even developped convergent views as to the basic dimensions and definitions. One should rather assume that this lack of commonly shared definitions points to the fact that the construct of cognitive styles is not a very useful one.

2. Different approaches

It is not easy to pinpoint the beginning of research on cognitive styles. The studies by Ash & Witkin (1948) and Witkin et al. (1954) are often regarded as the start of the cognitive style research and they were followed up on the basis of a revision of the concept during the 'Sixties. Contemporaneously Klein & Schlesinger (1951) introduced the concepts of "levelling" and "sharpening". They did not, however, apply the term cognitive style but spoke of perceptual behavior in a way similar to Witkin. Klein then introduced the term "cognitive style" which incorporated the different "cognitive controls".

Although the German psychologist Frölich (1972) did not provide a historical evaluation in the first German review of cognitive styles, he already referred to several approaches German Holistic and Gestalt psychology. Witkin's dichotomy "field dependence / field independence" is to be seen in the tradition of the older heuristic typology of perceptual and thinking processes within the poles "analytic vs. detaillistic" and "synthetic vs. holistic" (cf. Frölich, 1972, p.1020). In the early 'Fifties within the so-called "New-Look" research on perception the construct of cognitive styles becae fashionable and incorporated into personality theories.

A comparatively useful discussion of the history of the concept of "cognitive styles" has been presented by Vernon (1973). In older

psychological dictionaries the only possible reference he found was to Adler's "style of life". According to Vernon the term cognitive style was first used by Klein & Schlesinger (1951) and given special emphasis by Gardner (1953). He sees forerunners in German typology, which he had critically reviewed in 1933 (together with G. Allport) and he especially sees parallels to the typological concepts of Kretschmer, Sheldon, and Jung. As to the coherence of cognitive types and personality he refers to the German psychologists Meumann, Külpe and Jaensch. Finally, Vernon mentions the philosophical "Idealtypen" of Dilthey, Jaspers and Spranger (cf. Vernon, 1973, p.130).

In his review of cognitive styles the East-German psychologist Clauss also gives historical references and sees parallels with German typology (Jaensch, Kretschmer, Jung, Spranger). Whereas Vernon critically rejects such typologies, Clauss reaches the conclusion that such typological concepts are verified by new empirical research basing this statement on Witkin's notion that his view of perceptual processes in terms of styles was forced on him by the kind of empirical data he collected (cf. Witkin et al., 1954, p.505).

The construct of cognitive styles cannot be reduced to the older typologies but has been developed with reference to different theories in recent decades. It is situated at the cross-roads between General Psychology, Personality Psychology, Developmental Psychology and Social Psychology.

In General Psychology, Gestalt psychologists pose the question of special organisational structures of human perception. Whereas Koffka (1935) saw visual stimuli as decisive in spatial orientation, Gibson & Mowrer (1938) held the view that vestibule and somaesthetic stimuli were more important. Witkin has taken up both of these viewpoints and integrated them in his concept of field dependence/independence. According to his theory, field dependent persons are influenced more by visual stimuli whereas field independent persons rely more on information from their own body.

A rather different approach on cognitive styles was taken by Adorno et al. (1950) with their psychoanalysis-based study on the authoritarian personality. This study then opened a discussion of cognitive styles in Social Psychology.

In Personality Psychology it was Kelly (1955) who examined the coherence of personality traits and general cognitive dimensions. The term "cognitive style" is often said to have been introduced by him.

From a psychoanalytic point of view Klein & Gardner developed the concepts of cognitive styles and cognitive controls by regarding cognitive controls as regulating principles between drives and environmental conditions.

In Developmental Psychology the construct of cognitive styles became fashionable in the United States at the time of the discussion of preschool reforms. In this context, Witkin investigated the coherence of field dependence and educational styles (Witkin et al., 1962). The group of developmental psychologists of the Fels-study-project then began to focuss on cognitive styles in concept formation (Kagan, Moss & Sigel, 1963) and also developed the cognitive style "reflexivity vs. impulsivity" (Kagan et al., 1964). Numerous studies covering these areas were conducted during the 'Sixties (cf. Kagan & Kogan, 1970) yielding a multitude of heterogenous and even contradictory results (cf. Nickel, 1984; Nickel et al. 1985).

3. Cognitive styles and the "cognitive revolution" in psychology

The construct of cognitive styles has been fashionable in many subdisciplines of psychology, especially in the United States, and it is interesting to ask why it received so much attention in the 'Sixties. First of all, there is an interesting remark by Jerome Kagan who saw the interest of developmental psychologists as a result of the controversy between behaviorism and the Piaget-school. In the early 'Sixties research on cognitive processes was dominated by a "dichotomy of behaviorism and French structuralism" (Kagan, Moss & Sigel, 1963). From a modern viewpoint Amelang & Bartussek (1981) also refer to the growth of cognitive style research as an outcome of the growing interest of formerly behavioristically oriented psychologists in questions concerning structures of cognition.

Is the construct of cognitive styles still being followed up or has it vanished in current theoretical discussion? First of all, it seems to be the case that in spite of much criticism, cognitive styles are recognized as an acknowledged construct in Personality Psychology (cf. Pervin, 1978). Goldstein & Blackman (1978) even state a renewed interest in the subject, the only shift in emphasis being from content oriented to more structural style-concepts. This theoretical shift in approach has been affirmed by Kogan (1983) who emphasized that within the established field of cognitive style research

the older trait-tradition has been replaced by a new open concept with reference to information processing theories.

Thus, we may draw the conclusion that psychological interest in the construct of cognitive styles is a result of the general cognitive shift in psychology, which took place in the 'Sixties in the United States and a little later in Europe. Within this theoretical re-orientation with its rejection of radical behaviorism and renewed interest in structural processes, the construct of cognitive styles can be described as a new theoretical instrument applied to refute the strict S-R model psychology of behaviorism. This view is supported by the subsequent lack of interest in following up the typological model construct of cognitive styles. More recently ecological models are being developed (cf. Witkin & Berry, 1975) and new concepts within information processing theory are being introduced in order to substitute the construct of cognitive styles such as the concept of metacognition (cf. Weinert & Kluwe, 1984).

4. Some socio-historical aspects

A historical view of the research area of cognitive styles should include an analysis of the social factors which influence the genesis of new research programmes (Petzold, 1985b). Here, Pervin points to the analogy of stylistic constructs and technical improvements:

> Whereas the brain and the nervous system were formerly viewed as a telephone switchboard mechanism that connected stimuli with responses, now there is an emphasis on a very complex computer capable of performing varied operations. The 'cognitive revolution' emphasizes that between stimulus and response is a complex network of pathways and processes that facilitate the person's adaptation to a changing environment (Pervin, 1978, p.118).

A real analysis of the social factors should be concerned with the political and social situation at the time of the rise of the construct of cognitive styles in the United States. This was the time of the cognitive shift in psychology, and in Developmental Psychology in particular, which took place against the backdrop of demands for preschool reform and the funding of the "head-start-programme" initiated in the United States after the so-called 'Sputnik-shock'. The urgently needed reform of preschool curricula forced psychologists to overcome behaviourism and to find other concepts. Structuralist theories became fashionable and S-R theories were rejected leading to a rediscovery of Piaget. Within this controversy many psychologists tried to find an integrative viewpoint and one of these integrative attempts was based on a renewal of old typologies

within the concept of cognitive styles. The most interesting observation is that most of the researchers at that time did not acknowledge these theoretical connections but stated that the new view of the construct of styles was forced on them by their empirical data. The current discussion, however, shows that the empirical results were inhomogenous and contradictory and could not verify any kind of general cognitive typology. For this reason the construct of cognitive styles faded out in theoretical discussion after it had served its primary goal as an instrument for overcoming behaviorism in cognitive psychological research.

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TARDE, DREYFUS AND PUBLIC OPINION
HIS CONTRIBUTION TO CROWD PSYCHOLOGY AND ITS BACKGROUNDS

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Summery

The paper forms part of an ongoing series of studies on the origins of crowd psychology.

Tarde played a key role in its development. He inspired the first Italian as well as the first French author into publishing a complete book on this subject; Sighele (1891) and Fournial (1892). After a brief exposé of Tarde's life and work, the paper focuses on the four articles on crowd psychology he wrote in the course of the last decade of the 19th century: Les Crimes des Foules (1892), Foules et Sectes du Point de Vue Criminel (1893). Le public et la Foule (1898) and L'Opinion et la Conversation (1899). It shows that an important transformation took place between the former and the latter two.

In his early articles, for example, he leans heavily on a largely neuropsychological notion of "hypnotic suggestion" . This means that it is hard to extend its explanatory value beyond physically assembled (or biologically related) groups. This dead end in the development of crowd psychology is clearly visible in the works of Le Bon.

In Tarde's later articles, however, he implicitly shifts to more sociopsychological notion of "interaction".This means that a more convincing extension can be made to social movements and public opinion. This new opening in the development of crowd psychology is further exploited in the works of Park and other U.S. sociologists on "collective behavior".

The transformation is obscured by the fact that Tarde holds on to his central notion of imitation, although a different meaning is attached to it in the early and in the late 1890's. The paper furthermore suggests that the transformation may have been related to the Dreyfus Affair, which made it clear to liberal intellectuals that the public was similar to, as well as different from, the crowd.

The French criminologist Gabriel Tarde (1843-1904) was a many-sided but oft-forgotten man. In recent years he has been "rediscovered" as a founding father of social psychology and other disciplines (Rocheblave-Spenler & Milet in Tarde 1973; Lubek 1981, a.o.). This article focuses on his role in early crowd psychology, and claims that his true contribution to this field has been frequently misunderstood.

Crowd psychology in the larger sense came to cover a wide variety of phenomena. The main ones, however, can be brought under three headings: crowds in the narrower sense, social movements, and opinion currents. Or, as one definition of "collective dynamics" has it: "those patterns of social action, that are spontaneous and unstructured, in as much as they are not organized and are not reducible to social structure" (Lang & Lang, 1961, p. 4).

Although these different phenomena have certain elements in common, their study is mostly shattered over several disciplines today. Among these, two important traditions stand out. Within psychology, the main emphasis is on crowds and mostly on their regressive aspects. Their explanation is sought in de-individuation: a "process in which a series of antecedent social conditions (lead) to changes in perception of self and others, and thereby to a lowered threshold of normally restrained behavior" (Zimbardo, 1969). There is a sociological tradition too, however, which is often overlooked by historians of psychology. Here the main emphasis is on social movements and opinion currents, and often on their progressive aspects. Their explanation is sought in interaction, which helps alternative norms emerge when conventional patterns are experienced as unsatisfactory for some reason (Turner & Killian 1972 a.o.).

It is the claim of this article, that Tarde's application of "imitation " to early crowd psychology should not so much be understood as a prelude to the first, but rather to the second perspective.

Crowds and social movements

He never wrote a systematic monograph on this subject. He only wrote four different articles of some 30, 60, 90 pages, in which he surveyed different forms of collective behavior. They were tentative rather than exhaustive, which makes it interesting to consider both their disjunctions and continuities. Tarde himself linked the first and the second of the articles in his book "Essais et mélanges

sociologiques" (1895). But he excluded the first and included the second with the third and the fourth in his book "L'Opinion et la Foule" (1901). This may already be a hint as to his ambivalent feelings concerning the ideas put forward in the first article, which is often singled out as his major contribution by historians of crowd psychology.

This first article was "Les crimes des foules": a report presented to the Third International Conference on Criminal Anthropology in Brussels in August 1892, subsequently published in the Archives d'Anthropologie Criminelle (Vol. VII, pp. 353-386) and also in the proceedings of the conference (Actes du Troisième Congrès, pp. 73-90). At the time, the subject of crimes committed by or in crowds had already been in the limelight for some time. The revolutionary events of the preceding century had inspired a number of literary and historical authors to elaborate on the nature of the mob (Van Ginneken, 1985a).

In the second half of the 1880's various lawyers and physicians had taken up the issue in lectures and papers, among others in Italy (van Ginneken 1985b). In 1891 the young Italian criminologist Sighele (a student of Lombroso's associate Ferri) had published a first book on this subject: "La Folla Delinquente" (translated in French as "La Foule Criminelle", 1892). It focused primarily on criminal complicity in the crowd. In 1892 the French physician Fournial (a student of Tarde's associate Lacassagne) had published a second book on it: "La Psychologie des Foules". It focused more on the psycho-physiological changes in a crowd. Tarde's report on "Les crimes des foules" was closely related to both books (Barrows 1981).

In view of the following, several observations can be made on Tarde's report. Firstly: it mainly dealt with crowds in the narrow sense, of a large number of people temporarily assembled in one particular place. Although related phenomena were occasionally mentioned, he hardly elaborated on them. Secondly: although he stuck to his "newly discovered" explanatory concept of imitation, he applied it very much in the more fashionable sense of suggestion. The famous debate between Charcot's Paris' Salpétrière School and Bernheims Nancy School on the nature of hypnosis and hysteria had led to the contention that under certain circumstances the higher levels of psycho-physiological functioning (conscious attention and will) disintegrate, and lower levels (unconscious emotions and reflexes)

take over. Tarde's explanation of crowd behavior was very similar. But thirdly: just like Sighele and Fournial before him, Tarde was unable to develop this explanation into an elegant theory along the lines of early dynamic psychology. Only Le Bon really succeeded in doing this, a few years later. Thus , there there is no reason to consider "Les crims des foules" his major contribution to crowd psychology.

The second of Tarde's four articles on crowd psychology was "Foules et sectes du point de vue criminel", published in the Revue des Deux Mondes somewhat more than a year later, on November 15, 1893 (pp. 349-387). It started out with the same basic ideas, but developed them somewhat further. Already in "Les crimes des foules", he had briefly explored the similarities and differences between crowds and sects (section I). This time, the emphasis shifted entirely to this latter subject, and to the nature of criminal gangs., political clubs and social movements in general.

Once again, however, this was hardly revolutionary. The comparison had been made before, both by himself and others. The other criminological founder of crowd psychology Sighele, for example, was also studying "Le società criminali" was he called them, in a book on "La coppia criminale" (1892) translated in French as "Le crime à deux" (1893), later followed by "La Delinquenza Settaria" (1897) translated in French as "Psychologie des sectes" (1898). Yet, there was an important difference.

The theories of the others and especially of the medical founders of crowd psychology, Fournial and later Le Bon, relied heavily on suggestion as a psycho-physiological phenomenon. This very much limited the range of their explanation to groups which were in physical contact. Tarde had done the same in his first article on "Les crimes des foules". But although he stuck to suggestion in this second article on "Foules et sectes du point de vue criminel" he reverted from a rather psycho-physiological interpretation to the more socio-psychological interpretation which he had already outlined before. Whereas his explanation of crowd behavior in terms of imitation was hardly more convincing than an explanation in terms of suggestion per sé, his explanation of sects was much more so. It took some time, however, before he himself realized how close he had come to a conceptual breakthrough.

The first social psychology

After Tarde's second article on crowd psychology, there was a five year pause before he wrote the third. This half decade was characterized by important developments, both in his own thinking and in crowd psychology in general. As far as the latter was concerned, the major event was the publication of Le Bon's "Psychologie des foules" (1895), which became an instant best-seller. Although its claims to priority were immediately contested by his predecessor Sighele (Van Ginneken 1985c), it should be acknowledged that it was a much more elegant formulation of the supposed effects of hypnotic suggestion on crowds, which seemed to result in the submergence of the individual in the collective (that is, in de-individuation). If this approach appeared convincing in its application to physically assembled groups, though, it was much less so in the extension of this explanation to physically dispersed groups. That was stretching the argument a bit far, even if it took some time before this became clear to everyone.

Meanwhile, Tarde was completing his own system of thought. One should not forget that "Les Lois de l'Imitation" (1890) was only the first of several volumes. It was followed by "La Logique Sociale" (published somewhere between 1893 and 1895) and "L'Opposition Universelle" (1897). After this, the entire 1340-page trilogy was summarized in a slender 170-page booklet called "Les Lois Sociales" (1898). In that same year another collection of articles was published as "Etudes de psychologie sociale"; the first time ever a book title included this new term. From now on his public advocacy of a socio-psychology and/or a psycho-sociology played an ever larger role in his life and work.

What was his system of thought then? It consisted of several basic propositions, which were applied to a wide variety of themes. Firstly: the "real" is only a sample of the "possible", which comes about through "contingency". Secondly: this "real" is structured by "resemblances" and "differences". Thirdly: these "resemblances" and "differences" must be studied in their "repetition". Fourthly: this "repetition" takes on a different form in physics ("vibration"), in biology ("generation") and in sociology ("imitation"). Thus, the basic social process is imitation. However, it embraces several elements. First there is an invention, which leads to imitation by some, and opposition by others, which in turn leads to another

invention (or adaptation), when the whole cycle starts anew. Thus there is a constant confrontation of new fashions and old customs.

In psychology, the two basic elements of personality are beliefs (the cognitive component) and desires (the affective component). Within society, there is a constant "collective meditation" going on. Whether specific patterns are imitated or not, is dependent on certain conditions. On the one hand, there are logical laws: people try to judge the truth of beliefs and the value of desires on the basis of existing norms. On the other hand there are extra-logical laws: imitation depends on the credibility of a belief and/or the credulity of a person; on the desirability of a value or the servility of a person. This also results in certain hierarchical principles: imitation will spread from high to low, from inside to outside, etc.

All these processes can be most easily illustrated by water metaphors. Stones thrown into a pond will make ever wider circles, which will at a certain point start to interfere with each other. A river will follow its bed, but wear out dikes or pound on dams until they crumble one day. Tarde applied this model of the development and spread of new social phenomena to language, culture, religion and many other areas. Although he continued to refer to imitation as a result of some form of suggestion, it was clear that he was on to something entirely different now.

Public and opinion currents

In 1898 Tarde suddenly returned to the subject of crowd psychology, and wrote a third article on "Le Public et La Foule", which was published in the Revue de Paris (July 15-August 1). Already in the second article, he had briefly hinted at other phenomena, beyond social movements (Section IV a.o.). Now he elaborated on these phenomena, which he called opinion currents. He thereby hit on a very important idea. The concept of opinion currents pointed to real similarities between assembled crowds and "dispersed" crowds. However, it was necessary to clarify these terms.

The word crowd, he said, "has a prestigious attraction for the contemporary reader, encouraging certain writers to use this ambiguous word to designate all sorts of human groupings. It is important to put an end to this confusion, and notably not to confuse the crowd with the public, a word in itself subject to various interpretations, but which I shall attempt to define precisely. We speak of the public

at a theater, the public at some assembly, and here public means crowd. But this is neither the sole nor even the primary meaning, and while the importance of this type of public has declined or remains static, the invention of printing has caused a very different type of public to appear. One which never ceases to grow and whose indefinite extension is one of the most clearly marked traits of our period.

There is a psychology of crowds; there remains to be developed a psychology of the public, understood in this other sense as a purely spiritual collectivity, a dispersion of individuals who are physically separated and whose cohesion is entirely mental... They are all sitting in their own homes scattered over a vast territory, reading the same newspaper. What then is the bond between them? This bond lies in their simultaneous conviction or passion and in their awareness of sharing at the same time an idea or a wish with a great number of other men... I therefore cannot agree with that vigorous writer, Dr. Le Bon, that our age is the "era of crowds". It is the era of the public or publics, and that is a very different thing" (Clark 1969, pp. 277, 278, 281).

He then continued to compare the two phenomena. The differences between crowds and publics, Tarde said, were manifold. Crowds were a natural, primitive phenomenon. They were always temporary and limited in size, but one could only belong to one at a time, and one tended to belong to them totally. This explained their greater intolerance, he said. Publics, on the contrary, were a cultural, civilized phenomenon. They could by very durable and almost unlimited in size, but one would mostly belong to several at a time, and one tended to belong to them only partially. This explained their greater tolerance.

Of course, this tolerance was relative too. "The man of one book is to be feared, it has been said; but what is he beside the man of one newspaper! This man is each of us at heart, or nearly so, and therein lies the danger of modern times... Admittedly, it often happens, that an overexcited public produces fanatical crowds which run around in the streets crying "long live", of "death" to anything at all. In this sense the public could be defined as a potential crowd. But this fall from public to crowd, though extremely dangerous, is fairly rare..." (Clark 1969, pp. 283, 281/2). After comparing crowds and publics that were expectant, attentive, demonstrating or acting, Tarde concluded that the new forms of collective behavior

were a definite improvement over the old forms (although he insisted on the superiority of individual thought and action).

Somewhat more than a year later, he wrote the fourth and largest article on crowd psychology "L'Opinion et la Conversation", which was published in the same Revue de Paris (August 15-September 1).
It prolonged the argument along the same lines. The existence of different opinion currents, he said, resulted in the occurrence of a general Opinion (with a capital O): "a momentary, more or less logical cluster of judgments which, responding to current problems, is reproduced many times over in people of the same country, at the same time, in the same society... The transformation of an individual opinion into a social opinion, into Opinion, is due to public discourse in classical times and in the Middle Ages, to the press in our own time, and at all times, most particularly to... private conversations" (Clark 1969, p. 300).

Tarde had thus completed his cycle of studies on crowd psychology by formulating a new approach to the concept of public opinion. It is true that this concept had already been postulated by a number of political thinkers. But this had been limited to an elite, long before the rise of mass society. Therefore, few people had thought about the social processes involved. It is also true, that the other early crowd psychologists tried to extend their analyses in the direction of opinion currents, too. Sighele included a chapter on "L'Opinione publica" in a later version of his early book (chapter four in "L'Intelligenza della folla" 1903 which was split off from what was titled "I Delitti della folla" in 1901), Le Bon had included a part on "Les Opinions et les croyances" in his early book, and later wrote a separate book with this title (1911). But whereas Le Bon's formulations were more convincing as far as the crowd in a narrower sense was concerned, Tarde's formulations were more convincing as far as public opinion was concerned.

A different crowd psychology

At this point it is necessary to take a closer look at the road which Tarde traveled in his four articles on crowd psychology, and at the new perspectives which he discovered. Three developments stand out. The first one is related to the phenomena he dealt with. While Tarde started out with an article on the crowd in the narrower sense, he thereupon extended his analysis to sects or social movements, and

finally even to publics or opinion currents. He began with large groups that were physically assembled, moved on to groups in which there was only intermittent personal contact between members, and then to large groups that were physically dispersed.

Thus he was the first crowd psychologist to deal with the three main categories of collective behavior separately and in succession, and to systematically explore the similarities and differences between them. Although Le Bon pretended to deal with all three phenomena in his "Psychologie des Foules" as well, he never properly distinguished them, and therefore came up with too general explanations - which could to a certain extent be applied to crowds but not so easily to publics.

This points to a second important development in Tarde's thought. He stuck to his explanatory concept of imitation all along, and always linked it to the concept of suggestion. In "Les Lois de L'Imitation" f.e., he stayed in line with then-fashionable ideas, and compared it with "somnambulism" and later with hypnotism (Tarde 1890, 1979, pp. 82/3, 95, etc.) In the first article on crowds, he interpreted it in the same limited psycho-physiological sense as Le Bon. He approvingly quoted Fournial's claim that crowds are never rational but rather emotional or even reflexive, that they are never "frontal", hardly ever "occipital" but rather "spinal" beings (p. 76). Le Bon (pretending not to know Fournial's book with the same name) claimed that 'cerebral" life vanished in the crowds, and that "medular" life predominates (p. 9).

As we proceed through Tarde's articles on sects and publics, however, we see a gradual change in the way in which he uses the concept of suggestion. In the latter article, for instance, he contrasts "suggestion in proximity" and the "action of the look of others", with "suggestion at a distance" and the "thought of the look of others" (Tarde 1901, 1904, p. 5/6). This brings us to the question whether he really meant that the same kind of imitation was going on in both cases.

It is always hazardous to try to fathom the meaning of a word in a different era. Sometimes a classical author uses the same word as we do but means a different thing. Sometimes he uses a different word but means the same thing. In this case Tarde does not make it particularly easy for us to decide. At no place in his voluminous works on imitation does he give an unequivocal definition of his central

concept of imitation. He often limits himself to a comparison with other phenomena, like a "photographic imprint" (Tarde 1901, 1904, p. VIII).

Most historians of psychology have tended to interpret Tarde as if he meant a rather passive and unilateral process. Occasionally, however, specialists have noted that he really meant the active and reciprocal process - with is not called imitation but interaction today (Clark 1969, p. 66; Lubek 1981, p. 368, a.o.). Several historians of sociology agree (Coser 1971, p. 153; Szacki 1979, p. 252 a.o.).

If this is correct, it also becomes clear why Tarde left his first article on the crowed out of his book on "L'Opinion et la Foule". This first article was much too close to the ideas of Le Bon and his predecessors on a collective regression to an irrational level of psychological functioning and the fusion into some kind of "mental unity" on this basis. The three other articles, on the contrary, had an entirely different emphasis, as Tarde made clear in the 1901 preface to this collection.

"L'expression psychologie collective ou psychologie sociale est souvent comprise en un sens chimérique qu'il importe avant tout d'écarter. Il consiste à concevoir un esprit collectif, une conscience sociale, un nous, qui existerait en dehors ou au-dessus des esprits individuels. Nous n'avons nul besoin, à notre point de vue, de cette conception mystérieuse pour tracer entre la psychologie ordinaire et la psychologie sociale - que nous appellerions plus volontiers interspirituelle - une distinction très nette. Pendant que la première, en effet, s'attache aux rapports de l'esprit avec l'universalité des autres êtres extérieurs, la seconde étudie, ou doit étudier, les rapports mutuels des esprits, leurs influences unilatérales et réciproques - unilatérales d'abord, réciproques après" (Tarde 1901, 1904, p. V).

It was precisely this interactionist approach which enabled him to deal convincingly with crowds as well as social movements and opinion currents. It also enabled him - and this is a third essential development which should be identified - to surpass the completely negative view which Le Bon had of mass phenomena, and to propose a conditionally positive view on some of them. Tarde realized that certain forms of collective behavior were not only inevitable side-effects of society, but were even essential to its growth.

Some backgrounds of the breakthrough

In the course of the 1890's, Tarde gradually extended his ideas from crowds to sects and social movements, and then to publics and opinion currents. Although he stuck to imitation as the underlying process, he tended to interpret it more in the narrow sense of suggestion in the case of assembled groups, and more in the wider sense of interaction in the case of dispersed groups. This also permitted him to get away from an all negative value judgment on collective phenomena, and to admit to positive aspects in some cases.

One interesting question is, whether elements in Tarde's life or in the social context may have contributed to these breakthroughs.

During most of his adult life, Tarde was a local judge in a far-off province. It had remained relatively undisturbed by the Franco-Prussian War, the Commune Revolt, and similar threats to national integrity and social stability, which led to an obsession with cohesion and solidarity among the Parisian intelligentia. Tarde in contrast continued to think in terms of individualism, liberalism and a belief in spontaneous progress which was considered increasingly outdated by many contemporary compatriots, although some of it survived in the Anglosaxon world. It was precisely this "eccentric" position which enabled Tarde to overcome some of the limitations of French sociology of the day, and to exert a considerable influence on subsequent American developments.

This is only one element, however, contributing to Tarde's "discovery" of social interaction and public opinion. Other elements were related to current events in the fields he covered. He was a criminologist interested in social processes, and increasingly in political ones in particular. It were precisely new types of "political crime" which helped him to elaborate his alternative view of social processes.

The sudden interest in crimes committed by or in crowds in this whole period had very much been inspired by incidents accompanying the strikes and demonstrations during the rise of the workers movement in the 1880's, which in turn resulted in the reestablishment of the Socialist International in 1889 and the beginning of the First of May parades in 1890. The elites were particularly impressed by the fact that vast masses of workers turned up in the streets of dozens provincial towns and capital cities all over Europe on the same day

every year. This was a major background of early crowd psychology, including of Tarde's first article of mid 1892.
In his next article, Tarde reflected on the links between crimes committed by crowds and crimes committed by sects. In many cases, he said, the instigation to attacks on persons and property comes from criminal gangs, social movements, or political clubs. During his review of this question, however, he was forced to acknowledge that this relation could take various forms. The "suggestion" was often propagated by an organization, as in the case of the various socialist parties. But sometimes it was not all, as in the case of various anarchist groups.

This became increasingly clear when a "terrorist campaign" developed in the course of 1892. Its initiator, Ravachol, who was arrested after committing several bomb attacks in the spring of that year, at first just seemed an isolated madman. But after that, his actions triggered an avalanche of thousands of letter threats. They were followed by a bomb attack on the office of a major mining company in November, and another one on the house of representatives in December. Throughout the next two years a permanent succession of threats, attacks, arrests, and executions filled the newspaper pages. Thus, when Tarde discussed the crimes of sects in 1893, he dealt extensively with this phenomenon, and with the fact that it was not an organized group but rather an opinion current which perpetuated it.

Almost simultaneously there was another series of events which prompted him to think about the power of opinion, but in an entirely different manner. From the autumn of 1892 until the summer of 1893, a major political scandal gradually unfolded, which shook public confidence in the republican regime. It turned out that those who prepared to dig the Panama Canal had bribed at least a hundred parliamentarians and a similar number of journalists to advocate laws beneficiary to their big company and detrimental to small stockholders. The affair underlined the pivotal role which the press had come to play in the political life of the country.
Both episodes of anarchist violence and political scandals had occurred before (f.i. in the 1880's), and elsewhere (f.i. in Italy). This time, however, the situation was different. France was the first major country to have both a truly universal (male) suffrage and a large scale popular press. It was exactly in this same period (when

population stood at about 38 million), that a daily newspaper came to be read in the majority of households.

At this same time an affair unfolded which was to divide France for some ten years to come. Not on the basis of class or party, but simply on the basis of opinion. It was the Affair with a capital A. Which demonstrated the power of Opinion with a capital O, of Public Opinion, on an unprecedented scale.

Tarde and the Dreyfus affair

Early in 1894 the 50-year old Tarde was appointed director of criminal statistics at the Ministry of Justice, and moved to a house in Paris, close to both the Sorbonne University and the Ecole Militaire, which were to become the focal points of the agitation of the second half of the 1890's. After a rather secluded life he suddenly immersed himself in mundane activities, came to meet many well-known figures, and even received a Legion d'Honneur. After limiting himself to criminological matters and social processes in general, he suddenly turned his attention to politics in late 1894 (Favre 1983, p. 15). From 1896 on, he lectured on political matters at the Ecole Libre des Sciences Politiques. From 1898 on, furthermore, he lectured on social processes at the Collège Libre des Sciences Sociales. His earlier book on "Les transformations du droit" was followed in 1899 by a sequel on "Les transformations du pouvoir".

One element in his thinking on this "transformation of power" is of particular interest to us here. "L'évolution du pouvoir s'explique", he wrote that same year, 'par l'évolution de l'Opinion, qui s'explique elle-même par l'évolution de la conversation, qui s'explique à son tour par la série de ses sources différentes" (Tarde 1901, p. 134). Among these, the press was the most important. How did he reach this conclusion? The answer is that in the exact period when Tarde became interested in politics, there was one major affair which gradually came to occupy the centre stage.

Late in 1894 a Jewish officer with a German-sounding name had been arrested, accused of espionage. In 1895 he was convicted and deported to Devil's Island. In 1896, however, his family succeeded in persuading some journalists and politicians that he was innocent. In 1897 the public became increasingly divided. Gradually, in subsequent years, two blocs formed. On the one hand the anti-Dreyfusard bloc, in which the antisemite agitator Drumont and his newspaper La Libre

Parole payed a major role, alongside the Ligue des Patriotes and similar organizations. And on the other hand the (pro) Dreyfusard bloc, in which the radical politician Clémenceau and his newspaper L'Aurore played a major role, alongside the Ligue de Défense des Droits de l'Homme and similar organizations.

The highest excitement was reached in the six months between mid-January 1898, when the novelist Zola published his famous article "J'accuse" in the latter daily; and mid-July, when he was forced to flee to London to escape jail for slander. That same week, Tarde published his key article on "Le Public et la Foule". The affair took another turn in June and July 1899, when Zola returned from England and Dreyfus was sent back from Devil's Island to face a retrial. The next month, Tarde published his "L'Opinion et la Conversation". The next year, he was made a professor at the Collège de France, and a member of the Académie des Sciences Morales et Politiques.

Gabriel Tarde had never publicly taken political positions, and as a matter of fact it is hard to find any in his published works. His son Guillaume claimed, however, that the was an outspoken Dreyfusard and acknowledged a probable relation between the Affair and his articles on Public Opinion (conversation with this author, November 19, 1981). One study even claims that Tarde "rushed to sign a petition supporting Dreyfus's cause", but the source of this information is unclear (Barrows 1981, p. 197). More important however, are the references in Tarde's article on "Le Public et la Foule" itself. There are only two explicit references the Dreyfus Affair and two more to Drumont and antisemitism, like where Tarde speaks about the crucial role of a gifted writer in making manifest the latent feelings of certain parts of the population. Upon close reading, though, one can easily see that there are at least a dozen implicit references to the Affair, especially in the opening (pp. 5, 12, 13, 14, 15, 17) and closing (pp. 52, 57, 58, 59, 60, 61) sections of the article.

Many of the major ideas on the functioning and dysfunctioning of public opinion seem directly inspired by the Affair: how new objects of hatret are "invented" by certain journalists, how the crimes of crowds are often based on the feelings of publics, how entire nations may become dominated by intolerance, how the guarantee against such collective obsessions is formed by independent "intellectuals" (as a matter of fact the latter term was introduced during the Affair). The

Affair was truly the source of inspiration for his discussion of the role of public opninion in a democracy.

The influence of Tarde's ideas

In recent years there has been some reconsideration of the question why Tarde's ideas exerted so little influence on the subsequent development of psychology and sociology in France, and why they excerted some influence in the United States. In the latter case, however, the emphasis has been mostly on the use of his central notion of imitation by others, and even then in a very limited sense. Thus, ample attention has been given to the convergence of his ideas with those of Baldwin, one of the founding fathers of American psychology, and the residue they left in social learning theory. Ample attention has also been given to the convergence of his ideas with those or Ross, one of the founding fathers of social psychology and the residue they left in theories of social influence. Finally, some attention has been given to his friendship with Giddings, the dean of American sociology at Columbia University. Ocassionally others, such as Small at Chicago, are mentioned but little is made of Tarde's influence on them.

One could argue, though, that most historians of psychology have systematically tended to underestimate the fall-out of his thought in a major socio-psychological and psycho-sociological tradition, which helped to establish a large part of the field of attitude, interaction and communication research. Yet, there is a strong convergence between Tarde's thought and the Cooley-Mead-Thomas-Park traditon. Cooley appreciated Tarde's work to the extent that he "emphasized the social psychological foundations of society" (Coser 1971, p. 322). Mead owed "some of the focus of his work to Tarde" (Strauss in Mead 1956, 1977, o. XXIII).

The Chicago School's theory of opinions and attitudes bears a close resemblance to Tarde's theory of 'croyances et désirs', which also recognized the interplay between cognitive and conative factors in determining human behavior. The attitude concept was introduced in a book by Thomas and Znaniecki on the diffussion and change of cultural traits. The latter was to acknowledge later, that "a very consistent theory of this process was developed by Gabriel Tarde... its basic principles are still widely used, mostly without reference to their author" (quoted by Grupp, 1968, p. 344). Tarde also claimed

to have "demonstrated the masurability of individual belief and desire" (Clark 1969, p. 203). The first real attempt to measure attitude and social distance was undertaken by Bogardus, who considered Tarde "the chief founder of social psychology" (Bogardus 1940, 1949, p. 395). The idea had been given to him by Park who taught sociology at his alma mater Chicago.

The influence of Tarde, and particularly of his later crowd psychology, on Park has been recognized but vastly underestimated. Park's German doctoral dissertation, which has only recently been translated in English, not only drew heavily on Tarde but took its title "The Crowd and the Public" directly from his key article on this subject! Park and Burgess' famous "Introduction to the Science of Sociology" bore close similarities to Tarde's thinking. Not only was the central importance which their sociology gave to interaction similar to the central importance which Tarde's gave to imitation, but also was their central distinction between competition/conflict and accomodation/assimilation similar to Tarde's distinction between opposition and adaptation. Whereas their theories on collective behavior were built around intensified interaction which gave rise to an alternative definition of the situation, Tarde's theories were built around "collective meditation". The terminology was somewhat different, which may account for the neglect of this link, but the underlying ideas are strinkingly similar.

It was through this channel then, that some of the key ideas of Tarde entered in interaction and communication theory. Their traces can be easily be identified in the "crowd psychology" of interactionist sociologists such as Blumer, Shibutani, Lang & Lang, and Turner & Killian. Traces of Tarde can also be found in communication theory: such as the renewed emphasis on the mediating effect of "informal" communication (Tarde's emphasis on conversation), the discovery of the "two-step flow" of communication (Tarde's emphasis on hierarchical diffusion), the acknowledgment that "the medium is the message" (Tarde: "imitations are refracted by their media", quoted by Davis 1909, p. 98). etcetera.

It is only by identifying the "césure épistémologique" between Tarde's earlier and later articles on crowd psychology, and by acknowledging that his imitation concept took an interactionist twist, that we are able to see him for what the really was: a major

predecessor of opinion and attitude research, of communication and collective behavior studies.

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HYPNOTIC SUGGESTION AND CRIMINAL RESPONSIBILITY: FREE WILL AND INDIVIDUAL AGENCY IN LATE NINETEENTH CENTURY FRENCH PSYCHOPATHOLOGY

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Abstract

The following paper is part of an ongoing analysis of the employment of the hypnotic suggestion model as a general protype of social influence and behavior. This paper returns to the origins of the Nancy/Salpetrière debate of the late 1880's and early 1890's in order to highlight evolving conceptions of individual agency and cognition. In terms of the universality of hypnotic and suggestive influence, the rival theories of the Charcot and Bernheim schools are clearly delineated: the suggestion model held this influence to be a universal phenomenon, while the hypnosis model proposed that only a small, specific group of predisposed individuals were susceptible to hypnotic influence. Within this context, this paper examines in detail a late 19th century debate over the possibility of hypnotizing an individual against their will into the commission of a crime. This debate demonstrates the somewhat fluid boundary which separated the Nancy and Salpetrière models; while the Charcot group generally held that only those who were organically predisposed to criminal behavior could be coerced into crime under hypnosis, the Nancy group proposed that suggestive influence rendered the recipient an automat, and thus incapable of resisting coercive suggestions. Both groups tempered their positions, however, such that proponents of the hypnosis model acknowledged that social factors could moderate an individual's organic predisposition. Similarly, the supporters of suggestion felt obliged to admit that some individuals were able to resist coercive suggestions to which others would succumb. It is argued that this debate imparts an important new perspective to an understanding of individual agency and cognition and the nature of subconscious processes in this seminal model of social influence. In turn, it is argued that the examination of the foundations of the hypnosis and

suggestion models points to the need for a future reconsideration of the extension of these theories to other areas of social behavior.

And at the same time we must admit that very often, much more often than one would suppose, the controlling influence of an enchanting man, a fascinator of mobs, a sort of Donato on a large scale, exercises over his contemporaries an absolute rule which ought in great measure to cause the responsibility for the vices or the crimes bearing his effigy to be brought home to him. Again, it is only over nature's predisposed towards this contagion that it acquires this absolute power.(1).

Introduction

In a continuing analysis, previous research has focused upon both the genesis and the development of late 19th century French research into hypnosis and suggestion (2). Within this context, the application of this research beyond laboratory and medical practice in an attempt to explicate emerging social issues has been examined in some detail. Emphasis has been placed upon the emergence and eventual domination of pathological conceptions of the subconscious in French psychology, a trend which would culminate with the organic model of subconscious processes of the turn-of-the-century hypnosis school. The primary goal of this research program has been to emphasize how certain treatments of man and the subconscious, both methodologically and ideologically value-laden, have evolved through this seminal theory of social influence. Where they have been appropriated as an explanatory reference in other realms of social influence, such as the social psychology of the crowd, the hypnosis and suggestion models have also transmitted a narrowly-defined conceptualization of social influence and interaction. As a model of social influence, the manner in which the pathological interpretation of subconscious influence has engendered treatments of individual responsibility and cognition of action is deserving of further analysis. To accomplish this goal, the following analysis examines in detail a late 19th century French debate over the possibility of hypnotizing an individual into the commission of an antisocial crime, and explores the ramifications of this debate for evolving theories of social interaction.

Hypnosis versus Suggestion: Conceptions of the Individual

The debate over the degree to which hypnosis and suggestion provided a significant threat to individual liberty and free will has been generally overlooked by historians interested in the origins of this theory (3). I would argue, however, that the consequences of this conceptual dispute shed new light on the subsequent application of the hypnosis/suggestion model to the study of various instances of social interaction. This debate, which must be situated within the larger parameters of the Nancy/Salpetrière disagreement over the nature of hypnotic influence, revolved around an examination of the degree of automatism manifested by the hypnotized subject. As such, it was in turn symptomatic of more global French concerns with the possibility of free will and the responsibility of the individual in penal philosophy (4). The central question was thus: could an individual of high moral character (and thus not easily disposed to the commission of a crime) be made to break the law while under a somnabulistic trance? In other words, did the individual subject in hypnosis automatically surrender all moral responsibility, or were the moral restraints of the waking self also a factor in the successful resolution of a hypnotic suggestion?

As originally conceived by Charcot and his followers, the susceptibility of a subject to hypnotic suggestion was proof positive of a latent hysterical imbalance in that individual (5). The interrelationship between hysteria and hypnosis served as the conceptual base upon which the Salpetrière formulation of subconscious pathology was erected (6). Hysteria and hypnosis were believed to be closely related chiefly through the organic causal processes by which these states became manifest (7). When hypnotic suggestion was borrowed and adapted by the crowd psychologists of the early nineteenth century, the automatic association between hypnotizability and hysteria provided a powerful tool of invalidation of the social crowd, such that any activity in which hypnotic suggestion served as a plausible mechanism of influence automatically rendered the participants suspect. The employment of hypnosis and mental pathology to discount the emergence of socialist collectivities in the crowd psychologies of Gustave LeBon (8) and Gabriel Tarde (9) has elsewhere been documented (10). In a similar fashion, the association of hypnosis and hysteria has also been used in negative 'scientific' evaluations of the female (11) and psychical research (12).

It is not my intention to explore the full extent of the Nancy/Salpetrière debate and the determining role it played in the development of psychopathology in France. However, a basic understanding of the principal points of disagreement is necessary in order to fully grasp the significance of the debate over "les suggestions criminelles". I would like to specifically consider the degree to which the Nancy conception of suggestive influence of Bernheim, Liébault and their followers diverged from the Charcot association of mental pathology and organic causality. In an effort to explain the manner in which an individual developed a susceptibility to organic imbalance, Charcot and his followers offered the possibility that certain individuals, and only these individuals, demonstrated a specific predisposition to organic imbalance (and therefore hypnotic and hysteric susceptibility). In contrast, supporters of suggestion rejected the organic causality of the hypnosis conception, holding instead that suggestive influence was ultimately psychological in nature (13). In so doing, Bernheim and his followers would most definitely reorient the predisposition assumption of the Salpetrière, contending instead that suggestive influence was a universal phenomenon dependent upon situation-specific factors for successful resolution.

Hypnosis, Suggestion, and Criminal Responsibility

The narrower debate concerned with the possibility of criminal suggestion and hypnotic influence serves to highlight the fundamental points of opposition between these two competing theories of the subconscious. The Nancy position, fully in keeping with the Bernheim/Liébault emphasis on suggestion as a universal phenomenon, viewed the suggested individual as a specific type of automaton. The principal spokesman for the Nancy group, the Belgian physician Jules Liégeois, vigorously argued that the hypnotized individual became a full automaton under the influence of hypnotic suggestion, and consequently anyone could be hypnotized into committing a crime (14):

> Toute personne mise en état de somnambulisme profond devient, entre les mains de l'expérimenteur, un automate, tant sous le rapport moral que sous le rapport physique . . . . pour tous ceux qui en sont susceptibles, et par conséquent pour la société entière (15).

Support for the automaton position was provided by many of the leading exponents of the suggestion model, including Bernheim, Neaunis, and Liébault. The universal nature of this automatism was

however qualified by both Bernheim (16) and Liébault (17), who suggested that individuals under the influence of hypnotic of suggestive influence could none the less demonstrate remarkably strong resistance to those commands which were abhorent by their very nature (18). This qualification brought the Nancy school into a much closer rapprochement with the hypnosis theory of the Salpetrière. A brief overview of the extent to which this question was debated by prominent physicians and alienists of the day is provided by Bérillon (19).

In opposition to the Nancy school, the followers of Charcot held that only those who were predisposed to hypnosis could be hypnotized, and therefore only those showing an inclination of hypnotizability could be unwittingly coerced into crime. Since the predisposition to hypnosis indicated an inherently unstable mental condition, the hypnotizable individual could quite easily be predisposed to crime as well, considered another form of mental pathology. Somewhat surprisingly, the principal spokesman for the Salpetrière was the Swiss physician Joseph Delboeuf (20), whose previous declaration of support for the suggestion position in the larger hypnosis/suggestion debate had been a significant factor in the eventual ascendancy of the Nancy school. Delboeuf commenced the criminal suggestion debate in favor of the Nancy position, but gradually came to feel that only those who were predisposed to criminal activity could be made to break the law under hypnosis. His principal objection was directed towards the Nancy contention that hypnosis and suggestion evoked total automatism. Delboeuf felt instead that the automat individual could in no way be considered capable of the complexities of action and thought required in order to successfully commit most crimes.

Hypnotic Influence and 'la Vie Sociale'

This debate over criminal suggestion must be situated within the context of more global social preoccupations revolving around the question of individual responsibility and free will and the resultant implications for penal philosophy. Not the degree to which this debate would come to reflect general trends within criminal anthropology and penal philosophy, most importantly the contradiction between the Lombroso and Lacassagne schools over the primacy of hereditary, physiognomical versus social factors in criminal behavior (21). It is curious that historians have not previously remarked upon

the obvious correlations which existed between these two very different theoretical schisms, one emanating from within psychiatric circles, the other from criminal and social anthropology. In particular, there is a very direct correspondence between the Italian criminologists and the followers of Charcot in terms of the importance of innate, biological factors as determinants of abnormal behavior. Most importantly, for both Lombroso and Binet, these biological factors permitted the identification of afflicted (and therefore socially threatening) individuals.

Thus, Binet's refutation of the possibility of free will opens with a brief tribute to 'l'Ecole criminaliste italienne', who are given credit for divesting the ancient notion of 'le libre arbitre' of any rational credibility. The emphasis which Binet and Féré placed upon an organic predisposition in hypnosis had obvious similarities to the biological determinism of Lombroso and his followers. Specifically, Lombroso's conception of the occasional criminal (délinquant d'occasion) is strikingly similar to the Salpetrière description of the criminally predisposed:

> (le délinquant d'occasion) quand une force extérieure le pousse, (il) n'est pas retenu dans l'honnêté par une répugnance suffisante (22).
>
> Les hypnotisables ... ont en effet une constitution spéciale de leur système nerveux qui leur permet quelquefois de ne pas résister à certaines suggestions. (23).

Hypnosis first came to greater public attention in Europe through public séances in which large numbers of individuals voluntarily submitted themselves to hypnotic suggestion en masse (24). Prominent hypnotists of the day, led by Lombroso and the French physician Ladame, vigorously lobbied for the enactment of legislation which would prohibit hypnosis demonstrations outside of the laboratory (25). One of the first concerns these men signalled to political figures of the day was the vast potential for criminal coercion inherent in these demonstrations of mass hypnosis.

Various aspects of hypnotic suggestion as conceived at this time led to the extension of concern over criminal suggestion well beyond the isolated one-to-one interaction of hypnotizer and subject. Thus, one participant in a debate at the 1892 International Congress of Criminal Anthropology felt impelled to call attention to the suggestive capabilities of the popular press, which fostered the

reproduction of "certain criminal tendencies, robbery, assassination, poisonings, and suicides". (26). Another perceived social threat was representative of the organic bias of the hypnosis conception, wherein the hypnotic suggestion model was ultimately reduced to a medical metaphor (la contagion mentale - 27). Thus, participants in this debate considered the possibility that certain individuals could serve as refractors of hypnotic suggestion; it was argued that certain individuals could serve to focus hypnotic suggestion on those in the immediate vicinity (28). Judicial concerns were also expressed in regards to more private demonstrations of hypnotic coercion; several widely publicized cases revolved around private physicians taking undue liberty with hypnotized patients, who were of course almost exclusively female (some of these cases are mentioned by Laurence and Perry - 29). The most famous of these was probably the Bombard case, discussed in some detail by Tarde (30).

The various entreaties for an official response to the presumed threat of social influence in hypnosis demonstrations clearly involved considerations for the professionalization of psychiatric and psychological practice; those who called for a ban on public exhibitions of hypnosis were equally adamant in asserting that professional laboratory research, by accredited practicioners, should not suffer from this legislation (31). It is certainly conceivable that the debate over public hypnosis helped call attention to the combination of mass behavior and hypnotic suggestion which would eventually find a voice in the social psychology of the crowd. There is certainly evidence that Tarde, and most likely also LeBon, attended conferences where these issues were openly discussed (32). The publicity these issues attracted resulted in isolated instances of official suppression of public hypnosis demonstrations, most significantly by the Académie de Médicine de Belgique and the Parisian Cours d'Assisses (33).

Once the question of hypnosis and criminal suggestion had been broached by both alienists and judicial officials, a large number of experiments were subsequently developed in order to 'test' the possibility of hypnotizing an individual into criminal behavior. These experiments are typified by examples drawn from the last chapter of Le magnétisme animal (34), which were in turn preceded by an extensive article in Revue Philosophique (35). These experiments include instances in which a hypnotized patient was made to stab one

of the experimenters, doing so with such violence that the authors suggested others should refrain from replicating their experiments. Another example is provided by Delboeuf (36), who attempted to hypnotize a servant into shooting 'burglars', (in reality friends of the servant). The subject found it impossible to kill even under hypnotic influence, a result which was seen as supporting the Salpetrière contention that only certain predisposed individuals could be coerced in this fashion.

Predisposition and Criminal Suggestion

From this assertion, the debate then turned to determining the cause of this predisposition -- given that an individual who committed a crime was by a priori definition predisposed to doing so, to what extent should the individual be held responsible for their crime? Consider the conception of the individual which followed as a result of this predispositional factor. The emphasis on a deterministic process, whether innate or external to the social individual, validates the depiction of a certain segment of the population as incapable of rationally governing their own actions. Thus, this debate led naturally to the question of the social control of the individual, with the two opposing sides once again generally reflecting the Lacassagne and Lombroso positions on the ultimate determination of criminal tendencies. The position of Tarde is well situated within the Lyon school, of which he was of course a leading spokesman. Thus Tarde would suggest that the hypnotized criminal is no more responsible for their actions than are the mentally ill, since both conditions are ultimately reducible to an organic imbalance (37). Within this conception, the possibility of individuals who were susceptible to hypnosis being coerced into crime provided another reason for keeping all mentally disturbed individuals in isolation, a point which had been acknowledged by Esquirol as early as 1860:

> Les idiots n'ont rien à espérer de l'isolement; si on les renferme, ce n'est que pour les préserver des accidents auxquels leur état les expose, et pour empêcher qu'ils ne deviennent des instruments dont les malfaiteurs se sont servis quelquefois pour leurs tentatives criminelles. (38)

When it was extended from 'normal' to 'criminal' hypnosis, this predispositional factor was both maintained and strengthened. The experimental verification which was the cornerstone of research and theory at the Salpetrière served only to reinforce the conviction

that susceptibility to hypnosis, and thus criminally-oriented suggestions, was predetermined. In the end, the simple fact that an individual proved themselves capable of being hypnotized into crime was believed sufficient demonstration that they were predisposed to criminal behavior in general:

> Savez-vous si ces individus à qui vous inculquez l'idée de commettre un crime n'ont pas intérieurement une disposition à commettre ce crime, une disposition qui pourrait sommeiller inconsciemment pendant toute leur vie, si on ne les avait poussées en avant? (39)

In addition, subjects proving amenable to hypnotic suggestion were in a double blind, since it was argued that simply by submitting to hypnosis, the subject further weakened their ability to resist future suggestions. Moreover, in submitting individuals to hypnotic suggestion who were not previously predisposed, one risked the danger of weakening their will and resistance to such a degree that they would become one of the predisposed:

> Mais . . . ce serait une mesure très grave de soumettre aux pratiques réguliers de suggestion des enfants normalement constitués; on risquerait fort d'en faire des automates. (40)

The predisposition factor provides a striking indication of the somewhat permeable conceptual boundaries which separated both sides of the hypnosis/suggestion debate. On the one level, both schools advocated a deterministic approach to individual initiative and behavior which largely negated the possibility of free will. The hypnosis theorists contended that the source of this determinism was internal to the individual, and ultimately biological in nature. The suggestion group insisted instead that this determinism was largely external to the individual, and generally psychological in origin. But this dichotomy represents only the extreme positions of these two opposing models, and between these two conceptions lay a middle ground often occupied by adherents of both schools. Thus, for instance, Binet's forceful rejection of the possibility of free will, above and beyond the question of hypnotic influence, was tempered by the acknowledgement that the individual was partly responsible for the level of moral responsibility to which they had attained (41). Similarly, the conception of Liébault and Bernheim approaches the Charcot position in the admission that the universal susceptibility to automatism was leavened by an individual's moral temperament, since "on peut recontrer des résistances incroyables aux suggestions criminelles." (42)

Even Liégeois, the most adamant proponent of the universality of hypnotic suggestion, acknowledged that there was one group of individuals who should take special precautions against being hypnotized: those who dream aloud or talk in their sleep (rêvent à haute voix). With this conclusion, Liégeois would find himself in close agreement with Binet and Féré, who also felt that those who talked aloud in their sleep were a group at high risk:

> Que sont donc les personnes qui rêvent souvent à haute voix? Ce sont bel et bien des névropathes, et, autant que nous pouvons en juger par une pratique déjà longue de l'hypnotisme, tous les sujets hypnotisables offrent des stigmates de névropathie soit dans leur état actuel, soit dans leurs antécédents, et la plupart appartiennent par leur hérédité à la famille névropathique. (43)

It was Binet and Féré who would provide the most concise synthesis of the two different perspectives. While continuing to uphold the existence of a predisposition towards hypnosis, Binet and Féré disagreed with Tarde's contention that this susceptibility automatically rendered the individual not responsible for their actions while in the somnambulistic trance. In a separate study on moral responsibility and free will, Binet had already suggested that historical studies had established "des rapports multiples entre le crime et certain états pathologiques (folie, épilepsie, atavisme, dégénérescence)" (44). These factores helped establish the "fiction" of free will. And yet, he argued that the individual must accept a certain amount of responsibility for the level of social and moral responsibility to which they had attained, since "chaque fait grave de la vie passée de l'accusé pèse sur sa volonté présente et diminue sa part de liberté". (45).

It was therefore possible for Binet and Féré to argue that the predisposition to hypnosis was so pronounced that hypnotists could provide a valuable social service by identifying afflicted individuals in judicial court (46). This was balanced by the necessity of examining the responsibility of the accused for placing themselves in a position where the predisposition would become manifest. Why did they allow themselves to be hypnotized in the first place? Have they ever been hypnotized before? What is there about their character in general which made the events in question possible? In the final analysis, the synthesis proposed by Binet and Féré would founder upon its own inherent contradictions. Yet one can trace the beginnings of an interest in accommodating both social and innate factors in social

theory which would eventually evolve into a detailed analysis of suggestibility and social influence (47).

Hypnotic Suggestion as a Model of Social Influence

If the conceptualization of hypnotic and suggestive influence had remained strictly confined to the domain of mental pathology, the debate over criminal hypnosis would be simply an interesting early example of the entry of psychiatric theory and practice into the public forum. However, the model of suggestive influence has of course been extended far beyond the boundaries of psychiatry and psychopathology. Over the years, suggestion has evolved into a widely-applied yet ill-defined metaphor for a wide range of intra-personal influence in social psychology. In attaining this status, the original, clearly-defined distinctions between suggestion and hypnosis have largely become blurred. The appropriation and evolution of the hypnotic suggestion model as a unified theory has inevitably involved the acceptance of many of the preconceptions of the original formulations.

In many different areas, the model of suggestive influence continues to incorporate many of the often arbitrarily derived assumptions of the psychiatric hypnosis model. Primary among these is the conceptualization of the relationship between the source and the recipient of suggestive influence. As currently conceived in social psychology, this relationship is based upon an unequal power distribution between the dominator and the dominated (48). Moreover, the suggestive influence relationship maintains the fundamental distinction of the hypnosis model between those who understand the mechanisms of this influence (and are thus capable of eliciting its effects in others), and those who are not aware of these mechanisms (and thus are submitted to these influences).

I would argue that this relationship has a direct correspondence with the original conception of hypnotic suggestion, as practiced within psychiatric circles, wherein the hypnotizer dominated the will and behavior of the individual subject. It is within this question of domination and free will that the proceeding discussion of criminal responsibility may help to foster a needed reevaluation. Many of the preceding criticisms of the hypnosis and suggestion schools of mental pathology can be fruitfully applied to more current conceptions of

suggestive influence. Both are clearly based upon certain a priori assumptions concerning individual agency, cognition of action, and the degree of determinism inherent in both.

Perhaps the most evident legacy of the hypnosis model in the theoretical evolution of suggestive influence has been the assumption that these influence processes are carried out at the subconscious level (49). This is of course a direct reflection of the practice of hypnosis, wherein the hypnotic and suggestive commands served the explicit purpose of circumventing the conscious mind. More importantly, since the hypnosis and suggestion research of this time was primarily developed within the bounds of psychiatric practice, the subconscious became automatically associated with mental pathology. Hence the significance of the predispositional factor, wherein the ability to be hypnotized automatically implied a pathological aberration. This automatic association between hypnotic suggestion and mental pathology formed the foundation of the hypnosis model, and was also present to a lesser degree in the suggestion group's acknowledgement that some individuals are more predisposed to certain kinds of suggestive influence than others. The inexistence of the individual's responsibility for their own actions was not restricted to psychiatric practice, but has been explicated by means of the hypnosis/suggestion model.

Nowhere is this more evident than in the initial crowd psychologies of LeBon and Tarde, one of the first instances of the application of this model beyond the original, narrowly defined laboratory research programmes. The conceptual underpinnings of the hypnotic suggestion model, when applied to crowd behavior, can be seen to have served a specific ideological function. The initial crowd psychologies established to the satisfaction of their authors the importance of hypnotic suggestion in crowd behavior; indeed, hypnosis was considered a microcosm of all social interaction (50). In view of the general acceptance of the Charcot model by the crowd psychologists, the assertion that the socialist crowds were susceptible to hypnotic suggestion led inevitably to the claim that the individuals making up the crowd were therefore predisposed to criminal behavior. There can be no doubt as to the extent to which the Charcot model of hypnosis, as opposed to the Nancy conception, carried with it an implicit portrayal of the individual considered susceptible to hypnotic influence:

> Est-ce que chez tout le monde que la suggestion simple peut se produire? Non! Mais c'est principalement, exclusivement, dirai-je, chez des sujets de volonté faible, chez qui le système nerveux manque de force de résistance, des névroses, peut-être des dégénérés, des individus qui proviennent de familles offrant des tares constitutionnelles (51).

As has been elsewhere argued, claims by the early crowd psychologists that socialist crowds operated under the influence of the organic hypnosis of the Charcot school permitted these same kind of generalizations to be made about participants in contemporary crowds (52). Thus, the application of the hypnosis model implied that these crowds were composed of neurotics who were predisposed to criminal behavior. Perhaps even more importantly, this negative evaluation of the motivating forces in crowd behavior rejects the possibility of the participants being conscious of their own actions and responsible for their own behavior. The portrayal of crowd behavior based upon the application of the hypnosis model demonstrates the degree to which the complications inherent in this model accompanied hypnotic and suggestive influence as it was applied to other areas of behavior.

One could fruitfully make the same assertions about the evolution of the model of suggestive influence as a general theory of social interaction. By so doing, it may be possible to reorient many of the fundamental assumptions of a social theory which serves an explanatory function in many disparate areas of social psychology. I have elsewhere formulated this reevaluation in some detail (53); the most prominent questions which must be raised are centered around the conceptualization of the individual as the recipient of influence in the social collectivity. The suggestion model tends to continue and promote the conception of the individual as a submissive reagent to external influences. Most importantly, these influences continue to be restricted to the subconscious level, which today still entails an implicit association with the pathological.

Through the examination of the historical roots of this model, it is believed that social psychology might be more easily divested of the preconceptions discussed above. Future research efforts will therefore be designed so as to explore the development of a model in which the individual is neither submissive nor pathologically motivated, but is instead conceived of as a conscious being cognizant of both action and motivation. This historical study is intended to be a first small step in this direction.

Notes

1. Tarde (1890/1968, p. 199).
2. Apfelbaum & McGuire (1985); McGuire, (1984; in press).
3. The principal exception being Laurence & Perry (1983), who demonstrate that this debate was by no means restricted to France. The authors provide a cogent demonstration of the shifting boundaries which defined both sides of these debates.
4. cf Binet (1888). Other important contemporary treatments of the existence of free will included Fouillée, whose attempted synthesis of both determinist and indeterminist factors represents an influential defence of the possibility of free will (i.e. Fouillée, 1895); Chatterton-Hill's (1904) La physiologie morale, which presages the biological rejection of free will upheld by contemporary sociobiology; Lacassagne's arguments for the logical necessity of free will as a fundamental basis for all penal philosophy (Lacassagne, 1886; pp. 128-157); and Ribot (1883/1916), who suggested that 'la volonté' in general was ultimately nothing more than the reaction to "tout ce groupe d'états conscients, ou subconscients, qui constituent le moi à un moment donné" (p. 33). These theories are only a highly selective representation of the extent to which interest in the possibility of free will lent an important backdrop to the debate over criminal suggestion.
5. Janet (1919, p. 49).
6. McGuire (in press).
7. "Au contraire, les phénomènes hystériques sont bien plus justement comparables à ceux du somnambulisme. Toutes les crises sont identiques, même dans leurs variétés et leurs détails, à telle ou telle forme de somnambulisme complet; les accidents postérieurs à la crise, contractures ou paralysies, sont comparables aux suggestions posthypnotiques; tous les signes, anesthésies ou tares diverses qui persistent entre les crises, sont de la même nature que les signes caractéristiques de l'hémi-somnambulisme". Janet (1973, p. 420-421).
8. LeBon (1895).
9. Tarde (1893). For a detailed exploration of the importance of Tarde the social theorist, see Lubek (1981).
10. Apfelbaum & McGuire (1985).
11. Veith (1965).
12. One of the more interesting aspects of the initial years of hypnotic practice in France was the initial acceptance of psychical research as an important component of the psychopathology of the subconscious. The early history of psychical research in French psychology is given extensive treatment in McGuire (1983; 1984).
13. The principal spokesmen for the Nancy school of suggestive influence included: Liébault (1866); Bernheim (1886; 1917); Beaunis, and Richet (1875). Prominent students of Charcot included Richer (1885); de la Tourette; Janet (1919); and Binet and Féré (1885; 1887).
14. cf Liégeois (1892; 1896-1897).
15. Liégeois (1896-1897, p. 200-201).
16. Bernheim (1886).
17. Liébault (1895).
18. Laurence & Perry (1983).
19. Bézillon (1893).
20. Delboeuf (1895).
21. Lacassagne, (1886); Lombroso, (1890). For a further discussion of the Lombroso versus Lacassagne schools of criminal anthropology, see Nye (1976).
22. Lombroso (1890, p. 76).

23. Binet & Féré (1885, p. 266-267).
24. cf. Laurence & Perry (1983).
25. Liégeois (1896-1897).
26. Crocq (1893).
27. The contagion model of social influence is most extensively explored by Dumas (1911).
28. Crocq (1893).
29. Laurence & Perry (1983).
30. Tarde (1890/1968).
31. cf Anonyme (1895).
32. Apfelbaum & McGuire (1985).
33. The full extent of French legislation dealing with individual responsibility is extensively outlined by Lacassagne (1886) and Liégeois (1889).
34. Binet & Féré (1887).
35. Binet & Féré (1885).
36. Delboeuf (1895).
37. Tarde (1886; 1890/1968).
38. Cited by Ladame (1893, p. 317).
39. Crocq (1893, p. 315).
40. Binet & Féré (1887, p. 270).
41. Binet (1888).
42. Liébault (1895, p. 332).
43. Binet & Féré (1885, p. 278).
44. Binet (1888, p. 217).
45. ibid, p. 26.
46. Binet & Féré (1887, chapter 14).
47. Binet (1898; 1900). See also Apfelbaum (1981).
48. cf Apfelbaum & Lubek (1976).
49. McGuire (in press).
50. Tarde (1890/1968).
51. Crocq (1893, p. 313).
52. Apfelbaum & McGuire (1985).
53. McGuire (in press).

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STRUCTURE ET FONCTION DE L'EXPERIMENTALISME PSYCHO-SOCIAL CHEZ CHARLES FOURIER

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Summary

Fourier's early work generated what is called here 'social-psychological experimentalism' (SPE). Identified in terms of a score of descriptors, SPE is mainly a combination of a scientifically minded project with an action-research project. The hypothesis behind our historical account is that as a message product that is memorized, read or discussed, SPE is causal in a way that depends upon its inner structure.

This was true for the experimental schemes prevalent in biology (hemodynamics, embryology) and entering both economic thought (value-theory) and agronomic practice (A. Young, F. de La Rochefoucauld-Liancourt). Profile similarities between Liancourt and Fourier are many. Agronomic practices and some correlated developments seem to have primed fourierist SPE, which adds, as a main organizing device, the complementary attraction between people, and the systematic arrangement of hedonic structure with ecologic constraints. Under that approach, a variety of stable human types is essential. The organization matching this natural distribution had to be discovered theoretically and serve as an hypothesis for an experimental test. What is the degree of universality of both the discovery claimed and the test requested? From the beginning. Fourier was aware of two competing prerequisities: 1) system effects relate local causations to the whole; 2) simplified or smaller-scale structures and tests are conveivable: e.g. experiments with children. Such a flexibility discriminates between sectarian, utopian ritualism, and an inspiring source of both entrepreneurial or community settlements and more segmental trials, especially within the frame of the holistic attempts. Our first interest is in the "inspiring source" of experimentalism, as implemented namely by Godin in what became "le Familistère de Guise" (1865-1970).

Des traits de l'expérimentalisme sociopsychologique fourieriste

Il existe au 19è siècle une variété de socialisme que ses caractères particuliers rendent pertinent pour comprendre l'insertion et le développement de la psychologie sociale. C'est celui de Charles Fourier (1772-1837). Il présente, mélangés à d'autres, plusieurs traits caractéristiques, plus ou moins généraux et durables, de la psychologie sociale expérimentale: 1) intérèt pour le changement social 2) provoqué, 3) en vue de la satisfaction d'exigences collectives, 4) guidé par une analyse d'intention scientifique des conditions psychologiques, interpsychologiques et sociales des conduites 5) dans la construction de programmes d'action et d'organisation déterminés 6) destinés à essayer et éprouver, de façon "expérimentale", la validité des idées programmatrices, 7) le tout à échelle restreinte, fragmentaire, et non sociale globale, 8) avec l'agrément des individus participants, 9) de façon que la propagation du mode d'intervention dépende, de proche en proche, du résultat des épreuves localement réalisées. 10) suivant un processus analogue dans son mécanisme à celui du marché. L'expérience de Fourier est à la fois une expérimentation au sens scientifique et un essai de recherche-action, visant un changement social positif. La validité éventuelle serait à la fois celle des mécanismes causaux et celle des finalités réalisées.

Ces traits systématiques dans la pensée de Fourier pourraient bien être à la fois a) une réaction contre le réformisme révolutionnaire social global et coercitif, politique et centralisé, vécu avec la Révolution (il avait 17 ans en 1789 et se ruinait à partir de 1792); b) une inspiration de techniques et de pratiques déjà en plein développement avant la Révolution, et notamment de pratiques d'organisation agricole.

Ce que nous désirons mettre en évidence c'est, dans une longue séquence historique, la genèse de ce que nous appellerons l'expérimentalisme sociopsychologique dans sa forme fouriériste; et qui annonce sa destinée -ce qu'il est devenu - dans la suite de Fourier (Pagès, 1958, 1962, par. 1968).

1. Facteurs morphogénétiques de l'évolution historique

Avançons d'abord une idée théorique concernant l'histoire: le déroulement d'événements n'est pas purement affaire d' "interaction" directe entre hommes et "groupes" même quand leur milieu pertinent est un milieu "d'idées" plutôt que de "choses". L'objet dont ces

hommes et ces groupes traitent, quoiqu'il soit un produit humain, circule de support en support (cerveau, paroles, messages matériels...) c'est-à-dire de "noeud" en noeud ou innoeud. Un noeud est soit un organisme capable d'intéroception exprimable et donc de conduite, soit une partie ou ensemble assez autonome d'organisme(s) de cette sorte. Est innoeud tout ce qui n'est pas noeud. Notre objet a ainsi une structure propre (dite "anodale") et un système de propriétés qui en font un facteur causal en lui-même, avec des effets déterminateurs sur le déroulement des événements sans que ses propriétés cessent d'être réinterprétées et éventuellement altérées par chacun de ceux qui participent à son traitement. Est "anoeud" ce qui peut être invariant dans le changement de support, nodel ou in-nodal. Ici, notre objet est l'expérimentalisme social, apparu sous la forme déjà schématisée en dix points ci-dessus. Nous lui supposons en particulier un potentiel morphogénétique bien plus puissant que celui d'autres legs comme un troupeau, un champ ou une réserve de Haut-Médoc, et plus analogue à celui des formes artistiques selon Elie Faure (1927) ou surtout Henri Focillon (1939). Il existerait de même une relative autonomie de la morphogenèse épistémique, avec d'ailleurs accompagnement important de sentiments esthétiques, recouvrant une relative inconscience des participants.

2. Agrocentrisme et physiocratie, pensée et organisation agroéconomique

2.1. Nature du projet de Fourier

D'une part, le projet de Fourier est celui d'un essai d'association essentiellement agricole et cela dès son premier grand ouvrage, la théorie des quatre mouvements (1808, P. 76). Cette idée, née au début du Consulat (1799 d'après l'auteur), et développée en plein Blocus continental, vise explicitement à "l'anéantissement du monopole insulaire" c'est-à-dire à la destruction de la suprématie commerciale anglaise; mais aussi plus généralement à réduction du poids socioéconomique du capital commerical. Ce capital est considéré tout particulièrement en ce qu'il est retiré aux "industries"agricole et manufacturière, en insistant sur agricole. D'ailleurs Fourier annonce que "danks l'ordre societaire" les hommes "préfèreront le séjour des campagnes à celui des villes". Dans cette période de montée industrielle, il ne se préoccupe pas de mécanisation malgré sa réference aux inventeurs tel que Papin, méconnu comme lui-même, qui

est inventeur social (il imaginera une sorte de télécommunication visuelle, de type télévisuel avec réflexion sur des couches sphériques hautes: mais c'est une machine á socialité!) Sa notion de "l'industrie" est d'abord agricole comme sa notion de l'association est domestique-agricole (domagricole, dirons-nous). En revanche il pronostique des domestications animales nouvelles et merveilleuses. Il y a donc tout lieu de penser que son expérimentalisme est foncièrement d'orgine agricole.

2.2. Agrocentrisme et théorie de la valeur

2.21.Essais au sortir de la sapience populaire et des ruptures

Or on ne peut oublier que le 18è siècle avait vu un développement considérable de ce qu'on pourrait appeler un agronomisme sous ses formes anglaise et francaise. En Angleterre, le rôle des ideés de William Petty (1676-1690) est considérable. Chez lui comme en France chez Boisguilbert (1707) par exemple puis chez les Physiocrates (INED. 1958; Poitrineau, 1972) autour de Quesnay (1758) il se produit une mutation de la sagesse ou sapience populaire en pensée scientifique, notamment par la découverte de l'analogie fonctionelle et systémique entre le commerce et la physiologie de la circulation du sang. Petty et Quesnay sont médecins, c'est-à-dire biologistes. Il faut rappeler que la théorie de la circulation du sang (William Harvey, 1628) n'est développée et consolidée qu'en 1661 (Marcello Malpighi) avec la découverte des capillaires.

On trouve aisément dans la pensée multimillénaire ésopique, sans doute cantonnée à l'agriculture et encore refondue par La Fontaine (1668. V. 9. Le laboureur et ses enfants) l'idée paysanne que le travail (ou la peine) et non la terre, est la ressource principale créatrice de valeur. Le travail "est le fonds qui manque le moins", beaucoup plus sûr que les biens fonds, la propriété foncière, qui est cependant un moyen corrélatif: "Gardez-vous leur dit-il de vendre l'héritage (la terre) que nous ont laissé nos parents..." Désormais les débats sapientiels sur les facteurs de valeur ("axiogènes", "axiopoiétiques") deviennent des débats scientifiques livrés à une pensée systématique et au moins en principe "arithmétique", numérisée (Petty).

2.22.Percées d'expérimentation sociopsychologique en embryologie après déboussolage.

Il est vrai que les progrès iront moins vite que ceux qui, après Harvey (1651), ont fait passer l'embryologie, à travers le débat proprement déboussolé, entre partisans de l'oeuf ou de l'ovule et partisans du spermatozoïde, du stade encore en partie mythique (Bonnet rapporte à Eve l'emboîtement des germes, 1762) au stade de la théorie expérimentalisée de la fécondation (Spallanzani, 1785). N'oublions pas que le spermatozoïde avait pu, selon les auteurs, apparaître tantôt comme seul primogéniteur, tantôt comme un parasite. Comme beaucoup de termes biologiques, notamment éthologiques, "parasite" est une métaphore psycho-sociale. Les hypothèses en jeu dans le débat de l'ovisme sont des hypothèses sociopsychologiques. L'expérience cruciale de Spallanzani (cependant imparfaitement comprise de lui par un reste d'ovisme) est bien réellement, et non pas métaphoriquement, une expérience à la fois d'embryologie et de psychologie sociale de l'interemprise des sexes et des âges, par la nature même des hypothèses qu'elle périme ou cautionne. Ceci est aussi important que ce qui suit pour l'histoire de l'expérimentalisme social.

2.23. Persistance des oscillations en économie

Nous trouvons donc à la même époque un affolement de la boussole aussi ample en matière de valeur économique: entre l'oeuf de la terre-mère et la sueur fécondante du laboureur, avec d'autres complications, mais sur un thème mytho-métaphorique évident. Or cette fois, ce sera sans développement expérimental direct ni changement d'écelle réel ou même symbolique corrélatif. L'expérimentalisme économique sera aussi rare que son arithmétisation ou numérisation sera abondante (de W. Petty à Walras et à l'économétrie). Dans le sapience de La Fontaine, les facteur axiogènes sont équilibrés, travail, ressources foncières et "trésors" en monnaie ou métaux précieux, même si la prééminence est donnée, un peu comme plus tard chez les économistic classiques ou chez Marx, au travail (laborisme). Le trésor, alors central chez les mercantilistes (Colbertisme) était, populairement, marginalisé: "tant pis pour le trésor" est le mot du "laboriste" symbolisé par le "laboureur" de la fable, travailleur-type. Les facteurs sont quelque peu équilibrés dans l'esquisse de Petty, avec un laborisme plus marqué chez Cantillon (1755), qui dérive de l'entretien du travailleur la valeur de la terre. Ils se rééquilibreront chez les Physiocrates hétérodoxes comme

Turgot. Mais les Physiocrates orthodoxes sont exclusivement agrocentriques c'est-à-dire centrés sur le bien foncier, assimilé, à travers Malebranche et son occassionalisme (1674), à la Nature sous causalité divine. Les cycles naturels, bien gérés, préservent et multiplient les effets favorables. Ainsi y a-t-il un "produit net" terrien, en quelque sorte gratuit, comme celui dont profitent les lis des champs (Matthieu, VI, 26, 28): ici la quête des causes de plus-value est ouverte et renvoie, pour le miracle de la multiplication des apports, à la nature, comme plus tard, chez Marx, à la force du travail. La nature est vivante et ses processus spontanés, dûment canalisés, sont productifs, adjoncteurs de valeur, à la différence de ce qui se passe dans la manufacture des objets inertes (industrie), et à plus forte raison dans le commerce, l'un et l'autre stériles. Tout se passe ici comme si le propriétaire foncier (et son roi du "bon plaisir") étaient seuls préposés à la gestion des faveurs divines ou de la grâce. La production de valeur selon les physiocrates est une axiogenèse divine assistée par l'homme. C'est en revanche sans doute, aux yeux des paysans, une axiogenèse combinée à l'axiopoièse (action productive); et essentiellement une axiopoièse chez les "industrialistes" saint-simoniens (1820-22, 1829). Il revenait ou commerce de créer une théorie de la valeur liée au marché, offre et demande solvable, qui comporte la prédominance de valeur d'usage, utilité, ou ophélimité. C'est le marcatisme.

En ces matières donc, contrairement au cas de l'embryologie, l'affolement de la boussole n'a guère cessé et le débat continue, malgré les efforts de synthèse, comme récemment ceux de Serge-Christophe Kolm économiste, d'ailleurs attentif à l'expérimentation (1978). Peut-être est-ce parce que la numérisation sans expérimentation ne trouve guère ses concepts adéquats et se révèle impuissante à trancher; et que la sélection et la purification des concepts ne se fait guère sans agrandissement d'échelle, c'est-à-dire microscopie relative. Le type de problème est à nos yeux évidemment lié à celui de l'expérimentalisme social qui a peut-être seul posé le problème. Il est fondamental de le trouver aussi aux origines de ce que Fourier lui-même appellera une boussole (1808).

2.24. L'expérimentation agricole

En effet, l'agrocentrisme, quelle qu'en ait pu être la forme, a constitué un correctif majeur à l'égard du mercantilisme et s'est

marqué notamment par une inversion de la politique du prix des grains, passée du blocage au principe du "bon prix". Corrélativement l'agrocentrisme tend à développer l'agronomie à travers l'expérimentation agricole. Arthur Young lui-même, en dépit de tentatives personnelles inégales, publie en 1770 un "cours d'agriculture expérimentale". Le progès technique, en partie originaire des terres étroites (et donc "rares") de Flandre, se développe en Angleterre et gagne la France avec la noblesse latifondiste. Fertile ou non, le capital n'y était pas méprisé, comme condition du cycle naturel d'autorestauration des sols par la fumure végétale et animale: Victor de Mirabeau (1763), physiocrate, disait: "l'argent est le plus indispensable fumier qu'on puisse répandre sur la terre". C'est que de l'accélération et du renforcement du cycle par la fumure, et par les légumineuses fixatrices d'azote (prairies artificielles), dépendait la fertilité, et que cela supposait une synergie de la phytotechnie et de l'élevage, avec amélioration conjointe de la traction animale (attelage) et de l'équipement (labour, hersage, semoirs...) Cette évolution n'était guère possible que par la "réunion de fermes" ou leur coopération, de façon à intégrer des fonctions hétérogènes et complémentaires. Au surplus elle exige un développement de la technicité, y compris dans la fabrication et la réparation d'équipements, et donc dans les formations. L'accélération des cycles d'autorestauration entraine la suppression des procédés de repos triennal du sol. La jachère, importée de l'agriculture méditerranéenne, et qui allait de pair avec la survivance de disciplines communautaires et de terres communales à droit de vaine pâture, est remplacée par des rotations et associations de cultures. La dislocation des communaux (friches et landes) et leur défrichage implique, outre l'enclôturement, la suppression d'une série de droits, obligations et coutumes féodales, Beaucoup de ces coutumes étaient des traces d'une agriculture initérante semi-nomade et de ses procédés de brûlis et d'essartage. Pour donner une idée de l'accélération des transformations agraires on note que la technique méditerranéene de la jachère, qui avait été une invention, venait tout juste d'atteindre l'Ecosse lorsque son reflux commençait dans le sud de l'Angleterre (18è siècle).

3.F. de La Rochefoucauld-Liancourt, "Gentilhomme Socialiste".

3.1.La combinaison domo-agro-industrielle en marche avant, pendant et après la Révolution et l'Empire.

Le duc François de La Rochefoucauld-Liancourt (1747-1827) est un des plus marquants de ces propriétaires fonciers profondément intégrés au courant agronomiste et dont l'histoire est liée à celle de l'expérimentalisme agricole (Cf. Jean-Dominique de La Rochefaucauld, Claudine Wolikov, Guy Ikni, 1980; abrégé J.D.L.R. dans la suite du texte). En 1768, ami de Choiseul, disgracié à la Cour, il part pour l'Angleterre et s'informe de techniques agricoles avancées. Revenu à Liancourt, à 50 km au Nord de Paris, où il a son château et ses terres (1769), il commence à y essayer ces techniques de façon un peu dispersée, tout en y installant des ateliers de manufacture, puis une école de formation pour les fils de militaires pauvres (ou défunts), ceux d'abord de son régiment personnel (1780-1786). Cette école s'attirera la faveur de Louis XVI (roi bricoleur?) qui en fera une Ecole des Arts et Métiers (plus tard transférée à Compiègne puis à Châlons). Elle prendra un moment le nom d'Ecole des Enfants de la Patrie (1792). Il est vrai que le duc deviendra lui-même "Liancourt" en tant que citoyen, et citoyen révolutionnaire, auteur du mot: Non, sire, ce n'est pas une révolte, c'est une révolution (Nuit finale du 14 juillet 1789). A partir de 1775 au moins et de la guerre des farines (émeutes répondant à des "disettes factices" par accaparement) l'orientation économique de Liancourt aura été pensée politiquement.

Cependant un programme agronomique ne se réalisera que peu à peu, aprés 1783 (mort de son père), et sans doute en partie d'abord à travers l'amitié familiale établie avec Arthur Young au cours du voyage des deux fils du duc et de leur précepteur en Angleterre en 1784-86. Cette relation existe en dépit du fait que Young pense en termes d'investissement capitaliste (rentabilité et disponibilité de l'argent) tandis que Liancourt ne cessera jamais de penser en termes d'économie de service social, ses prélèvements seigneuriaux du début s'accompagnant de requêtes de subventions d'Etat aux pratiques de "bienfaisance", tandis que se dessine chez lui, après 1800, un tour d'esprit certes plus capitaliste, mais aussi plus associatif. Préoccupé d'abord d'expériences culturales fragmentaires, sortes d'expériences-modèles, Liancourt est d'abord très en retard, à en juger par les notes de Young (1792), sur son ancien patron Choiseul ou sa belle-soeur Mme de Pons (J.D.L.R., P. 108-109) au sein d'une

aristocratie latifondiste éclairée et qui lui est proche. En 1786 il fait des démarches pour établir une "ferme expérimentale", qu'il n'a réalisée qu'en 1790 à Louveaucourt avec Young comme conseiller, et Reeves, un fermier anglais du Suffolk (Sud-Est anglais) comme maître d'oeuvre: le cheptel et l'outillage étaient aussi importés en partie. Dès 1792, Liancourt devra émigrer et sa ferme passée sous séquestre sera dévolue à un Conventionnel (1794) qui dira trouver ses innovations peu rentables. Il aura eu juste le temps, apparemment, de tenter de réaliser une intégration de l'élevage (ovin, et de gros bétail), de cultures fourragères et sans doute de cultures variées. Car ce qui était nouveau, très clairement, y était l'extension proportionnelle de l'élevage et c'est ce que Young relevait manifestement par opposition à ses observations antérieures. C'est en effet l'élevage sélectif, et sans doute hybridateur, associé notamment aux légumineuses (papilionacées fourragères ou vivrières) qui signifiait au plus haut point la découverte d'une organisation systémique parce que synergique, modèle peut-être d'une pensée écologique associative. Cette économie dominera le progrès agricole jusqu'a l'introduction massive des engrais chimiques vers le milieu du 19è siècle (Liebig en 1840).

C'est donc bien, fondamentalement, sur la mise à l'épreuve exemplaire d'une découverte centrale et de son contexte que s'orientait l'activité de Liancourt par ailleurs sollicitée par son appui au partage des terres aux fins de multiplier le stimulant de la propriété. Peut-être ces deux orientations n'étaient-elles pas parfaitement compatibles. La ferme expérimentale elle-même ne dépassait pas 50 hectares. Peut-être le problème des stimulants humains, que devaient poser, et la pratique révolutionnaire et, sous une forme systématique, Fourier, relevait-il d'un autre domaine théorique (qui ne s'articulait pas automatiqement au premier): celui de l'organisation humaine. Et les problèmes de ce domaine étaient aussi ceux de Liancourt.

C'est pourquoi il y a chez lui plus et moins que le créateur d'une ferme modèle. Longtemps inférieur à d'autres comme expérimentateur agricole, il prend en réalité un ensemble de problèmes à un autre niveau. D'abord soucieux d'améliorations locales et exemplaires disséminées dans son marquisat, on le voit autour de 1789 viser à regrouper ses réalisations, particulièrement manufacturières. En fait, il tend à réorganiser la société rurale par

une sorte d'urbanisation économique, technique et éducative, qui dessine, centré sur l'agriculture et encore distendu dans la société rurale, un ensemble domestique, agricole, manufacturier et éducatif.

Cette activité est inséparable de l'action politique. En 1789, le duc est député aux Etats Généraux, Président de l'Asemblée Nationale (après la prise de la Bastille) le 18 juillet 89, le lendemain du jour ou Louis XVI reçoit la cocarde tricolore, et pendant la Grande Peur, jusqu'au 3 août, à la veille de la nuit du 4 août qui abolit les privilèges. On retrouve ce bègue Président du Comité de mendicité (1790), puis membre du Club des Feuillants, formé par l'aile modérée du Club des Jacobins après l'affaire de Varennes, avec La Fayette, Siéyès, Barnave (déjà "sociologue" de la Révolution, 1792). Il fait ce qu'il peut pour entrainer la noblesse à renoncer à ses privilèges et pour pousser le roi à la tête de la Révolution, mais aussi pour empêcher la Révolution de supprimer le roi. Monarchiste constitutionnel, il doit émigrer après l'arrestation du roi (10 août 1792). Rentré d'un exil anglais et surtout américain après le Consulat (1799), il n'arrête pas son action réformatrice, plutôt militante, dirions-nous, que "philanthropique" au sens actual d'un mot qui s'écrivait aussi Ami des hommes. Il soutient les progrès médicaux (vaccine) aussi bien qu'agricoles. Député aux Cent Jours, puis pair à la Restauration, il déploiera inlassablement une activité gratuite dans les conseils concernant la politique agricole, mais aussi manufacturière, hospitalière, carcérale, enseignante, ainsi que dans le mouvement anti-esclavagiste. Son excès de libéralisme lui vaudra d'être destitué de toutes ses charges en 1825, disgracié sous Charles X comme au temps de Mme du Barry.

Ce contexte politique atteste de la part du duc un exceptionnel acharnement dans une voie continuée à travers les plus grands bouleversements. Il marque d'un caractère singulier ses tentatives à Liancourt et la reprise qu'il en fait après son retour d'exil, fermier dans un pavillon qui subsiste de son château au milieu de son parc mis en culture. C'est là que, rappelant Reeves, il réalise vraiment une "des grandes fermes expérimentales" que souhaite le pouvoir. Au dire de Cambry, le Préfet de l'Oise: "il perfectionne toutes les espèces de cultures, suggère les plus belles races d'animaux et répand chez tous les voisins les procédés de la nouvelle agriculture". (Cambry, P. 313). On retrouve la mise en relief de l'élevage (mouton mérinos) et la variété des cultures végétales. On

reconnaît aussi, à côté du développement d'un hospice d'origine paternelle, la relance du travail manufacturier. Il perfectionne localement les ponts (un métallique) et les chaussées (macadamisation). Trois ans encore avant sa mort, il crée un cours de mécanique appliguée dans son château. Si l'on réunit les fonctions d'assistance hospitalière et d'éducation, c'est un noyau d'organisation domestique qui tend à se dégager, au sein d'un complexe domo-agro-industriel formant système.

D'autre part, dans l'esprit de l'association non étatique, Liancourt crée dans ses manufactures, des associations mutuelles de prévoyance tandis qu'il fonde avec des banquiers la Caisse d'Epargne de Paris (1818). Le mutualisme guide encore ses interventions dans l'enseignement à l'échelle nationale et locale (à Liancourt), où il opte pour la méthode d'enseignement mutuel, un monitorat pédagogique hiérarchisé: les plus instruits y sont les moniteurss des moins instruits.

3.2 Spectrographie comparée de Liancourt et Fourier

L'ensemble de ces détails, traités en indicateurs de conception et d'action, constituent un spectre idéo-pratique caractérisé, avec des formants propres à différent domaines.

Il est possible de comparer ici ces "spectres" chez Fourier et Liancourt. Cette comparaison, qui ressort du texte, mais qui pourrait être explicitée en tableau systématique, est sans acception d'interemprise effective entre les deux personnages. Elle constate seulement les coincidences entre plusieurs conceptions ou démarches, certaines oppositions aussi, comme le fait de la réalisation essentiellement textuelle et conceptive chez Fourier, à la fois conceptive et pratique chez Liancourt. Ainsi encore, Fourier intègre, critique et développe la notion d'enseignement mutuel en 1822 (mais non en 1808): cela relève du "formant" éducation dans le "spectre". L'anglophile initiale de Liancourt diffère des réserves d'ailleurs limitées de Fourier. On vérifiera sans peine que leur spectre d'intersection est considérable et dépasse de beaucoup le rapport entre une ferme-modèle et le phalanstère, et même entre ce dernier et le complexe domo-agro-industriel de Liancourt.

Une différence réside dans le fait que le spectre fouriériste est condensé dans l'idée du phalanstère, même si sa conception d'un "garantisme" évolutif de la société globale, d'une sorte de sécurisme

rèformiste (R. Pagès, 1958), y est détaillée à coté de celle de l'expérimentation intégrée. Chez Liancourt les mêmes traits du spectre apparaissent mais comme des membres disjoints.

Une autre différence évidente et d'ailleurs essentielle mais dont il faudrait mesurer le profondeur, se trouve dans le caractère associatif du phalanstère, donc son volontariat lié à la condition d'une "attraction passionnée" ou attirance individuelle. Ainsi, alors que Fourier dit que l'enseignement intégral mutuel sera, "en harmonie", la méethode essentielle (1829), il reproche à l'application qui en a été faite l'hétérogénéité aléatoire des élèves et l'absence d'attirance mutuelle des participants. Or Liancourt, traducteur de Lancaster (1810, 1815) état, dans la société pour l'instruction élémentaire, le promoteur par excellence de l'enseignement mutuel qu'il appliquait, l'un des tout premiers, dans son hospice de l'Oise; et qu'il avait contribué à organiser à l'échelle nationale, en lutte contre les ultras cléricaux. Il y a ici un lien d'interférence où la position critique de Fourier est évidemment quand même au pôle opposé aux ultras, au-delà de Liancourt mais du même côté.

Les différences ne peuvent cacher une analogie essentielle. Pour l'un et pour l'autre réformateur les processus révolutionnaires spontanés sont autant des symptômes à interpréter que des voies de résolution des difficultés sociales. L'expérimentation, facteur et application des connaisances scientifiques, est la partie la plus maitrisable, la plus sûre, la moins coûteuse, des transformations sociales. La politique peut jouer un rôle de facilitation, mais le mode de diffusion des acquis expérimentaux est l'exemplarité, non la coercition dont le despotisme et la Terreur ou Contre-Terreur fournissent des illustrations. Quant à cette distance acquise par l'expérimentalisme social à l'égard de la politique empirique, on notera, conjointement, le fait que Liancourt n'a jamais été rétabli dans son titre de duc par Napoléon, qui lui a offert cependant en 1811 le Ministère du Commerce et des Manufactures, offre refusée. (C'est la position qu'occupera Chaptal, ami de Liancourt, aux Cent-Jours.)

Nous sommes partis, pour rencontrer Liancourt, de l'hypothèse d'une inspiration de Fourier par des amorces d'expérimentalisme social liées à l'agronomie. C'est en substance celle de l'emprise

inspiratrice par amorçage (cf. R. Pagès & M. Derghal, 1984). Certes l'expérimentation agricole présente les caractères statistiques qui mènent aux plans d'expérience de R.B. Fisher (1935) (cf. l'expérience de Liancourt sur le plantage du blé, menée en contradiction, malheureuse d'ailleurs, avec son voisin et expropriateur Isoré, mais qui lui vaudra cependant une récompense de la Société impériale d'agriculture, dont il est: sans doute la netteté du test d'hypothèse par le rendement y était-elle pour quelque chose). Mais l'important est que l'expérimentation agricole, par ses effets, tendait à être domestique-agricole, selon l'expression de Fourier: elle affectait le genre de vie dans lequel habiter et travailler ne sont pas dissociées. Elle se transformait intrinsèquement en essai social évaluable. Autour du foyer d'une méethode, c'est un fonds commun important qui se découvre entre l'expérimentalisme agraire de l'un et le project socio-expérimentaliste de l'autre.

3.3. Liancourt dans le réseau subjectif d'appui de Fourier

Cependant un recoupement de spectre n'est pas un lien causal, Existe-t-il des signes d'une emprise quelconque, dans un sens ou dans l'autre, entre le réseau de Liancourt et celui de Fourier? Fourier est plus jeune de 25 ans et mourra 10 ans après lui. Il a 20 ans quand le duc part en exil et celui-ci ne reviendra qu'en 1799, l'année même ou Fourier dit avoir conçu l'essentiel de sa théorie. Cependant Fourier, qui voit dans la jachère et l'insuffisance des fumures les deux défauts essentiels de l'agriculture courante (1822, IV), peut bien avoir profité de la vulgarisation de l'agronomie anglaise: ainsi le Directoire fait traduire Arthur Young, l'expert et ami de Liancourt, sous le titre "Le cultivateur anglais". Mais Fourier cite surtout les "grandes fermes anglaises" comme des "bagnes", et non comme l'antidote au morcellement des terres, autre fléau selon lui.

Procédons autrement: tout individu a un réseau de réferences, incluant un réseau subjectif d'appui. Or en novembre 1822, Fourier publie son deuxième grand ouvrage, le Traité de l'association domestique-agricole" (1300 pages environ). Il s'en fait le démarcheur. Charles Pellarin, son principal biographe (1849, 1882) avant Emile Lehouck (1978), relève des destinataires du Traité lui-même. Ce sont, dans l'ordre: "Chaptal, Larochefoucault (sic), Laplace, etc. parmi certaines notabilités qu'il croyait susceptibles

d'y prendre intérêt". Cette première distribution parait s'être faite en novembre ou décembre 1822. Devant l'insuccès, Fourier écrit aussitôt un sommaire de 120 pages (1823) dont il distribue "un assez grand nombre". Il y aurait donc eu une double distribution décalée et hiérarchisée, sans doute en fonction de probabilités de lecture. Et Pellarin ajoute: "Mais la plupart des personnages ne daignèrent même pas en prendre connaissance. Les plus polis s'excusaient, comme... Benjamin Constant."

D'après Pellarin, 25 ans après (1874, P. 37 sq.), il est assez clair que Fourier a bien frappé "à toutes les portes indistinctement", mais qu'il n'y a pas présenté le même cadeau. Et ce ne sont pas les politiques purs qu'il a visés d'abord, apparemment, que ce soit le Premier Ministre ultra Villèle ou même Benjamin Constant et les libéraux. Sauf sélection de point de vue du médecin bernardien Pellarin et à 25 ans de distance, entre les deux savants Chaptal, auteur imminent de "la Chimie appliquée à l'agriculture" (1823), et Laplace, ce dernier considéré par Fourier comme le plus grand des newtoniens, tous deux anciens Ministres de l'Intérieur du Premier Consul, -un La Rochefoucauld (mai orthographié) ne peut guère être que le duc de Liancourt, ami de Chaptal, à l'Académie des Sciences depuis 1821, sorte de "ministre officieux du bien public", qui devait être relevé de ses charges en 1823 par l'ultra, Ministre de la police, Corbière, -et dont le cercueil devait être fracassé au sol au cours des obsèques par des brutalités de la police du même Ministre (1827). Il s'agirait de lui plutôt que de l'autre duc de la Rochefoucauld (-Doudeauville), beaucoup moins populaire, Directeur des Postes en 1822, qui devait il est vrai obtenir de la faveur de Charles X, étant Ministre de la Maison du Roi, les moyens de créer l'Ecole d'agriculture de Grignon: c'était son lointain parent (J.D.L.R., 1980, P. 339), celui qu'Alexandre Dumas appelle "proche parent", (1854-55, chap. CL et CLI) dans la position privilégiée qu'il lui donne aux funérailles de Liancourt, dont il fait une description d'apparat romancée. En fait, la famille Doudeauville appartient plutôt aux ligues secrètes des ultras, chevaliers de la Foi et de la Congrégation. Le fils, Sosthène, avait acheté un journal important et quelques autres dans la lutte des ultras pour baillonner la presse. Le réseau d'appui de Fourier contenait, entre Laplace et Chaptal, Liancourt et non Doudeauville. Et pourtant Dumas (ibid.) disait bien que Liancourt était un gentilhomme "socialiste" et ....

un libéral: "on profitait de sa mort pour faire une démonstration contre les ultras et contre les jésuites."

Tel est le fil ténu et isolé qui pourrait suggérer un rapport direct réel, alors sans doute unilatéral, de Fourier, grand lecteur de journaux, à François de la Rochefoucauld-Liancourt. Ce fil mériterait d'être suivi. Il supposerait un effort d'éclaircissement de maints détails biographiques dont l'obscurité relative n'apparaît dans le cas de Liancourt qu'à la faveur d'un savoir déjà important. Il permettrait de restituer Fourier, jusqu'ici traité en aérolithe, à une trame de formation et à des mailles de réferences comparatives dont les fils ne sont peu visibles que parce que cet autodidacte ne les a jamais lui-même bien repérés. (Il est vrai que Liancourt, grand seigneur, était aussi autodidacte).

Dans le cas de Fourier, la légende hagiographique est contaminée par la légende caricaturale sur fond d'ignorance. Lehouck (o.c.) a fait un travail critique important et il construit, avec de bonnes raisons textuelles, l'idée d'un Fourier de tradition ésotérique et qui s'éloignerait de la science "positiviste" tout en se rapprochant d'une pensée plus écologique ou écologiste.

4.Débordement des concepts physiques et conception thymique dans la science et la pratique psycho-sociale

4.1. L'illuminisme, condition conjoncturelle d'une psychologie non newtonienne (Saint-Martin).

Nous serions là bien écartés de l'expérimentalisme social s'il était lié à une vision purement techniciste d'une agronomie étroite. Nous avons vu que le fonds commun "spectral" de Fourier et de Liancourt était fort loin de s'y réduire. Ce qui est vrai, c'est qu'un "physiocratisme" de l'ordre naturel chez Fourier se développe sur un fond quasi-mythique de cosmologie et d'illuminisme (S. Debout, 1967, P. 365-392) dont Claude de Saint-Martin (1743-1803 paraît le principal inspirateur (par exemple 1775, 1790, 1800). Nous ne pouvons qu'esquisser une analyse du sens de cet illuminisme de Fourier, anthropocosmologique dans son contenu, avec l'analogie microcosme-macrocosme par la médiation de l'image de Dieu, et sa priorité à l'anthropomorphisme, -ésotérique dans sa nature, car il s'agit d'une vérité globale révélée de façon essentiellement rapide ou soudaine et qui est la pensée et la réalité de Dieu (théosophie).

On comprend que cette composante de la pensée de Fourier ait parfois répugné à des disciples comme le polytechnicien Victor Considérant.

Mais l'illuminisme de la fin du 18è siècle a probablement un sens très différent de ce qu'in anachronisme pouvait suggérer. Il est avant tout scientifiquement unitaire, chez Saint-Martin comme chez Fourier dont les Harmonices Mundi de Kepler sont un des rares livres de chevet. La pensée galiléo-newtonienne ne gardait du concept de mouvement que le déplacement passif. Dans une période où l'harmonie du monde humain était au plus loin il fallait reconstituer des concepts aptes à traiter de la conduite humaine et même animale, ne serait-ce que de la locomotion : elle n'est pas un déplacement passif et répond surtout à une attirance qui n'est pas l'attraction newtonienne. Dans cette culture hypnotiséz par Newton, il s'agit de former des concepts newtoniens pour traiter des sujets de conduite, tout en s'intégrant dans un ensemble où Newton ait toute sa place, mais non toute la place. La pesanteur ou la gravité, dont l'étude a engendré la mécanique, a produit des concepts mécaniques. Saint-Martin s'essaie à parler de "gravité active", et l'étude des mobiles directionnels (1790) ("boussoles", orexies contrôlées par la thymie, manifestée par les émotions) paraît fournir ce type de concept. Même si l'énergie nécessaire à leur création et à leur défense conduit à un anthropomorphisme universel tendant à réexpliquer les déplacements astraux par une motilité, ce contrenewtonisme n'est que la condition subjective de la création d'une théorie de la conduite, propre à guider une politique d'harmonisation sociale, fondée sur "la vraie nature" des hommes. Chez Saint-Martin, inspiré de Martines de Pasqually, puis de Jacob Boehme -celui-là même qui fournit à Engels son concept du tourment dialectique ou Qual-l'enjeu est, comme chez Hegel, surtout mystique, malgré son théocratisme. Chez Fourier (comme chez Marx) l'enjeu est celui de la pratique sociale. Elyphas Lévi (1860) dit de lui: "un mystique matérialiste retourna le système de Swedenborg [en fait celui de Saint-Martin: Fourier ne connaissait guère Swedenborg] pour créer sur la terre le paradis des attractions proportionnelles aux destinées". Mis à part la couleur newtonienne et le vieillissement des mots, l'attraction industrielle signifie bien, psychologiquement, l'attirance industrieuse (ou laborieuse) traitée comme ordre du monde.

4.2 L'attirance industrieuse

Or chez Liancourt, Ami des hommes, certes le développement domo-agro-industriel tend à étre pensé de façon à assurer le bonheur collectif (1819), mais il n'en recourt pas moins de façon extraordinairement massive (et curieusement non commentée, ni par lui ni par son exégète homonyme. J.D.L.R.) au travail des enfants notamment en manufacture. Quand on sait que le travail des enfants a) était une habitude rurale et artisanale pas forcément tortionnaire; b) a fait un des principaux scandales de l'industrialisation sauvage et continue à le faire, cela frappe. Quand Fourier, qui lui-même ne se prive pas de participation infantile dans ses projets, en fait état, il l'intègre dans son schème d'attraction passionnée, ou d'attirance maximale (chez les enfants, cf. "les petites hordes", in Fourier, 1835-36; Silberling, 1911). C'est que la thymie est pour lui essentielle. De l'agronomie, la politique ou gestion des hommes retient des schémas de rentabilité et de sélection, animale et végétale et, plus tard, humaine. Pour Fourier au contraire, la voie du rendement multiplié n'est que la conséquence de la restauration de la boussole affective définie en fait par Saint-Martin, et qui est celle du plaisir et de la souffrance, les deux pôles thymiques. "Le but... de produire <u>l'attraction industrielle</u> [= laborieuse] est de nous entraîner par l'appât du plaisir au travail agricole qui est aujourd'hui un supplice pour l'homme bien né. Les fonctions telles que le labourage nous inspirent avec raison un dégoût voisin de l'horreur." (1808, Note 4, p. 233). Cependant, de par la définition illuministe, les goûts naturels sont en nombre nécessaire et suffisent pour assurer l'attirance industrieuse totale. Ils exicuent les opérations de sélection.

Dans cette perspective, "travail" n'est pas seulement une ressource pour l'exploration des terres: c'est un mode de vie qui a sa fin en soi. Sa qualité thymique est un objectif que l'expérimentation se destine à réaliser et à évaluer. Il ne s'agit pas seulement d'expérimenter sur des races de moutons, mais à la fois sur la validité économique et la validité psycho-sociale d'un type d'organisation qui tienne compte des dispositions congénitales des hommes, à leurs propres fins. Comme dans les recherches princeps de A. Bavelas (1948) et H.J. Leavitt (1951) sur les réseaux, deux critères, deux variables indépendantes apparaissent: le rendement et la <u>satisfaction</u>; tandis que, comme dans ces recherches également, les variables indépendantes sont les paramètres d'un mode d'organisation.

Les précisions concernant les variables expérimentales, indépendantes, ajoutent une clause de contenu aux dix clauses inventoriées au départ dans ls définition de l'expérimentalisme psycho-social.

4.3. Le modele experimental dans son contenu et sa forme générale

4.31.Le contenu théorique

Nous sommes donc bien en présence d'un modèle expérimental assez précis, qui s'est élaboré sans doute à partir de l'esprit des entreprises agricoles modèles, constituant par elle-mêmes et à ce titre a) des expériences globales, et b) conjointement des stations expérimentales, ayant vocation pour réaliser des expériences fragmentaires dans leur sein, si l'expérimentation, c'est-à-dire l'action probatoire, peut-être divisée, s'agissant, à certains égards, d'une vérité globale. C'est un point que nous retrouverons.

A cette construction d'une conception expérimentale, il faut dès lors adjoindre sa conjugaison avec une théorie sociopsychologique plus précise: la théorie est que la structure passionnelle (soit, en gros, orexique ou pulsionnelle) des hommes est 1) spécifiquement et individuellement stable et 2) intra- en interindividuellement diversifiéee, 3) qu'elle entraîne des exigences stables et diversifiées en matière de sanction intrinsèque, de l'activité, notamment du travail, 4) qu'elle est optimale pour un certain type d'organisation du milieu aménagé et peuplé, 5) que le type de milieu doit être scientifiquement découvert et 6) soigneusement aménagé. Il s'agit d'un "canton" d'essai, avec un nombre restreint mais suffisant de personnes, pour que l'essentiel de la variété humaine pertinente aux différentes activités y soit représenté. (La personologie propre à Fourier le conduit à 1620 personnes, soit 810 par sexe, qu'une combinatoire complexe doit distribuer dans 150 séries, avec 40 goûts dominants par personne, dans la version initiale (1808, o.c., p. 240). C'est là que triomphe le calcul, modèle mathématique implicite en effet, dont se prévaut le comptable Fourier.

4.32 La structure formelle sous-jacente au modèle

En quoi cette conception, est-elle proprement expérimentale, en l'absence formelle de groupe-témoin? Une édition posthume de 1841 de l'ouvrage de 1808, lui-même postérieur de neuf ans à l'illumination initiale datée de 1799, répond à cela très clairement par une

métaphore agricole: "les habitants d'un pays fort arriéré cultivent leurs champs de père en fils, d'après une méthode qui leur donne quatre-vingt-dix-neuf pieds d'ivraie pour un pied de blé. Un homme survient qui, ayant longtemps étudié les lois de la nature, a découvert une nouvelle méthode. Il expose, en critiquant l'ancienne, que la méthode nouvelle doit produire avec cent fois moins de travail quatre-vingt-dix-neuf beaux épis pour un brun d'ivraie. Il demande que dans le coin d'un champ on fasse l'expérience de sa méthode. Or voici que les paysans s'ameutent contre cet homme et parlent de la lapider, parce que, croient-ils, le misérable porte atteinte à la culture du blé, insulte les cultivateurs...." Telle est la parabole de ton évangélique et quelque peu bonimenteur qui présente de façon claire un champ d'expérience: la champ est divisé en deux parcelles, la parcelle expérimentale, objet du traitement nouveau, et son complément dans l'esemble du champ homogène, le reste du champ, objet du traitement usuel. Certes l'expérience présentée symboliquement est agronomique et phytotechnique, tandis que l'experimentation fouriériste est socionomique et anthropotechnique. Mais la portée réelle indirecte de l'agronomie et, aussi bien, de toute technique industrielle, est anthropotechnique : d'où les menaces de lapidation. Chaque expérimentation sur l'homme doit compter avec l'effet de nouveauté et surtout l'effet de résistance à l'emprise analytique, prélude d'une emprise transformatrice de l'extérieur, hétéronome et aliénante (Pagès & Lemoine, 1979). Le propre de la science expérimentale est de déterminer des immixts, c'est-à-dire des points d'insertion efficaces de l'action scientifiquement éclairée.

5.Les conditions de calibrage expérimental et de calibrage de l'action

5.1.Calibrage local contre effet global: Les détournements d'effets par l'insertion.

Fourier a lui-même ressenti profondément cette aliénation liée à ses yeux à l'artificialisme philosophique, caractéristique des Jacobins, marqué par l'insuffisance en matière de sciences de l'homme, donc par l'aveuglement et donc aussi par une espèce de magie volontaire propitiatoire, corrélativement volontiers sacrificiante, dans l'inconscience du sens véritable des actes. Le terrorisme, rouge ou blanc, se croit technique lui aussi quand il est souvent propitiatoire en effet, ou expiatoire. Mais l'artificialisme vraiment

technique, par exemple agronomique, est lui aussi, on l'a vu, indirectement politique et anthropotechnique, en ce qu'il transforme la vie sociale. Il pose donc des problèmes analogues à ceux de la cécité politique directe. C'est ainsi que de développement de l'art nautique, en parvenant à la navigation planétaire courante, accroît jusqu'au monopole la puissance du capital commercial: "dès lors cet excès de connaissance [technique] nous devient funeste". La structure retardataire de la société de l'époque fait que "si elle s'élève à un tel degré de science [sectorielle] c'est un peu pour son malheur" (1808, p. 195), c'est-à-dire pour son "déclin" et sa "caducité". Le retard systématique de l'anthropologie peut rendre certains développements techniques toxiques et même léteaux si les conditions de leur intégration ne sont pas créées." En civilisation la pauvreté nait de l'abondance même (Nouveau monde industriel, 1829). L'effet pervers ainsi décrit n'est pas tel par rapport à des objectifs initiaux mais par rapport à des résultats intermédiaires et partiels.

Nous sommes ici fort loin de toute espèce de notions traditionnelles dans la théorie de l'action, implicite ou explicite, et nous sommes affrontés au problème du calibrage de l'action sociale; directement posé d'ailleurs inévitablement, et en termes clairs, par l'expérimentation et son calibrage.

Un développement fragmentaire d'une variable favorable peut entrer en conflit avec d'autres variables positives : c'était ce que montraient avec une netteté remarquable les premières expériences sur les réseaux de Bavelas (o.c.: R. Pagès, 1971) où respectivement le plaisir et la performance n'étaient pas synergiques pour les sujets, mais autagoniques. Toute hausse fragmentaire de productivité, ou productivation fragmentaire, est susceptible de dégrader ou de détériorer d'autres facteurs positifs. Tout succès d'une entreprise en détruit d'autres. Les réseaux de causalité sont fragmentés en systèmes partiels mais il y a des réseaux causaux surimposés entre ces systèmes.

Et si l'on se place au contraire au niveau global, les notions "agrégatives" ou additives, telles celle de forces dites "productives" (Marx) ou celle de "revenu national", (économétrie) méconnaissent les rapports divers et notamment antagoniques entre leurs composantes. A petite échelle une relation causale déterminée peut être validée expérimentalement, mais dépend en fait d'une tierce variable, qui reste souvent latente dans une expérience partielle,

mais qui se manifeste au changement de site expérimental, se traduisant par un changement de système ou du fait d'une complexification. Tel est bien et de proche en proche un effet de milieu, par excellence objet d'écologie.

5.2. L'ecologie du milieu humain potentiel normal

Comme le suggère Lehouck (1978, Introduction) l'illuminisme de Fourier, avec son fond disons de théodicée et le naturisme qui en procède, est bien une sorte d'écologisme. L'interventionisme social lié à l'expérimentation vise bien à la restitution d'un accord avec un système naturel supposé perturbé ou irréalisé. Mais c'est un système potentiel. L'homme est à la recherche de son propre milieu de coaptation ou milieu spécifique optimal. Il est probable que cette idée n'était guère possible à cette époque que sous sa forme illuministe, c'est-à-dire, aussi, mystique et révélationiste. Il n'est pas surprenant que cette forme et ce context aient rebuté jusqu'à ses disciples et il est vrai qu'elle charriait avec elle non seulement des mythes messianiques mais le contenu d'une psychologie, d'une sociopsychologie, et notamment d'une psycho-sexologie éminemment subversives. Ce n'est surtout pas avec la respectabilité de la société de son temps que l'homme de Fourier était coapté. Mais il importe sans doute de comprendre que si l'irrationalisme partiel de Fourier est conjuncturellement associé à sa psychologie sociale et sexuelle, et à son érotisme (1807) on n'a aucun motif de penser que cette conjonction, et la conjonction d'oppositions qu'elle détermine, aient un caractère autre que circonstanciel. C'est chez W. Reich (1947) qu'on verra un moment se reconstituer une configuration voisine, avant la phase magique des huttes à "orgone", mais non chez S. Freud, théoricien de la religion comme névrose (1929) certes mais érotiquement beaucoup moins libéral que Reich et Fourier : c'est que Fourier met au nombre et au sommet des "passions rectrices" l'harmonisme, c'est-à-dire le besoin irrépressible de participer à une organisation sociale harmonieuse et affectueuse, à peu près là où Freud situe le surmoi (1920) hypostase du refoulement supposé inéluctable.

A méconnaître cette dissociation possible de l'illuminisme et de la psychologie socioaffective, on méconnaitrait la portée de l'association entre l'écologisme et l'expérimentalisme de Fourier. On ne comprendrait pas pourquoi Fourier se détache de Saint-Martin

malgré un "coloris d'illuminisme", pourquoi son associationnisme se recoupe quelque peu avec le mutualisme et le solidarisme issus de la "bienfaisance" mais la dépassant explicitement, correcteurs du marché chez Liancourt. On attribuerait à une validité caractérielle son extraordinaire insistance sur sa priorité de deécouverte alors qu'il s'agit évidemment de l'idee que sa révélation à lui, son illumination, est radicalement différente de celle de Saint-Martin. Mais nous sommes ici, reconnaissons-le, dans la zone obscure où l'ostentation de savoir total caractérise le messianisme. Nul n'a plus que Fourier attaqué le charlatanisme. Nous manquons fâcheusement d'une psychologie des messies. Fourier pense en tout cas qu'il a découvert le modèle du complément organisationnel, jamais encore réalisé, de la psycho-structure humaine spécifique. Il reste à éprouver réellement cette adéquation. Et tel est le sens de son expérimentalisme. On peut imaginer qu'il s'insère dans une recherche où la théorie de la structure psychique est sans cesse remaniée en rapport avec une psychologie des relations sociales et réciproquement.

5.3. La phalange, unité psycho-sociale normale

Expérimentaliste, Fourier tente d'abord de déterminer l'échelle utile d'épreuve qui est en même temps un degré de complexité et un seuil qualitatif d'émergence et de démonstration du système harmonieux. D'où la délimitation de la "phalange" minimale, et peu importe ici l'arbitraire évident de son volume "précis" (dépendant des "810" caractèeres individuels pour chaque sexe) tel que Fourier la décrit. L'important est l'idée de système et des conditions minimales de composition aptes à constituer le système ou le "germe" du système psycho-social. C'est d'ailleurs à l'évidence cette ideée que visait le mot "phalange" évocateur de compacité et de force autour d'une schème de sens du type "poutre" ou "bloc", dans l'histoire des langues européennes (Grandsaignes d'Hauterive, 1949). C'est ce qu'illustre remarquablement un texte "antiquaire" de Flaubert (Salammbo, 1862, chap. VIII): "Au milieu se hérissait la phalange formée de syntagmes ou carrés pleins [...] les six premiers rangs croisaient leurs sarisses [lances] en les tenant par le milieu et les dix rangs inférieurs les appuyaient sur les épaules de leurs compagnons [...] devant eux [...] et cette horrible masse quandrangulaire remuait d'une seule pièce, semblait vivre comme un bête et fonctionner comme une machine. Deux chohortes d'éléphants la bordaient régulièrement

[...] "Telle serait la phalange punique d'Hamilcar. (Et il faudrait peut-être y conjuguer la troupe homosexuelle de type thébain, décrite au milieu du chapitre XIV). Tel est un système à forte démarcation et connexité internes, dont la cohésion et le monolithisme sont sentis comme ceux d'une machine et d'un organisme à la fois.

De cette systémicité de la construction à éprouver, Fourier tire l'idée que seule elle garantit la stabilité des dispositions particulières qui s'y inscrivent, ce qui est en soi une notion capitale. Elle est exprimée avec une vigueur singulière dans le premier ouvrage: "La mécanique passionnée ne peut pas s'organiser imcomplètement, chaque partie est nécessaire au tout, et l'absence de quelques rouages mettrait en désordre toute la machine: c'est pourquoi l'on ne pourrait pas former une demi-phalange de sectes [séries] progressives au nombre d'une soixantaine seulement" (1808, p. 239-240), là où le programme en exige 150. "Dans une fondation approximative le système est faussé par la réduction" (1829) et "En échelle réduite, on échouerait sur l'accord de répartition, les impulsions de cupidité ne seraient pas assez graduées pour s'équilibrer..." (id). La phalange est proposée comme un remède encore syncrétique et inanalysé bien qu'il soit construit. Mais il l'est avec des règles de construction qui paraissent n'être pas maniables séparément de la réalisation unique. L'expérimentation se résoudrait en démarche d'évaluation d'un produit ou d'une réaction complexe.

5.4. Les variations expérimentales

Cependant, il ne faut pas perdre de vue la structure de la pensée de Fourier. Son "remède" n'est pas vraiment syncrétique et l'auteur, en termes médicaux, n'est pas un empirique. Il construit sa phalange comme opérationalisation d'une variable, l' "attraction passionnée", ou attirance maximale qui elle-même combine de nombreux paramètres. Dès lors le problème d'échelle en dépend. Fourier en visage la réduction. Et quasiment d'emblée il la précise, non comme un palliatif ou un expédient, mais de façon tout à fait systématique. Au texte tout ou rien ci-dessus, Fourier ajoute dans la même phrase: "il n'y a de mitoyen entre l'harmonie et l'incohérence qu'une ordre sociétaire à passions nuancées". Et il renvoie au chapitre "Ménage progressif ou tribu à neuf groupes" (id. p. 139). C'est que, rappelle-t-il, il convient d'éviter en tout point l'uniformité; car l'uniformité, la tiédeur et la médiocrité sont les trois ennemis naturels des passions

et de l'harmonie, puisque l'équilibre des passions ne peut s'établir que par un choc régulier des contraires". La leçon de Houdar de la Motte a porté chez cet homme du 18è siècle (cf. Pagès & Derghal, o.c.): "L'ennui naquit un jour de l'uniformité". Mais comme l'organisation visée est plus petite et de moindre avancement, il faudra agir sur ce paramètre de variété et passer des contrastes aux nuances.

Ainsi s'éloigne-t-il dès le départ du tout ou rien du rituel magique ou théurgique. Ce n'est pas une concession tardive aus disciples ni un accommodement, même si les disciples viennent vite à trouver plus commodes les variantes simplifiées. Il considère des minima de dimension. "En essai de première phalange il ne faudra pas réduire de plus de moitié le clavier de 810 caractères" (1822, III). En 1828 (o.c.) il préconise des "fermes d'asile" avec une quarantaine de familles formant environ 200 personnes, (soit donc selon son estimation des familles de cinq personnes en moyenne). "Il faut amener ces fermes [coopératives ou de garantie] à une ombre d'association sur l'ensemble du mécanisme, la culture, la fabrique, le commerce, la cuisine et le soin des enfants." (1822, o.c., III, p. 285). (Soulignage ajouté à cette notion d'un dégradé.)

Quoi qu'il en soit des orthodoxes infidèles et des dissidents parfois plus fidèles, dont Lehouck fait à juste titre ressortir des imcompréhensions, il est certain que Fourier lui-même s'est engagé sur la voie de la pratique sociale et que la volonté de réaliser ne l'a jamais quitté. L'effet en a été fécond parce que le principal but à atteindre [...] l'attraction industrielle, et l'équilibre passionnel, s'est ainsi dégagé des opérationalisations trop textuelles et du ritualisme littéral, aussi bien pratique que théorique. Ainsi l'expérimentalisme psycho-social de Fourier se séparait-il peu à peu du dogmatisme messianique et d'une liturgie rêvée, même si celle-ci était riche des détails les plus inventifs. Ainsi la voie était-elle ouverte à l'expérimentation et donc aux seules expériences essaybles ou faisables. On voit Fourier envisager des expériences à forte proportion d'enfants, et même préférentiellement d'âge inférieur à 9 ans, moins viciés par les préjugés; expériences particulièrement engagées sur des secteurs d'activité limités. "Deux moyens sûrs pour une petite phalange seront le renfort d'enfants impubères et la gastrosophie, par lesquels, il faut poursuivre l'équilibre des passions" et l'attraction industrielle "par masses de séries engrenées (1829, p. 384). La

spécialisation des populations dans le sens de populations plus aisément "expérimentalisables", l'analyse des fonctions susceptibles d'être "traitées" comme la convivialité gastronomique : tout cela ouvre sur l'analyse en variables multiples par démembrement du projet syncrétique ou synthétique initial. Le problème des niveaux systémiques circonscrits n'en restera pas moins posé. Il affectera profondément le mouvement coopératif. Mais l'ouverture est faite par Fourier lui-même, inconstestablement, pour un expérimentalisme à la fois plus analytique et plus libre. Elle tend à dégager des variables particulières, en termes de taille de l'organisation, de degré de constraste entre les constituants, d'âge des participants, des secteurs de sociabilité et de transformation, l'attirance elle-même à l'égard des types d'activité et des personnes étant sélectionnée. Derrière ces choix existent des hypothèses sociopsychologiques, qu'il n'est pas difficile de rendre explicites, et qui sont d'une remarquable intérèt. Par exemple, les hypothèses sur le rôle socialisant de la sexualité et de la commensalité (gastrosophie) ne sont pas précisément négligeables.

5.5.Caractère fondamental et originel de la diversification expérimentale chez Fourier

On aurait pu imaginer que cette analyse s'oppose à une sorte de fétichisme de la phalange, et que les aménagements envisagés dans l'essai pratique unique ou dans les essais soient des sortes de concessions de type réformiste par rapport à un essayisme plus radical et plus exigeant -à la façon des concessions classiques du marxisme ou du révolutionarisme. Il n'en est rien. Dès 1807, date de la rédaction du premier ouvrage, la démarche de variation de l'hétérogénéité de la composition, en fonction directe de la taille de l'organisation, est non seulement décrite, mais largement justifiée par le recours à une théorie dont le sens n'est pas toujours facile à saisir: c'est la théorie des périodes ou phases de développement qui sont en réalité des degrés d'échelle d'harmonisation, dont les échelons peuvent être plus ou moins vite traversés et dont l'acte expérimental permettrait de brûler les étapes évolutives. La phalange est elle-même une réalisation de "8è période" ou période la plus avancée dans la progression harmonisatrice. Fourier a donc construit une théorie à la fois scalaire et historique, qui permet de donner, aux variations

d'échelle et de thème d'expérience, une tout autre justification que l'accommodement avec les difficultés du terrain. Le niveau d'intervention dépend largement de la qualité du savoir atteint et de la compétence dans les opérations constructives, donc de qualités scientifiques et techniques.

Ce qui est justement très singulier, c'est, -plutôt qu'une histoire de dégradations de la théorie par l'expérience vécue, ce qui correspondrait à nos habitudes, assez cyniques, d'analyse des doctrines politiques-, un surprenant exemple de théorie qui n'a pas eu à faire de concessions dégradantes en perte de finalité. Les variations expérimentales fondamentales étaient abordées dès les premières années comme partie intégrante de l'élaboration théorique et de la préparation empirique (concurremment, il est vrai, avec un thème de l'épreuve unique et décisive. On note aussi l'étonnante maturité de l'expression de la théorie dès la Lettre de Fourier au Grand Juge, c'est-à-dire au Ministre de l'Intérieur (1804): non seulement l'attraction passionnée y est décrite, mais aussi le rôle positif de l'inégalité, les relations domestiques, l'échelle des périodes, etc. On ne peut douter que si la théorie n'a pas été une révélation transcendante, en tout cas, elle a dû surgir assez brusquement, sans doute à l'issue d'une préparation profonde, mais dans un accès d'inspiration intense. Dans son chapitre II, Lehouck (1978) a beaucoup fait pour éclairer la phase créatrice. On y voit clairement deux faits capitaux.

1) La théorie de l'échelle d'évolution, dans le sens de ce que Fourier appelle garantisme, y précède l'idée expérimentale, et s'applique plus spécialement à l'architecture et à l'urbanisme. A Paris surtout, et en voyage, les avantages d'un cadre collectif des fonctions habituellement domestiques, ont frappé Fourier commis-voyageur fort gaudisseur: il s'agissait en gros de la chambre et de la table. Le réformisme technique et cumulatif n'est pas second chez Fourier, mais premier. Le plan d'urbanisme pour la ville de Bordeaux en décembre 1796 est significatif à cet égard (P. 75 sq.) et sa reprise associationniste (1822).

2) Une "Notice inédite sur la découverte des lois intégrales du mouvement" (1819) attribue la réussite à une démarche inspirée d'un procédé d'analogie algébrique", consistant, littéralement, à supposer le problème résolu. "J'entrepris, écrit Fourier, par curiosité d'abord, le <u>calcul de la vérité supposée</u>, l'hypothèse de la vérité

générale en relation industrielle et des dispositions qui feraient naître cette vérité" (nous soulignons). C'est la démarche que nous avonds appelée, au paragraphe 5.2., l'écologisme visant le milieu humain potentiel normal. Ce que Fourier appelle véridique, ou vérité, c'est l'adéquation entre le système orexique (passionnel) des humains et leurs conditions d'existence socioécologiques. Telle est la démarche aventureuse, "case-cou" dit-il, où Fourier s'est engagé.

Il semble bien qu'à partir de cette formulation théorique, obtenue pour l'essentiel dès 1799 ou 1800, Fourier a profité, comme un scientifique, d'informations nouvelles, de descriptions, pour enrichir la théorie et les programmes d'application. La théorie n'est pas une doctrine: elle ne se dégrade pas au contact des réalités, elle se constitue, se critique et progresse malgré l'absence d'expérimentation réelle de la part de l'auteur. A défaut cependant d'expérimentation et à défaut de formalisation stricte, il est inévitable que la supposition du problème résolu oscille entre la science et la science-fiction qui s'enrichit également. C'est aussi vrai de tout modèle de ce type. Cependant on n'a pas assez pris au sérieux les modèles arithmétiques de Fourier, et les hypothèses qu'ils mettent en oeuvre, par exemple dans le calcul des "séries". Ainsi l'hypothèse de l'égalité interindividuelle du nombre des passions associatives fait partie des postulats dont la critique factuelle pourrait être fructueuse.

La tentative de Fourier faisait ressortir pas contraste les limites de ce qu'il appelait, et que nous appelons comme les médecins, "l'empirisme", c'est-à-dire en fait l'empirisme passif, en science et en action sociopsychologiques. Il nous reste à expliquer dans sa perspective pourquoi et comment l'humanité a pu se constituer un patrimoine génétique adapté à un monde qui n'existe pas. Une autre hypothèse que Fourier a abordée, c'est que ce patrimoine a correspondu à une vie "sauvage" dans des conditions démographiques et techniques très différentes; et qu'il nous reste à créer, dans des conditions profondément transformées, l'équivalent du milieu "naturel" de coaptation, à peu près comme le Phalanstère devait créer les "équivalents de la richesse" pour tous.

6. Conclusions

Fourier laissait, avec son oeuvre et ses disciples, le tracé d'une démarche expérimentale et hypothético-expérimentale suffisamment

audacieuse et inspiratrice pour être féconde. Cette fécondité est sans doute dans l'esprit de nombreux essais sociaux effectifs et dans le legs largement exploité par la pensée socialiste, y compris marxiste. Mais l'esprit hypothético-expérimental élaboré par Fourier en est généralement absent. On le trouve au contraire chez André Godin et dans ses expériences partielles, parties intégrantes d'une opération plus générale de constitution d'une association domestique-industrielle, qui sera non pas le Phalanstère mais le Familistère de Guise (Marie Godin, 1878-1906; J. Prudhommeaux, 1919; R. Pagès, 1958). Ce qui s'est produit là procède directement des souplesses de l'expérimentalisme de Fourier et sans doute de ses disciples "sociétaires" et ne constitue pas un abâtardissement mais un système à emboîtement d'expériences restreintes, résultat d'une longue préparation.

Ce qui s'est passé ensuite, bien plutôt qu'un abâtardissement, c'est une résorption de la pratique fouriériste dans le mouvement coopératiste et le délaissement de la théorie, fort dédaignée en France après 1871, défaite ressentie par la culture française (cf. Renan, 1871), mais tout particulièrement à travers la Commune, par le courant socialiste français, comme Marx l'avait espéré du sort des armes. Après ces lancements d'une remarquable vigueur, et en dehors de l'épisode d'Alfred Binet (1900), la psychologie sociale expérimentale a mis très longtemps en France pour se reconstituer. La force du centralisme étatique n'a pas du y être étrangère en tant qu'elle tend à politiser au sens des doctrines politiques toutes les sciences sociopsychologiques. Le type de pensée de Liancourt, s'il fournit beaucoup d'amorces à Fourier, était bien plus proche de la vulgate culturelle, et le reste. C'est ce qui fait apparaître aujourd'hui Fourier ou même Godin comme des chercheurs à la fois naïfs, très inventifs et très neufs, culturellement en jachère.

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POUR UNE HISTOIRE RECENTE ET ENCORE A FAIRE DE LA NOTION DE CREATIVITE: "LOI PSYCHOLOGIQUE OU LOI DU MARCHE?"

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Summary

The recent history of the notion of creativity is studied through the work of J.L. Moreno, J.W. Getzels and P.W. Jackson, R.B. Cattell, and J.P. Guilford. This history seems wrapped up with the revival of individualistic ideologies and liberal political and economic models. The notion of creativity is used in different domains of psychology (psychometrics, social, clinical, school and industrial psychology). Underlying it is an individualistic and "sociophobic" model defined by a set of opposite elements (nature-culture, individual-social, spontaneity-rationality) which are connected with a counter-cultural model or a model requiring adaptation to the changes brought about by the economic crisis.

Bien que plongeant ses racines dans des fonds philosophiques et idéologiques plus anciens, la créativité constitue en Psychologie une notion récente. Si l'Intelligence a fait l'objet de nombreux travaux de la part des historiens, sociologues des sciences ou épistémologues, travaux souvent polémiques (Gould, 1981; Lemaine et Matalon, 1985; Samelson, 1977) de par les enjeux épistémologiques, raciaux, politiques impliqués par la diffusion de cette notion et de par ses applications sociales, la créativité, ses concepts et ses pratiques n'ont pas jusqu'ici connu la même attention. Peut être parce que nous sommes trop partie prenante dans cette histoire récente, peut être aussi parce que cette notion véhicule un halo de nouveauté, d'innovation, et de critique par rapport aux conceptions anciennes et que son caractère "contestataire", "révolutionnaire", "spontané" fait qu'elle apparaitrait encore indépassable.

Telle n'est pas notre position, on l'aura compris et nous allons essayer très rapidement de pointer un certain nombre de corrélations entre le développement des connaissances en la matière au cours des trente dernières années et l'histoire économique, politique et

idéologique de ce que dans notre pays on appelle avec une certaine nostalgie les "trente glorieuses".

Récente, la notion l'est, et plus encore la diffusion populaire du mot créativité, celui-ci n'étant inscrit au dictionnaire que depuis 1976 après une tentative infructueuse en 1971 donnant lieu à un débat à l'Académie Française où se sont opposés André Chanson et Louis Armand. Le premier considérait en effet que le mot créativité correspondait à une "notion vide imposée par une mode pseudo-intellectuelle qui allait très vite se démoder" et que par conséquent il était inutile de le faire entrer au dictionnaire, le second qui a joué un rôle important dans le redépoliement du capitalisme Français et la modernisation de l'appareil productif pensait que cette entrée était indispensable dans la mesure où l'invention ne "pouvait plus être considérée comme quelque chose qui relève du hasard ou d'une élite".

L'évolution de la production scientifique depuis 1950, date à laquelle Guilford dans un article programme affirmait que "le problème du psychologue (était) celui de la personnalité créatrice" (p. 444), est impressionnante: de moins de 2‰ des Psychological Abstracts en 1950, elle suit à partir des années soixante une progression constante pour atteindre en 1970 1% des articles. Le nombre total des travaux et publications portant sur la créativité est évalué à près de 12.000 depuis 1960 dans le seul domaine de la psychologie scientifique et sans parler de la multitude d'ouvrages de vulgarisation ou d'applications pédagogiques, industrielles, artistiques ou politiques qui depuis peu ont assuré la diffusion sociale de la notion.

Aux Etats-Unis où elle apparait de manière significative dans les années cinquante, la notion de créativité a connu divers développements. D'une part, sous la forme de recherches menées dans le domaine de la psychologie différentielle et expérimentale (Guilford 1950, 1956, 1973; Getzel et Jackson, 1962; Forrance, 1962, 1966; Wallach et Kogan, 1965 etc.) et, d'autre part, sous la forme de développement "pratico-théoriques" centrés sur la résolution de problèmes (Gordon, 1961; Osborn, 1965) ou le développement personnel (Moreno 1954; Rogers, 1966) conceptions néoreichiennes, potentiel humain, etc.

Beaucoup plus tardifs dans notre pays, ces développements ont surtout été portés par les courants humanistes et du développement

personnel et diffusés dans des cabinets spécialisés (formation, publicité, etc.) dont les animateurs reprenaient et développaient quelques variantes de méthodes élaborées aux Etats-Unis. Le développement de l'étude de la créativité n'y a pas connu en psychologie différentielle et expérimentale le même succès.

Avant mai 1968, avant les mots d'ordre appelant "l'imagination au pouvoir", le discours sur la créativité apparait limité à quelques annonces d'offres d'emplois; Le Monde du 12 janvier 1968 par example, recherche: "la créativité dans l'entreprise. Des cadres créatifs: alors que la recherche, le marketing, la publicité consomment de plus en plus d'imagination, comment ne pas souhaiter développer son potentiel créatif pour contribuer à l'essors de son entreprise et garantir sa promotion".

Les journalistes économiques volontiers critiques vis-à-vis de l'appareil productif européen de l'époque trouvent des accents guerriers pour en prescrire la rénovation, et l'alignement sur un capitalisme international à la concurrence supposée impitoyable: J.J. Servan-Schreiber, dans son manifeste de 1967 Le défi américain n'hésite pas à proposer l'introduction de l'imagination au sein même de l'appareil de production: "la guerre ne nous est pas livrée à coups de dollars, de pétrole, de tonnes d'acier ni même de machines modernes, mais à coup d'imagination créatrice et de talent d'organisation" (p.11).

A propos du livre de Anthony Jay Machiavel et les princes de l'entreprise, Le Monde du 5 mars 1968 signale: "l'imagination est la reine des batailles industrielles. On ne peut encore formuler à son sujet de lois scientifiques. Du moins, l'expérience enseigne que les mouvements novateurs naissent d'un même schéma: une personnalité créatrice qui travaille entourée d'un noyau également fécond".

Quelques "Vingt ans Après" (sic) ce discours "novateur" d'une minorité est devenu, la crise aidant, un discours largement dominant (voir les Cinquièmes Assises Nationales des Entreprises du Conseil National du Patronnat Français). Un discours largement majoritaire proposant à la fois une "explication" à la crise et sa solution, hommes politiques, journalistes de télévision spécialisés, chanteur reconverti dans le conseil politique abondent dans le même sens: créer, créer pour s'adapter, s'adapter pour survivre. Parallèlement à cette mobilisation "médiatique" on assiste à un renouveau des thèses

libérales et à leur exacerbation sous différentes formes: libertariennes ou anarcho-capitablistes.

Quelle part les psychologues ont-ils pris dans ce grand mouvement idéologique? Ont-ils trouvé les "lois scientifiques" que certains appelaient de leurs voeux ou se sont-ils contentés de reproduire ces thèmes idéologiques en leur donnant un verni scientifique? Ou, encore, ont-ils fourni, légitimé un modèle du fonctionnement psychologique de la nature humaine, qui, nous le verrons, pourrait rendre théoriquement "acceptable" le fonctionnement d'un modèle général, global, essentiellement politique permettant d'appréhender de manière non contradictoire, activités industrielles, processus de production (économique, scientifique, artistique, etc.) et fonctionnement social. Ce modèle général est très largement connu: on peut le qualifier "d'individualiste et sociophobe", modèle bipolaire se structurant autour d'une opposition Individu-Social, entrainant à sa suite tout un cortège d'oppositions telles que: Culture-Nature, Intellectuel-Affectif, Rationalité-Imagination, Liberté individuelle-Contraintes sociales, etc.". (Le Disert, 1983). Cette série d'oppositions, ce modèle figuratif donnant une représentation du pouvoir et de l'articulation "Individuel-Collectif" caractérise l'ensemble des conceptions de la créativité dans les différents domaines de la psychologie. Nous avons choisi d'en observer le fonctionnement chez deux types d'auteurs représentatifs du domaine: d'une part, parmi ceux que Guilford qualifie de "philosophes", Moreno nous apparait exemplaire, et, d'autre part, parmi les "scientifiques", différienci-alistes ou expérimentalistes, Cattell dont l'évolution théorique, les renversements de position, semblent significatifs de changements de conception plus générale dans les années 50-60. Par ailleurs, un examen de littérature relative à la pédagogie et aux techniques de créativité (Osborn, Gordon par exemple) montre que les méthodes employées dans ces domaines constituent en fait une aplication du modèle (non-directivité, proscription du jugement critique, consigne de donner libre cours à son imagination etc.).

Moreno a recours à la notion de spontanéité pour caractériser la nature humaine. Pour lui: "dans l'évolution humaine, la spontanéité est apparue avant la libido, la mémoire et l'intelligence. Bien qu'elle soit la faculté la plus universelle et du point de vue de l'évolution la plus ancienne, elle est pourtant le facteur le moins développé parmi ceux opèrant dans le monde de l'homme. Elle est en

effet, le plus souvent découragée et contrariée par les mécanismes culturels" (Moreno, 1954, p.17). Cette citation résume les différentes composantes de la conception de la créativité: le caractère "naturel", "essentiel" de la spontanéité est attesté par deux antériorités, phylogénétique et ontogénétique: avant la mémoire et l'intelligence, éléments déjà sans doute trop rationnels, trop socialisés, contrôlés, on est en revanche plus étonné de voir y figurer la libido! Cette spontanéité apparait contrariée par la rigidité des cadres culturels puisque:

> il y a des modèles cristallisés (cultural conserves) qui soustendent toutes les formes d'activités créatrices comme l'alphabet, les nombres, le langage, les notations musicales. Ils peuvent opèrer, tantôt comme une discipline, tantôt comme un obstacle" (Moreno, 1954, p.16). Langage, écriture, autant de freins à la création mais également, pour notre prophète, toutes les contraintes du monde moderne: la plus grande faiblesse de notre temps réside dans l'incapacité de l'homme à opposer à la machine, à la routine culturelle, au robot autre chose que la soumission, le sabotage destructif ou la révolution sociale. Le problème de la transformation de l'homme lui-même et non seulement de son environnement est le problème le plus grand qui puisse se poser à une époque où les puissances techniques s'entendent si bien à imposer la machine, la culture standardisée et le robot (Moreno, 1954, p.357).

La conception contre-culturelle affichée par Moreno devrait en bonne logique apporter une solution à cette aliénation de la créativité, mais non, ce qui est proposé ce n'est pas une modification de l'environnement par une quelconque "révolution sociale" mais une transformation de l'individu lui-même par ce qu'il a par ailleurs qualifié de "révolution thérapeutique" libérant le "prolétariat affectif" puisque: "une bonne part des troubles psychiques et sociaux dont souffre l'humanité peut être attribuée à une épanouissement insuffisant de la spontanéité" (p.17). L'idéologie de la révolte qui dénonce le caractère coercitif de la culture et de la société se trouve alors bien émoussé et l'ambiguité apparait encore plus grande lorsqu'à plusieurs reprises il affirme: "être créatif, c'est être spontané, c'est à dire être capable de s'adapter à des situations nouvelles et par là même difficiles".

La notion de créativité se retrouve donc assez "puissante", au sens théorique du terme, pour offrir également une représentation du changement individuel et du changement collectif de l'Histoire!

Tout autre devrait apparaitre le courant différencialiste qui, au nom de la nécessité de méthodologies et d'un contrôle rigoureux,

affirme que ces "philosophes" ne pouvaient mettre en avant que des "catégories philosophiques abstraites" (Guilford).

Alors que la notion d'intelligence a déjà donné lieu à de nombreuses investigations, tout se passe comme si, le développement économique aidant, elle s'avère insuffisante à rendre compte sous la forme naturalisée d'aptitude, d'un certain nombre d'utilités sociales. C'est sans doute une telle "nécessité" qui conduit un auteur comme Guilford à élaborer un modèle de l'intellect dans lequel il distingue deux types d'opérations: la pensée convergente et la pensée divergente. "Dans la pensée convergente, il y a généralement une conclusion ou une réponse qui est considérée comme unique et la pensée est canalisée et contrôlée en direction de cette réponse"- ..."Dans la pensée divergente, d'autre part, il y a une recherche qui va dans différentes directions" (Guilford, 1956, pp.267-293).

A la pensée divergente, qui renvoie à l'aptitude créatrice, sont associées des facteurs comme la fluidité, l'originalité, la flexibilité que Guilford définit comme la faculté d'adaptation qu'il oppose à la rigidité. Le recours aux catégories naturelles dans la "définition" d'opérations cognitives fait d'une part sombrer la conceptualisation dans le modèle précédemment décrit et, d'autre part, induit une représentation typologique opposant, selon Getzel et Jackson, l'individu créatif à l'intelligent:

> l'adolescent créatif semble avoir la possibilité de se libérer de ce qui est habituel, de "diverger" par rapport aux autres. L'adolescent intelligent, au contraire, semble avoir une forte aptitude et un très grand besoin de se concentrer sur ce qui est habituel, d'être "canalisé" ou "contrôlé" pour arriver à la réponse correcte et habituelle. Il parait se dérober devant les risques et les incertitudes et rechercher la sécurité et la sûreté du connu" (Getzel et Jackson, 1973, p.44).

L'utilisation dans la description de catégories personnologiques quotidiennes permet sans doute, à travers des modèles figuratifs, d'asseoir les modèles du fonctionnement cognitif sur des basis plus "solides". L'essence de ces deux types minutieusement décrite par Landsheere (1963) laisse apparaitre des liens qu'entretiennent les aspects figuratifs dans la représentation des processus cognitifs avec le modèle diffusé:

> Devant un problème à résoudre, la pensée convergente se cantonne scrupuleusement aux données initialement fournies; elle creuse, déduit avec prudence et rigueur, sans prendre de risques, elle reste entre ses rails et aboutit à une solution qui n'est pas souvent originale, mais présente des garanties de colidité. C'est l'intelligence

> de l'homme discipliné, routinier, aussi celle du bourgeois circonscept, de l'écolier modèle, du parfait comptable, du directeur d'entreprise "solide comme un roc"... L'essence de la pensée divergente réside dans la capacité à produire des formes nouvelles, de conjuguer des éléments qui l'on considère d'habitude comme indépendants ou disparates. C'est si l'on veut, la faculté créatrice, l'imagination, la fantaisie.

Du côté des spécialistes de la personnalité, Cattell et Drevdhal (1953) à la recherche du profil moyen de la personnalité créatrice, comparent les scores de chercheurs scientifiques postulés idéotypes créatifs à celles de sujets appartenant à d'autres professions (notamment des administrateurs). A l'aide du 16 P.F., ils observent des différences significatives sur les facteurs suivants:

A (schizo-cyclothymie);

C (émotivité-stabilité émotionnelle);

M (esprit pratique-imaginatif):

Q_2 (attachement au groupe-indépendance).

En 1966, Jones et Cattell observent des différences significatives sur les facteurs M (esprit imaginatif) et Q_2 (indépendance) pour le même type de population. Les résultats montrent que les facteurs M et Q_2 dans la deuxième étude discriminent les deux types de populations plus significativement qu'ils ne le faisaient dans la première. Cette évolution dans les résultats peut être interprétée comme l'expression d'une représentation de plus en plus affirmée de l'individu créatif à travers des dimensions comme l'imagination et l'indépendance. On peut, par ailleurs, constater une évolution dans la terminologie des attributs choisis par les auteurs pour définir le facteur M, facteur de l'imagination et de la créativité. A l'origine, en 1947, lors des premières tentatives d'élaboration du 16 P.F., Cattell l'avait désigné en utilisant les termes suivants: spiessburger-Bohemianism... Ce n'est que progressivement que le facteur M va connaitre une connotation plus valorisée socialement. Cette évolution dans la conception de Cattell ne se limite pas au facteur M, mais porte aussi sur l'appréciation du facteur de second ordre: Introversion-Extraversion, superfacteur beaucoup plus décisif dans les conceptions personnologiques...

Deux textes publiés par Cattell à quelques années de distance (1963 et 1968) illustrent parfaitement le caractère approximatif, voire partial, de ce courant de recherche. Au terme de son évolution, celui-ci affirme en 1968, l'existence de facteurs communs à tous les

individus créatifs: "s'il semble y avoir des différences entres scientifiques, artistes et écrivains ce sont des différences de techniques et d'intérêts, le caractéristique fondamentale de la personne créative, original est d'être un type de personnalité" (Cattell et coll., 1968, p. 323). C'est ainsi qu'il considère que les artistes et les écrivains sont plus imaginatifs (M), plus émotifs (I), plus tendus (Q_4) que les scientifiques. Il semble que dans cette énumération Cattell se rend coupable d'un "péché par omission" puisque dans les facteurs discriminants les artistes, les écrivains des scientifiques il oublie de citer les facteurs liés à l'extraversion (E, F, H, Q_2) et ne tient compte que du facteur A (schizothymie-cyclothymie) négativement noté qui représente 10% de la variance du facteur de second ordre.

Cette omission n'est pas nouvelle on la retrouve déjà en 1958, dans une étude où Cattell avait conclu au caractère introverti des artistes et des écrivains bien que les résultats soient bien plus nuancés, puisque le facteur de second ordre introversion-extraversion se composait de cinq facteurs primaires: le facteur A (schizothymie-cyclothymie) bien sûr mais aussi le facteur E (soumission-dominance), F (desurgence-surgence), H (threctia-parmia) et Q_2 (attachement au groupe-indépendance), non pris en compte par l'auteur dans ses conclusions. Pour toute "explication" d'un tel oubli ses justifications purement idéologiques apparait dans le texte de 1963, où il dénonce avec véhémence la conception diffusée dans les milieux éducatifs valorisant l'extraverti: "Je soutiens, en dépit du fait que les écoles aient une tendresse toute particulière pour l'idéal de l'extraverti, comme si cela était synonyme de santé mentale, l'évidence indubitable que la personne créative est introvertie" (Cattell, 1963, p.129).

En fait, si Cattell valorise tellement le facteur A, c'est sans doute parce qu'il le considère comme étant le signe d'un type de supériorité intellectuelle:

> Les chercheurs scientifiques font décidément preuve de schyzothymie (A)... Mais comme d'autres facteurs de personnalité, le facteur A est aussi plus que cela, pour ce que nous connaissons de l'aspect génétique du facteur A, et des informations que nous avons quant à sa régression dans l'accomplissement de certaines activités. Par exemple, la schyzothymie est négativement corrélée avec l'accomplissement d'une activité de vendeur mais non négativement avec celle de leader (Cattell, 1963, p.121).

Dans sa conception, à la schyzothymie s'associent le sérieux, la profondeur d'esprit. L'individu créatif ne peut être qu'introverti puisqu'il possède une vie intérieure intense. Cette conception est incompatible avec la conception de l'individu créatif sociable, sensible aux relations interpersonnelles qui est en train de se mettre en place à la même époque:

> Je crois que la toute première cause expliquant cette faiblesse de performance (L'attribution de prix Nobel) est ce culte de l'extraverti dans nos écoles... Il n'est pas dans notre intention de faire de l'extraversion un synonyme de l'athlétisme; cependant, peut-être ont-ils quelque chose à voir l'un avec l'autre (Cattell, 1963, p.130).

Ce qu'il dénonce dans le "culte" de l'extraverti c'est bien l'émergence du nouveau modèle de l'individu créatif qui, dans cette évolution de l'introversion vers l'extraversion révèle la prise en compte de nouveaux éléments idéologiques tels que l'apologie du corps, de la spontanéité et l'importance des relations interpersonnelles, éléments absents de l'ancienne conception dont il a été un des tenants les plus convaincus.

Dans le texte publié en 1968, Cattell tente avec retard de se rallier à la nouvelle conception.

> Le génie scientifique typique semble être introverti et stable, mais d'un autre côté, dire que le scientifique semble, d'après les données biographiques, introverti plutôt qu'extraverti, est une constation trop simple qui demande quelques précisions... La tendance générale à l'introversion ne s'applique pas à tous les composants, mais semble être largement concentrée dans le facteur A... Mais s'il apparait être généralement vrai d'après l'étude de biographies, que les scientifiques éminents sont plutôt schyzothymiques, ceci n'est pas vrai pour d'autres composants du facteur introversion-extraversion; le facteur H met en évidence qu'ils font preuve de débrouillardise, d'adaptabilité et d'audace probablement largement constitutionnelles, sur le facteur Fn cependant, il semble bien que les éminents scientifiques du Passé n'aient pas fait preuve d'expansivité (Cattell, 1968, pp.313-314).

Pondérant sa conception antérieure il insiste sur le caractère astucieux, adaptable et audacieux de l'individu créatif, le facteur F négativement noté devenant descriptif de scientifiques "du Passé". Il tente à ce prix de justifier le bien-fondé de ses positions et de participer, moyennant quelques pirouettes, à la diffusion du nouveau modèle de l'individu créatif. Admettant le caractère "paradoxal" de la dimension introversion, il pense ainsi se rallier à moindres frais sans pour autant se déjuger scientifiquement.

> Ne tombons pas dans le snobisme intellectuel de refuser tout caractère de créativité à des actes qui se situent dans l'échelle des valeurs à un niveau plus bas que les chefs-d'oeuvre dans le domaine de l'art, de la musique ou de la littérature. La personne qui invente une nouvelle fermeture de fenêtre, compose un chant fraternel de collège, dispose des articles dans un magasin ou fait un agréable parterre de fleurs dans un jardin exécute un acte créatif (Cattell, 1968, p.268).

Idéologie de l'artisanat aidant, la transition est accomplie, la querelle des Anciens et des Modernes se règle au profit, semble-t-il de ces derniers et ce sans trop d'inquisition méthodologique.

La notion de créativité qu'elle soit appréhendée sous l'angle de l'aptitude ou de la personnalité renvoie à une circularité entre les deux approches, circularité tentant de rendre compte d'un modèle trouvant son origine et sa raison d'être dans un lieu extérieur à celui généralement reconnu au savoir scientifique.

Cette tentative de conceptualisation de la créativité marque le passage, à un moment historique donné (phase d'expansion économique et d'adaptation à cette expansion) à une vision plus "libérale", plus "démocratique" de la place de chaque individu dans la formation sociale. La naturalisation qu'elle implique et la polysémie possible: tout le monde est créatif, mais cependant, certains le sont plus que d'autres puisque suffisamment indépendants, sûrs d'eux, originaux, pour s'affranchir du contrôle social ou de la routine ("les entrepreneurs") rendent possible aujourd'hui une idéologie de sortir de crise: le manque de créativité individuelle explique la crise et l'échec, l'intérêt collectif exigeant alors que les "meilleurs gagnent". Ce faisant elle remplie bien sa fonction idéologique, à savoir d'expliquer la totalité du fonctionnement de l'individu et de la société et, de trouver une alternative idéologique de type réformiste à opposer à des conceptions plus radicales. Dans cette perspective, "les philosophes", Guilford, leur détracteur et Cattell le factorialiste rigoureux se rejoignent. Pour Guilford

> nous manquons sérieusement d'ingéniosité et d'énergie pour résoudre les problèmes sociaux... sans aucun doute, nous vivons des temps très agités. La destruction sociale est souvent prophètisée par les pessimistes... Mais je pense qu'il existe aussi un pourcentage important d'optimistes qui ont foi dans la nature humaine et en ses possibilités de résoudre les problèmes de notre temps... Je crois que c'est H.G. Wells qui a dit un jour que l'avenir de notre civilisation dépendait d'une course entre l'éducation et la catastrophe. En accord avec lui, nous voudrions réviser sa formule en ajoutant après

éducation l'adjectif créatif (Guilford, 1973, pp.260-262).

Cattell quant à lui, se trouve prêt à intégrer des notions issues du domaine des aptitudes cognitives pour nous fournir notre citation de conclusion: "Il nous faut des sociétés avec des citoyens à l'esprit flexible qui sont capables de concevoir des solutions constructives et créatives aux problèmes de gouvernements, car il faut progresser par une évolution pacifique au lieu de lutter à travers des révolutions" (Cattell, 1968, p.268). Conclusion scientifique au terme d'une vie de "recherche" ou testament politique à une époque où libéralisme oblige, les élitistes et autres eugénistes ne peuvent qu'avancer masqués?

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HEYMANS' AND WIERSMA'S QUESTIONNAIRE FROM 1905 REANALYZED IN 1985[1]

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Summary

At the beginning of this century G. Heymans, philosopher and founder of psychological research in The Netherlands, and D. Wiersma, a well known psychiatrist, devised a questionnaire which was originally intended to explore the heridity of psychological traits. The questionnaire consisted of 90 questions which referrred to divergent aspects of the human character. These questions represented, in everyday language, Heymans' theoretical model of personality. Over 400 people, mainly Dutch physicians, completed the questionnaires which involved describing the members of one or more families whom they knew well. Thus nearly 2500 descriptions were received by the investigators. The study has become well known mainly as a result of the analysis carried out by Heymans which related all responses to three special traits: activity, emotionality and the so called 'secondary function'. He founded a typology by relating each question to every combination of these three traits of temperament. Van der Werff and I, assisted by other faculty-staff, have taken the original forms which are still kept in the archives of our museum and have put all these data into a computer file. This has allowed us both to check Heymans' results and also to reanalyze this interesting material. The results of this analysis are reported in the present paper.

The aim of the present paper is to take a fresh look at the study of the heridity of personality traits which was carried out by Heymans and Wiersma, both working in Groningen in The Netherlands at the beginning of this century.

Wiersma lectured on psychiatry and neurology from 1897 until 1930 at the University of Groningen. His fame as a psychiatrist on the one hand and his personal and scientific contacts with Heymans on the other guaranteed that their cooperative enterprise would become a successful one.

Heymans was teaching philosophy and psychology in Groningen at about the same time as Wiersma, from 1890 until 1927 and is

recognized as the founder of psychology in The Netherlands. Heymans' earlier studies were of the experimental type. They investigated optical illusions and other sensory effects. Heymans therefore made a swing from Wundtian to Galtonian research practice, just as so many others had done at that time (Danziger, 1983).

However, he never lost sight of the need to explore causal processes operating in individuals in order to explain the differences and resemblances between individuals. He therefore set out to compare the constellation of individual dispositions with the forces, such as gravitation, which cause physical changes in nature. In the same way as an object which is left free in space is attracted by gravitation and is thereby thrown to the earth, so, Heymans argues, the thought of some motive, if it comes into one's consciousness, will be changed by one's inclinations into the corresponding action. Heymans argues that the forces which bring motives into one's mind are governed by individual temperamental characteristics.

The questionnaire we shall discuss was sent by Heymans and Wiersma on the 15th of April 1905 to all Dutch medical practitioners (3000 in those days) and to some other people. The aim, as explained in an accomanying letter, was to gather facts about the heridity of normal psychological traits. Everyone was asked to fill in as many forms as they wished with respect to adult members of a family of their acquaintance.

This questionnaire was addressed to the medical profession because of their supposedly excellent scientific interest and their insight in human nature.

Early in January of the next year, 1906, Heymans and Wiersma had received the data of 437 families. This meant that 2415 character descriptions could be analyzed. The responses of each subject were noted on a small preprinted card. Heymans must have been an outstanding systematic and orderly man. He not only kept these cards but also the original inventories. All this material is still in good condition and perfect organizational order and can be viewed in the museum of Groningen University.

My colleague Van der Werff and I started to put this extremely interesting 80 year old material into a computer system. This has been no mean task. One of the faculty staff who carried out this work, Mr. M.P. Kost, had to punch in over half a million ones and zeros before any analyses could be carried out.

I shall not discuss the original aim of the inquiry -the heridity-research of personality traits, but shall confine myself to he structure of the questions. I will concentrate on the theoretical analysis of the interrelations of the responses given to various questions.

The lay-out of the questionnaires was as follows: first of all came three groups of about 10 questions concerning temperament; next came about 20 questions concerning intelligence; and this was followed by about 40 questions concerning dispositions. Finally various other questions ended the questionnaire.

The three groups of temperamental questions we shall discuss further relate to questions about 'activity', 'emotionality' and about the so called 'primary or secondary functioning' which Heymans described as being the degree of influence of past consciousness upon present behavior. These three groups of questions had a specific place in the study as they served as fundamental traits to classify the other responses to questions which dealt with intelligence and dispositions. From his earlier work, the analysis of biographies of famous people, Heymans had learned that these characteristics could effectively be used as a means for classifying other traits.

The significance of such a classification lies not in the fact that it is true or false, but in its usefulness. If one can classify people, one can predict more of their traits. Examples of classification can perhaps clarify this point: compare the classification of Linnaeus. If a plant belongs to a particular family, we also know a number of its details.
Similarly with the classification of Mendelejew: using his classification of chemical elements it is possible to predict the properties of substances we have not even found yet. Therefore it appears to be very useful to have optimal criteria for a good classification.

We will now first trace the historical sources from which the criteria Heymans used for this threefold temperamental classification were developed.

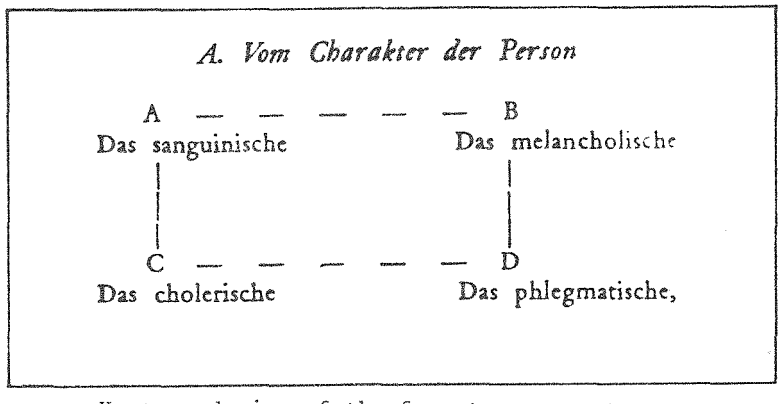
An old tradition, from Hippocrates, about 350 BC on, assigned individuals to the four well known temperaments which were named after the supposed dominance of the body fluids:

- sanguinic (derived from the Greek word for blood), meaning: cheerful, easy-going;
- phlegmatic (from phlegma), meaning: quiet, slowly;

- choleric (from gall), meaning: irascible, impatient;
- melancholic (from black gall), meaning: depressed.

This doctrine of 4 temperaments for classifying people was taken over by Galenus about 150 AD and in the Western world in the 17th century by J. de la Bruyère. Immanuel Kant described these 4 temperaments in his Anthropology in 1800. He ordered them as follows:

Fig. 1



Kants ordering of the four temperaments.

Two points are of particular interest to us in this way of ordering:

1. Each pair is taken as a contrast.
2. Both pairs of temperaments are seen as judgments along a specified dimension.

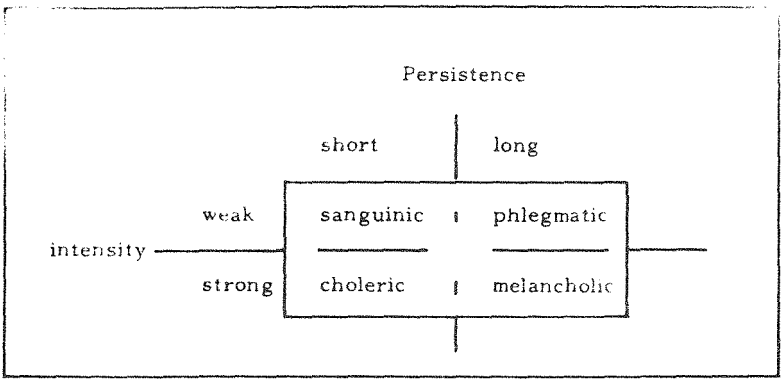
This ordering of the four temperaments was interpreted by Heymans as meaning that the temperamental differences are the result of two causal factors. These causal factors he called 'emotionality' and 'activity'. Kant's judgment-dimension, action (German: 'Tätigkeit') seemed to Heymans to be a factor which causes activity (German: 'Aktivität') and Kant's judgment-dimension, mood (German: 'Affektivität') became for Heymans the factor which causes emotionality (German: Emotionalität).

The terms 'activity' and 'emotionality' resemble the names 'les actives' and 'les emotives' which Malapert (1897) used in his typology some years before Heymans.

Heymans may have been guided to this interpretation of the differences between temperamental characteristics by the work of the

famous German psychologist, Wilhelm Wundt, who was about 25 years older than Heymans. Wundt tried to reduce the differences between the 4 temperaments to fundamental aspects of emotional life namely to persistence and to the intensity of the emotions.

Fig. 2



Wundt's analysis of the four temperaments.

This point of view is different from that of Kant in two ways.

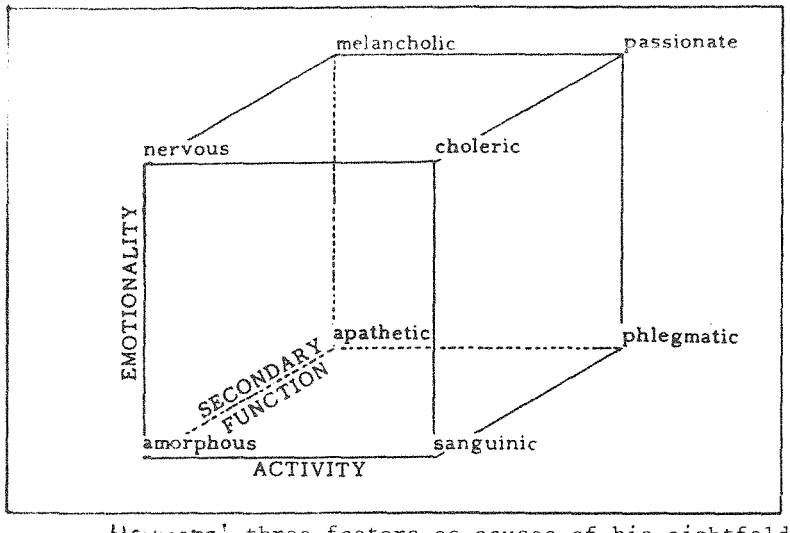
1. Wundt finds a different ordering.
2. Wundt gives no dimensions of judgment but an analysis by which he relates the differences in temperament to more general descriptive terms of inner life.

Heymans also used the general ideas of Wundt's grouping in forming his own system of causal factors of temperamental differences. He ascribes the differences in intensity to the factor 'emotionality' and the difference in persistence to the factor 'secondary function'. With 'secondary function' Heymans implied the grade of influence of past consciousness upon present inner and outer behavior. The opposite he called 'primary function', which therefore implies the influence of the present upon behavior and thinking. Heymans and Wiersma borrowed the term 'secondary function' from a neurologist from Vienna, Otto Grosz, who described this concept in a little book, published in 1902, Ueber die zerebrale Sekundär Funktion (in English: "About the cerebral secondary function"). The meaning of this term for Grosz, however, was the characteristic of some people to have isolated groups of associations.

Heymans therefore borrowed a descriptive concept and reformulated it as a causal factor to interpret differences between people.

Up to this point we have seen where Heymans obtained the three factors which in his opinion were the cause of differences in temperament.

Fig. 3



Heymans' three factors as causes of his eightfold temperamental classification.

Three out of the four new items which do not correspond to the old temperamental categories can already be found in Malapert's typology. He had called them 'apathiques', 'passionnées' and 'amorphes'.

For his questionnaire, Heymans could of course not use these theoretical concepts directly but he had to rephrase the questions into everyday language. Heymans then used some of the questions as criteria for his classification.

For 'activity' three terms were chosen as criteria:

2. In occupational, trade, school and household duties always industrious, or only working by fits and starts, or usually lazy?

3. Even in leisure hours mostly occupied or prone to take it easy?
5. Prone to delay, or habitually attacking everything energetically and completing it?
For 'emotionality' only one term was chosen as a criterium:
9. Emotional or not emotional?
Because of his uncertainty with the concept of 'secondary function' Heymans considered all ten questions as constituting the criteria.
17. Easily comforted or long unconsolable?
18. Easily reconciled or difficult to reconcile?
19. Varying sympathies or firm in affections?
20. Attached to old remembrances or occupied by new impressions and friends?
21. Opinionated or easily persuaded?
22. Being fond of change or sticking to routine?
23. Constantly changing field of activity?
24. Often engaged in planning large ventures which never materialize?
25. More swayed by future plans or by immediate results?
26. Activities which harmonize with or are contrary to one's principles?

Using these criteria Heymans was able to divide his cardboard copies of the questionnaire-forms into eight files, each of which represented one class of temperament. He could now also count for each class the number of responses given to the other questions about intelligence, dispositions and so on.

The advent of the computer has made it possible to compare the outcomes of every question with every other question so that we can see from correlation coefficients or cluster-analysis which classification and questions give the best results.

We shall present our results of the intercorrelations of the responses given to the alternatives of the first 26 questions which Heymans designed for his classification of temperaments.

For the 'activity' questions we have found two clusters:

I		
2.	Always industrious	Working by fits and starts
3.	Mostly occupied	Take it easy
4.		Prone to neglect obligatory work for non obligatory
5.	Attacking everything energetically and completing it	Prone to delay
II		
5.	Attacking everything energetically and completing it	Prone to delay
8.	Resolute	Irresolute
6.	Persevering	Easily discouraged

In the above table the four terms we have found in our first activity cluster and also the three terms we have found in the second

activity cluster are presented. Question 4 consists of only one alternative. Question 5 appears in both clusters.

As we can see from these results 'activity' has two meanings. The terms 2,3 and 5 which Heymans used as criteria for activity all belonged to the first of the two clusters. This shows that we can now find with much more precision than could be achieved in Heymans' day the terms which together define the classes of a classification. It also highlights Heymans' good intuition in choosing satisfactory criterion terms.

These two activity clusters correspond remarkably well with Malapert's distinction in "les vifs" et "les lents": "Les vifs, ayant un besoin permanent, d'agir pour agir" and "Les lents, plus continue plus persévérante, du moins se maintenant plus facilement dans la même direction".

For the 'secondary function' questions we have found only one weak cluster:

20.	New impressions and friends	attached to old remembrances
22.	Fond of change	Sticking to routine
19.	Varying sympathies	Firm in affections
26.	Activities contrary to principles	In harmony with principles
24.	Planning large ventures	
25.	Immediate results	Future plans
17.	Easily comforted	Long unconsolable
23.	Constantly changing	
16.	Gloomy	Light hearted

We have presented here Heymans' eight 'secondary function' terms and one of his emotionality terms which fits into our 'secondary function' cluster.

We have also found two clusters for the terms of Heymans' emotionality factor. Moreover, it is shown that two of the questions (1 and 7) which Heymans originally designed as activity-items in fact fit better into these emotionality clusters. The most remarkable feature however, is that from the analysis of Heymans' data we have again retrieved the old four temperaments which formed the starting point of Heymans' theorizing. These are shown in the Table below, together with the terms of the two emotionality clusters which we have found.

I		
'Choleric'		'Phlegmatic'
10.	Violent	Cool and business-like
1.	Mobile and busy	Calm and quiet

7.	Impulsive	Deliberate
15.		An even temper
9.	Emotional	Unemotional
11.	Irritable	Good tempered
II		
'Sanguinic'		'Melancholic'
11.	Good tempered	Irritable
12.	Idealizing	Critical
13.	Trustful	Suspicious
14.	Tolerant	Intolerant

From the questions that fit these two emotionality clusters one particular question, number 11, fits both. Although one of the alternative responses to question 15 "alternating happy and depressed" did fit into the first cluster it was not very strong.

Although the order of clusters 1 and 2 is reversed in comparison to those of Kant which we showed earlier, this is due to the order of the questions and has therefore no further significance.

Heymans, starting from old literature of the four temperaments, made an effort to develop hypotheses about their causation. From these supposed causes, he then derived questions which he expressed in everyday speech. From a further re-analysis of the responses to this questionnaire we have rediscovered the original four temperaments. The gain is nevertheless remarkable. Not only have Heymans' data shown two activity clusters and one weak secondary function cluster but moreover, we have now questions which can operationalize the old four temperaments.

Moreover, we now know by our analysis that neither 'activity' (Heymans' interpretation of Kant's 'Tätigkeit') nor 'secondary function' (Wundt's 'duration'-aspect of emotions) has anything to do with these four temperaments.

Heymans has not been very fortunate in his explanation of the temperamental differences but, on the other hand, he did have a very good intuition both for finding the right formulation for his questionnaire and also for his selection of the criterion terms for his classification.

1.The author wishes to thank Mrs. J.L. Jackson-Roy for the translation into English.

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DE "L'HERESIE PHYSIOLOGIQUE" A "L'OBSCENITE SCIENTIFIQUE" L'INNOMMABLE DANS L'HISTOIRE

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Summary.
Recently reprinted, the work of Marc Bloch on the omnipotence of kings (Les Rois Thaumaturges) provides an example of a stumbling block for historians confronting problems in their own field and by means of their own concepts that belong to a different conceptual framework. In Bloch's work, the Freudian system would have been capable to account for the specificity of the mental processes that may underlie the eruption of certain disconcerting events. When analysing the various pseudo-solutions and impasses followed by Bloch in order to avoid discussing "things Freudian", it becomes clear how much remains to be done if we are to understand the phenomena which demarcate the transition from the political realm to the religious.

> "L'unique médecin du peuple, pendant mille ans, fut la Sorcière. (...) Quand Paracelse, à Bâle, en 1527, brûla toute la médecine, il déclara ne savoir rien que ce qu'il apprit des sorcières. Cela valait une récompense. Elles l'eurent. On les paya en tortures, en bûchers. (...) Notez qu'à certaines époques, par ce seul mot Sorcière, la haine tue qui elle veut."
>
> Jules Michelet. La Sorcière

Que se passe-t-il lorsqu'en un quelconque endroit de son "territoire", l'historien, le politologue aussi bien, se trouve confronté au surgissement, où à l'insistante existence, de phénoménes dont l'approche, l'analyse et la connaissance semblent rebelles à toute prise conceptuelle en usage?

A l'endroit de la question cruciale du pouvoir, ce genre de situation ne manque pas de se produire qui semble rendre muets ou hébétes ceux des spécialistes qui s'y trouvent convoqués.

Aux confins de l'histoire, de la politique et du droit, se profile ainsi cette abrupte et essentielle donnée que constitue la dualité du pouvoir: le pouvoir comme fonction et le pouvoir en tant

qu'il désigne, abusivement, au moyen d'une opération langagière qui n'est pas anodine, la personne qui l'exerce, qui en joue ..., qui en jouit.

En "Occident", si l'on veut bien un instant n'être pas trop regardant quant aux fondements et à l'adéquation de cette expression, les formes modernes du pouvoir reposent, en théorie du moins, sur cette distinction, celle qui intervient entre la fonction et la personne; la dualité du pouvoir se trouve donc être la réafformée, proclamée, à défaut d'être respectée dans sa plénitude. Pour autant, il s'agit la de notre Occident moderne et il n'en a pas toujours été ainsi. On doit à Ernst Kantorowicz (1) d'avoir éclairé le long cheminement qui, depuis les lendemains de l'an mille jusqu'à l'orée de notre temps, a été celui de l'évolution des formes de pouvoir et de la mise en place de cette dualité dans les textes de loi. Cheminement scandé par ces étapes, celles qu'Ernst Kantorowicz nomme du "Roi-Christ", du "Roi-Loi" et enfin du "Roi-Corps politique", qui conduit vers une laïcisation officielle du pouvoir, laquelle n'exclut pas la persistence de formes de religiosité plus subtiles que celles affichées aux temps prégrégoriens, le centralisme constituant l'une de ces religions dont Pierre Legendre (2) rappelle qu'elles n'ont pas plus que les canonistes, besoin de Dieu pour exister.

A chacune de ces étapes, la dualité du pouvoir, la distinction entre la fonction et la personne, entre le corps public et le corps privé du monarque ou du président, sont choses qui ont été un peu plus marquées dans les textes mais aussi dans les faits; la montée vers les formes modernes de l'Etat, caractérisées entre autres choses par ce que Marcel Gauchet - lecteur averti de Kantorowicz - désigne comme un mouvement de "suprapersonnalisation" (3), a été sous-tendue par ce passage dont Ernst Kantorowicz souligne l'importance autant que la complexité, celui de la psychologie collective à l'action politique.

Mais quant aux lois qui pourraient régir ce mouvement de transformation fondateur, quant à celles mises en jeu par ce processus de "suprapersonnalisation", elles semblent bien aujourd'hui encore être considérées comme celles d'une mystérieuse alchimie comptable de notre ignorance et de nos "résistances".

C'est dans la sphère de ce bouillonnement où voisinent le miraculeux et le magique, le divin autant que le diaboloque, qu'il y a lieu de situer de l'innommable et de repérer ce qui vient tenter de

le recouvrir, une psychologie ancrée tout entière dans l'ordre du biologique, appendice éphémère de la neurophysiologie moderne.

Un exemple particulièrement révélateur et instructif de ces manoeuvres d'évitement et des impasses auxquelles elles ne manquent pas de conduire au nom de la rationalité et de la scientificité toutes puissantes, nous est donné par le travail qui fut celui de Marc Bloch, cet historien martyr fusillé par les nazis en 1944, fondateur avec Lucien Febvre de l'Ecole des Annales, travail destiné à rendre compte d'un ensemble de manifestations qu'il regroupa sous le titre original des Rois Thaumaturges (4).

Les phénoménes auxquels Marc Bloch portait ainsi une attention aussi érudite que soutenue étaient de deux sortes: guérison des écrouelles ou scrofules (adénites tuberculeuses) au moyen du toucher par la main royale - cela se passait à la cour des rois de France - distribution, à la cour des rois d'Angleterre, d'anneaux consacrés, les "cramp-rings", censés appaiser les manifestations plus ou moins abusivement regroupées sous le lable de l'épilepsie.

Ce qu'il pouvait y avoir d'audace, pour un historien débutant, à vouloir faire de telles manifestations, plus proches de l'anecdote et de la "petite histoire" que de l'événement majeur, le matière d'un travail d'envergure, cela n'échappait pas à Marc Bloch qui répondit aux objections avancées par plus d'un que ces cas de "guérison" étaient bien connus des érudits et des curieux et qu'il entendait démontrer qu'avec "... ce qui n'était jusqu'à présent que de l'anecdote (...) on pouvait faire de l'histoire." (5)

Tant qu'il sera question de rendre compte du détail de ces événements, de donner au lecteur connaissance de leurs développements, de leur chronologie et de tous leurs à côtés, soit pendant plus des deux-tiers de l'ouvrage, Marc Bloch excelle, se révélant être le grand historien que l'on sait. Là où les choses s'obscurcissent au point de retenir notre attention, c'est lorsque Marc Bloch, dans la toute dernière partie de l'ouvrage, se fixe pour objectif de comprendre la nature de ces manifestations, lorsqu'il envisage de répondre à la question du "Comment ça marche?"

Dans un premier temps, l'historien rencontre les explications qui accompagnèrent la naissance et les premiers développements de ces phénomènes, celles qui se réfèrent au registre du surnaturel et qui alléguent d'une identité entre ce pouvoir de guérison et la miséricorde divine. A ces explications prises dans la trame de la

conception religieuse du pouvoir politique furent opposées, dans les temps qui suivirent, des critiques de deux ordres: les unes que l'on peut dire d'humeur, sarcastiques, ironiques, mais peu souvieuses d'aller au fond des choses, dues aussi bien à Montesquieu qu'à Voltaire, les autres, désireuses de comprendre et de rendre compte de ce qui advenait ainsi, souvieuses d'user d'autres moyens que ceux de la religion et que l'on peut regrouper sous l'étiquette "naturaliste".

Dans le cadre de ce second ordre d'explication, la distinction entre la fonction royale et la personne du roi n'était pas plus établie que précédemment mais l'argumentation tendait à se diversifier. L'essence du phénomène s'y trouvait d'abord rapportée à la nature de la personne royale, sa nature organique, physiologique; le toucher miraculeux s'expliquait alors par le type de nourriture, épices, plantes, qu'absorbaient les personnes princières et les propriétés ainsi acquises se transmettaient par voie de sang au fil de la dynastie. Mais cette essence pouvait aussi être référée, et il y a là une avancée de taille, à ce que nous appellerions aujourd'hui la nature de la demande adressée au pouvoir et à la personne royale confondues. L'idée se fait jour, dans cette perspective, d'un registre spécifique, propre au phénoméne observé, le registre de l'imagination, entendu non comme la négation ou la dérision de ce dont on parle mais tout au contraire comme la reconnaissance de son caractère irréductible, somme l'identification d'un espace dont l'existence serait sous-tendue tout à la fois par le désir de guerrir et la fascination, effroi ou émerveillement, produite par le pompe de la cérémonie du toucher et par le rituel entourant toute apparition de la personne royale.

Confronté à ce deuxième aspect de l'explication "naturaliste", Marc Bloch réagit négativement en faisant reproche de sa crédulité à cette démarche mais plus encore en réfutant toute idée d'un registre spécifique, propre au phénomène observé, le registre considéré comme étant celui de l'imagination, et en usant à ce propos d'arguments qui vont nous précipiter vers le centre de gravité de cette affaire, vers ce qui pour l'historien relève tout à la fois de l'innommable et de l'obsédant: "Le toucher eut été en somme, écrit Marc Bloch à propos de cette avancée "naturaliste" et avec une pointe d'ironie agacée, une sorte de psychothérapie (et) les rois autant de Charcot sans le savoir." (6)

L'opposition entre l'explication qui fait appel au registre du surnaturel et l'argumentation naturaliste va se trouver parasitée et bientôt mise à l'écart au profit d'une autre dans laquelle Marc Bloch ne sera pas seulement un observateur s'efforçant d'être neutre mais un partisan cédant comme tel à la polémique.

Cette fois, l'argumentation naturaliste va céder le pas à une construction organisée autour de l'idée de raison.

Sous cette nouvelle bannière, celle du rationalisme, vont être conduites de modernes et civiles croisades destinées à combattre ces infidèles à la science que sont les défenseurs d'un espace qui serait celui de la chose psychique.

Situant d'emblée les termes et le tonalité du débat, Marc Bloch s'étonne, non sans quelque condescendance, de ce qu'il puisse y avoir encore, au moment où il écrit et alors que les sciences physiologiques et neurophysiologiques ont connu d'immenses avancées - on parlerait sans aucun doute aujourd'hui des perspectives nouvelles offertes par le développement des neurosciences - des gens pour croire à cette idée, celle d'une "... explication psychothérapique du miracle royal." (7)

L'étonnement de l'historien loin d'être feint est double, il porte aussi bien, on va le voir, sur les faits eux-mêmes et sur la croyance dont ils peuvent être l'objet que sur l'idée qu'il pourrait y avoir, de ces faits mais probablement de faits de même nature, une "... interprétation psychique...". (8)

Cette "interprétation psychique" dont Marc Bloch fait ainsi état en 1924 n'est autre que celle qui fut élaborée entre les années 1870 et 1890 et dont Charcot fut le maître d'oeuvre. Aux questions que ne manquait pas de poser l'existence répertoriée de cas de quérison par le toucher royal, mais à d'autres aussi bien d'un ordre voisin, il était répondu et mis en évidence à la Salpétrière que certaines formes de plaies, lésions ou oedémes pouvaient fort bien être les traces de manifestations hystériques, ce qui expliquait qu'elles puissent aussi bien disparaître suite à l'influence d'un "... autre ébranlement de même nature." (9).

Au lieu de se demander si ce point de vue avait été dépassé, si la théorie du fonctionnement psychique avait connu des développements autorisant à aller plus avant dans la compréhension du phénomène, Marc Bloch considère que l'interprétation psychique est toute entière contenue dans cette approche déjà ancienne. Partant, cette

"interprétation psychique" qui ne l'a en réalité jamais véritablement retenu lui apparait comme totalement disqualifiée au regard des connaissances scientifiques modernes, celles de 1924, lesquelles, à quelques détails nouveaux près, reprennent pour l'essentiel les arguments qu'utilisèrent les artisans du "démembrement et (du) démantèlement" de Charcot et de son oeuvre (10).

Dans cette perspective où la simple idée d'une dimension psychique non immédiatement réductible à un substrat organique semble irrecevable, il n'y a évidemment pas de place pour la "névrose hystérique" identifiée par Charcot ni pour ses diverses formes d'expression. Seules deux formes bien distinctes de manifestations sont alors reconnues: celle qui, la guerre aidant, prendra le statut de trahison patriotique, à savoir ce qui est considéré comme étant de l'ordre de la simulation et celle qui, étant identifiée comme une atteinte organique, exclut toute dimension "nerveuse" ou psychique. Pas d'ordre psychique donc, pas d'hystérie, recouvrement et ignorance de la position féminine reconnue et magistralement défendue quelques soixante années plus tôt par Michelet dans La Sorcière, le choix n'est plus qu'entre l'imposture et la maladie.

Le cas de la simulation ne faisant l'objet d'aucune interrogation critique se référant à une perspective sémiologique qui ouvrirait à l'idée de symptôme mais étant au contraire inscrit dans un ordre moral dont la seule issue parait bien être, en temps de guerre, le peloton d'exécution, il ne retient pas l'historien. Reste donc la seconde catégorie, celle de la maladie.

Peut-on alors imaginer que dans ce cas, celui de la maladie, certaines individualités, investies de la fonction monarchique aient pu faire oeuvre de guérison en usant pour cela d'un hypothétique pouvoir de suggestion? Cette question, Marc Bloch la pose à des spécialistes qui s'empressent de lui répondre qu'à soutenir une pareille thèse on ne manquerait pas de s'inscrire dans "... l'hérésie physiologique"! Hérésie! Le rationalisme a décidément recours à de bien curieux termes lorsqu'à tort ou à raison il se sent mis en cause et l'on peut déjà dire que ça n'est pas raisons que nous invoquions à l'instant la mémoire de Jules Michelet.

Mais d'où vient cette réponse et surtout d'où vient cette idée, ce questionnement de Marc Bloch concernant une possible guérison par la suggestion?

Il s'agit là d'un emprunt, effectué non sans quelques imprécisions, que l'historien fait à Babinski, cet élève de Charcot qui sera le maître d'oeuvre de la dissolution du concept de névrose hystérique au moyen duquel Charcot avait cerné l'espace spécifique des manifestations psychiques, qui sera l'artisan de son remplacement par la notion, combien plus rassurante pour l'ordre médical de "pithiatisme", notion qui lui semblait permettre une dissociation entre ce qu'il pouvait en être de la simulation d'une part et de la maladie d'autre part. C'est dans ce contexte, celui de la première guerre mondiale qui occasionna un retour en force des manifestations hystériques - dépressions, crises nerveuses, mutilations volontaires etc... - que Babinski fut amené à recourrir à la suggestion pour tenter de faire disparaître des manifestations qu'il continuait silencieusement à considérer comme de la simulation mais dont la désignation comme telle eut conduit ceux qui en étaient les sujets devant les tribunaux militaires puis les pelotons d'exécution.

Bien qu'ayant été lui-même un acteur de cette guerre, tant au front que devant ces tribunaux militaires où il eut à défendre des soldats suspectés d'avoir cherché à se faire réformer frauduleusement, Marc Bloch semble bien ignorer ce qu'il en fut de la supercherie de Babinski: d'où sa question et son appaisement au su de la réponse. Il peut alors conclure au fait rassurant que les cas de guérison par le toucher royal n'étaient que fables, mise en scène destinée à entretenir l'illusion de la toute puissance royale. Quant aux adénites tuberculeuses, les "vraies"! il n'était qu'à s'en remettre pour ce qui était de leur guérison aux progrès de la science médicale.

Demeure alors une question qui ennuie fort l'historien: pourquoi tant de gens ont-ils cru à la guérison par le toucher royal? Pourquoi tant de milliers de pauvres gens se pressaient-ils à cette cérémonie, manifestant en cela leur demande et plus encore leur certitude d'une possible guérison? Notons-le au passage, de telles questions peuvent être posées à propos de bien d'autres événements: il n'est qu'à songer aux foules de Nuremberg, aux contemporaines prières de Téhéran ou à l'affliction du mouvement Communiste international lors de la mort de Staline parmi d'autres exemples possibles pour réaliser que de tels faits mettent en jeu bien autre chose que la dimension anecdotique sous laquelle ils sont généralement présentés, pour comprendre qu'il est là question du passage déjà évoqué qui conduit

de la psychologie collective à l'action politique. Mais, soit qu'il ne saisisse pas l'importance du probleme, soit qu'il éprouve de la gêne à son endroit de la croyance du plus grand nombre dans le pouvoir thaumaturgique d'un, qu'un expédiant des plus lapidaires, celui qui tient dans l'idée qu'il s'agirait là d'une "erreur collective" (11), par où il retrouve la problématique de la suggestion, les conceptions de Gustave Le Bon dont il avait connaissance et dont un cadre paradigmatique organisé autour des idées d'illusion et de faux.

Confronté à un phénomène historique dont les racines et les retombées concernent cette donnée centrale de la théorie politique qu'est la question de la dualité du pouvoir et ses prolongements sous la forme de la dérive permanente du politique dans le religieux, l'audacieux historien rate la possible ouverture, il demeure prisonnier de l'alternative rationaliste qui oppose le vrai de l'organique, du physiologique et du besoin au faux de l'imaginaire, de la demande ignorante de son objet, de la manifestation psychique si intempestive, si inclassable et si dérangeante qu'elle en conduisit plus d'un et plus d'une au bûcher.

Mais en 1924, en conservant l'irremplaçable référence que constitue l'écart inaugural de Charcot, une autre voie était possible, celle d'un dépassement du maître de la Salpétrière qui ne soit pas un démantélement de son oeuvre, la voie de la reconnaissance maintenue de la spécificité de la dimension psychique, la voie freudienne de la prise en compte de l'inconscient et de l'ordre du désir, irréductible à l'ordre du besoin. Fait important bien que rarement souligné, cette voie là atteste silencieusement de la dualité du pouvoir, elle distingue d'emblée, de manière constitutive entre la fonction et la personne. C'est en effet en isolant la dimension du transfert, avec ce qu'elle suppose de partition entre la personne réelle, privée et la personne publique, investie des emblêmes du pouvoir et vécue à ce titre comme nantie, douée d'un pouvoir, celui par exemple de ... guérison, que Freud trouve une issue au piège ... démoniaque qui ne nous donnait à choisir qu'entre la simulation et la maladie.

Mais la compréhension aussi bien que la prise en compte de cette dimension transférentielle implique la reconnaissance de l'ensemble de la théorie freudienne, la prise au sérieux - le mot est de Freud - de l'inconscient et de ce qui le constitue, le processus de refoulement par où la pulsion sexuelle trouve à se rejouer sous des formes

non immédiatement identifiables comme telles. De cet apport décisif par où s'accomplit le dépassement et non la destruction de l'avancée de Charcot (12), non seulement Marc Bloch ne semble pas avoir eu connaissance en dépit de sa pratique de la langue allemande et de sa culture germanique, mais pire encore, il semble en avoir été résolument détourné à considérer ce jugement décisif de son collègue et ami Charles Blondel concernant l'oeuvre freudienne: une "obscénité scientifique" (13)

Entre "l'hérésie physiologique" et "l'obscénité scientifique", quelque chose d'innommable demeure dans le champ de l'histoire et de la politique, quelque chose qui échappe à l'historien désireux pourtant de faire de l'histoire avec ce que les autres considéraient comme de l'anecdote. Ce qui est ainsi laissé pour compte et qu'avaient entrevue les "naturalistes" qui soutenaient la thèse de l'imagination, c'est que la question de la guérison ne se pose pas comme telle, qu'elle n'a de sens qu'à être liée à cette autre que Marc Bloch a malencontreusement isolée, celle de la croyance. La "guérison" ne s'opère que dans l'espace transférentiel engendré par la croyance des demandeurs dans le pouvoir d'un qu'ils ont identifié à la fonction. Que cette dimension transférentielle qui se soutient d'un trait - marque, emblême, voix, nom - soit au coeur de la psychologie collective, c'est là ce que Freud a reconnu dès ... 1921! Qu'il y ait là l'amorce d'un mouvement vers l'action politique caractérisée notamment par l'attente, la demande d'un secours, d'une servitude adressées par tous ou presque à un ou à son nom, c'est là quelque chose qui fut tôt souligné par La Boétie et que Freud à son tour ne manqua pas de relever, dans L'Avenir d'une illusion notamment où l'on peut lire que "Les opprimés peuvent (...) être atachés affectivement à ceux qui les oppriment, et malgré leur hostilité contre ceux-ci voir en leurs maîtres leur idéal". (14)

Pourquoi cet arrêt sur des événements que l'on pourrait croire perdus dans la nuit des temps? Pour deux raisons au moins.

La première tient au fait qu'en cette proche fin d'un siècle que l'on peut dire avoir été plus qu'aucun autre marqué par la chose politique, nous demeurons dans une ignorance à peu près totale des processus qui se déroulent sous nos yeux et au fil desquels la politique, au prix de millions de vies humaines, peut à tout instant basculer dans le registre de la religion.

La seconde raison est liée à un anniversaire et aux considérations qu'il peut engendrer. Presqu'un siècle après la venue de Freud à Paris et sa rencontre décisive avec Charcot, l'ouvrage de Marc Bloch a été réédité par les soins d'historiens de l'Ecole des Annales qui se considèrent à just titre comme ses héritiers. C'est à l'un d'eux, Jacques Le Goff, que fut confié le soin de rédiger une préface afin que soit présenté au plus grand nombre ce chef d'oeuvre qui n'était plus connu que de quelques uns. De cette préface émouvante et respectueuse, j'ai retenu ce passage, particulièrement révélateur de la sorte de paralysie qui frappe les historiens - mais ils sont en bonne compagnie - lorsque la chose psychique se manifeste dans le cadre de leur territoire (15). Force est en effet de constater que si Jacques Le Goff use dans ce passage, en 1983, du terme de psychanalyse, cela ne change rien au fait que ce dont il est question demeure de l'ordre de l'innommable puisque l'apparition de ce terme sous la plume du grand médiéviste contemporain s'effectue au moyen d'un procédé dont Freud nous a fait connaître le sens et la fonction, la dénégation. Voici donc ce passage où s'il n'est certes plus question d'"hérésie physiologique" ou d'"obscénité scientifique" le vif du sujet est encore enfoui dans ce que Marc Bloch appelait les "choses profondes": "Profondeur, écrit Jacques Le Goff, métaphore dont il ne faut pas oublier que, sans amener l'histoire jusqu'à la psychanalyse, elle a été depuis un demi-siècle un des concepts flous qui ont aidé l'histoire à franchir les limites et les barrières, à aller ailleurs, plus loin, plus au coeur des phénomènes, des hommes et des sociétés historiques." (16)

Références

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(4) Marc Bloch, Les rois thaumaturges, Gallimard, Paris, 1983.
(5) Marc Bloch, op cité 1983, p. 18.
(6) Ibidem p. 417
(7) Ibid p. 418
(8) Ibid p. 419
(9) Ibid p. 419
(10) Elisabeth Roudinesco, Histoire de la psychanalyse en France. La bataille de cent ans, Vol I Paris: Ramsay, 1982.
(11) Marc Bloch, op cité 1983, p. 429
(12) Freud plus d'une fois a marqué son intérêt pour des phénomènes

du même ordre que ceux étudiés par Marc Bloch et il n'a jamais manqué, là comme ailleurs, de souligner l'importance de l'apport de Charcot. Cf. Sigmund Freud, Charcot, in: Résultats, idées, problèmes, Vol I, Paris: P.U.F., 1984; Sigmund Freud, La Naissance de la psychanalyse. Lettres à Wilhelm Fliess, Paris: P.U.F., 1956; Sigmund Freud, Une névrose diabolique au XVIIe siècle, in: L'inquiétante étrangeté et autres essais. Paris: Gallimard, 1985.

(13) Charles Blondel, La psychanalyse, Paris: Alcan, 1924, cité par Elisabeth Roudinesco, op cité 1982, p. 283

(14) Sigmund Freud, L'avenir d'une illusion. Paris: P.U.F., 1970, p. 20

(15) Je n'avais pas encore connaissance au moment où j'écrivais ce texte du travail de Philippe Boutry et Jacques Nassif, Martin l'Archange. Paris: Gallimard, 1985, qui me semble marquer l'aube d'un renouveau d'une collaboration que n'épuiserait pas le qualificatif d'interdisciplinarité.

(16) Marc Bloch, op cité 1983, Préface de Jacques Le Goff, p. XVIII.

MATTERS HISTORY

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Summary.
This paper is concerned with the processes through which objects are constituted as historical modalities through the application of techniques of discursive regularization which manifest themselves as institutional practices. These objects, it is argued, are the embodiments of power; power which operates through techniques of dispersion and combination. In this paper, 'history' is considered in terms of 'historicity', that is, 'modality' or manner of temporal movement.

Taking the case record as an empirical example, this paper argues that modern discourses operate and erect histories which, when analyzed discursively, display the incorporation of political, economic and social 'empiricities' (that is, fields of investigation, of truth, domains of object-relations and concepts, structures of authority and sites of speech) into the object-body addressed by the record. These 'empiricities' disperse data through the historicity expounded by the record and in so doing combine a series of diverse relationships into a 'body' of knowledge which is, above all, an object of practical intervention and thus displays the formation of a domain of strategic power.

The analytical method is then compared with other contemporary theories of textual- and micro-practice - ethnomethodology, deconstructionism, structuralism - in order to elucidate the specificity of power relations as revealed through discourse analysis.

The paper concludes with some remarks concerning the particularity of the relationship between the subject of history and the object of discourse.

I. Techniques and bodies

This paper is concerned with the processes through which 'bodies' are constituted as 'things', i.e. as accountable, knowable, useable and moveable objects. The argument centers around the effectivity of institutional discourses in creating regularities of knowledge,

information, and 'fields of presence' within which 'bodies' are located as objects of institutional concern. These objects, it will be argued, are the embodiments of power; power which operates through techniques of dispersion and combination. Since the constitution of bodies is a very broad topic this paper will concentrate on one effect or aspect of the process: making history. In this paper 'history' will be considered in terms of 'historicity', i.e. 'modality', or manner of temporal movement.

II. Discursive regularization

'Objects' are only describable as such on the condition that they display regularity. One cannot conceive of an object that does not conform to a set of criteria which define it as the object in question rather than any other object. Conformity is thus a crucial element in the definition of an object. What is peculiar to a technique is the specificity of its application: a technique is a local phenomenon. A 'body', on the other hand, is a general phenomenon, i.e. it consists of many parts - it is the generalization of a multiplicity of defining characteristics. 'Discursive regularization' refers to the massing or summing of the multiplicity of parts into a whole. This massing works, in part, through standardization (e.g. measurement of common characteristics), but also through deviation (e.g. size of severity as distance from a norm). It must be remembered that what occurs in discourses is not the dispersion or combination of parts, but the dispersion and combination of techniques - this will become clearer in the example below.

III. Conceptual clarification

a) By what right can a regularization be termed 'discursive'? What distinguishes it from any other thesis or analysis of order?

The distinction between the discursive and the other levels of order is analytical. 'Discourse' displays a level of analysis of the order of 'things'. It is concerned with the specific dependencies and relationships of objects to one another, and with the convergence and divergence of 'fields of presence' which announce the facticity, spatiality and temporality of objects. Rather than asking: What effect does one object have upon another? (e.g. social class and education, or wealth and status, or word and meaning, etc.), a discursive analysis demands the revelation of the techniques and

strategies, the convergences and divergences by which objects can be said to be related and can thus be said to exist as these or those objects. According to Foucault, the 'discursive' refers to:

> ... the group of relations that discourse must establish in order to speak of this or that object, name them, analyze them, classify them, explain them, etc.. The relations characterize not the language (langue) used by discourse, nor the circumstances in which it is employed, but discourse itself as a practice. (AK. 46)

The aim of a discursive analysis is to 'depresentify' objects (AK. 41) to extract the foundations on which their possibility rests: to make objects 'speak'. Objects, far from being the silent partners to discourse, are in fact the enunciations of which it is composed. This is the first prescription for a discursive analysis: Treat the object as the said rather than the thing spoken about.

b) On what criteria is regularization to be judged? How, in these fields of multiplicity, of convergence and divergence, can a regularity, which is not situated inside the object to which it refers, be described?

Regularity is not a feature of objects, it is a feature of order. Objects do not display order. Rather, the reverse is true: order displays objects. Consider the 'great' scientific discoveries in the fields of medicine, biology or psychiatry (to which Foucault refers). How could one possibly 'discover' bacteria (or even less a specific bacterium) except by a technique of intervention which codified and regulated the structures of 'strains', a technique which regulated the relation of the observer to the thing observed, a technique which involved an ever more precise differentiation of natural species and their qualities? Even less can one imagine Lombroso's 'scientific' studies of deviance and physiology outside of a practice which regularized physiological features and their development in relation to life-patterns. Or consider the much more mundane and immediate regularity which controls objects (and especially bodies): that of time. The earth may well rotate on its axis and travel around the sun, but it does not do the former in accordance with the days of the year or the latter in accordance with the years of infinity. The earth is oblivious to order: It does not decree that some humans will work on Mondays or rest on Saturdays. Nor are the concepts of weekdays or hours or minutes applicable to it. They are only applicable to the effects or consequences of the

earth's attributes, one of which is movement. Regularity is thus an aspect of effect and attribution. Who is to say that time will not one day display a different regularity, commensurate with the uses to which it is put and consequential in the determinations it exerts?

This is the second prescription for a discursive analysis: treat the regular as object and not the object as regular.

c) A final point. If, in conducting such an analysis, a regularity can be observed, em-bodied, object-ified, does this not immediately display the intrinsic nature of the object rather than its extrinsic qualities? Does regularity not, by its constitutive determination, usurp the place previously occupied by the regular object? It is in this question that the issue of practice and the techniques by which it deploys regularity becomes crucial. For what is at stake here is not a reconstitution, a remaking of a reality which really does present such and such characteristics. It is instead an investigation, a display of engagement: What are the realities which are attended to? What relationships arise out of the combination of a method and a substance? What historical modalities are expressed by these relationships?

IV. The constitution of bodies

The field of discursive analysis concerns those incorporations of 'empiricites' (i.e. fields of investigation, of truth, domains of object-relations and concepts, structures of authority and sites of speech) (e.g. 'discipline') into and around certain fields of objects and the strategies by which they are maintained, supported or rejected, transferred and developed. For purposes of exemplification the functions performed by the techniques of support, transference, etc., are analyzed in the first instance in terms of the relationships established between the empirical authority and the object(s) of its practice. Let us take an example, a familiar one at the present time, not least because certain authorities have spoken of it, examined it, and pointed to its place within a field of power relations. The example is the 'case record' kept on clients of the 'health' and 'social' services (see Goffman 1959: The Moral Career of the Mental Patient). Already relationships of engagement are apparent: although 'health' and 'social' interventions are services in our society it is the recipient of the service whose qualities are en-cased; the

'client' has no equivalent injunction to case-record the specific qualities of the service's institutional arrangements of personnel. Nor is the document kept with the client: it is a document written by professionals for their use, a document which ensures a continuity of knowledge and forms a basis for intervention. But nor is the record the 'property' of the professional, it is a constituent feature of the service-institution. The client cannot see the record, the professional cannot remove it: the record 'belongs to' the institution. It is in the same relationship as the bricks of which a hospital is built, or the desks on either side of which the client and the professional confront one another in a juridical and pedagogical manner. The cabinets which contain the records are like the cupboards which contain the drugs: access is hierarchical and specific. The files, like the pill bottles and the medicines, can be replenished or altered under the appropriate authority, but they cannot be removed or emptied, they cannot be opened up to public (or private) inspection. They are not subject to a checking, like a customer would examine a commodity before the guarantee expired. Records are kept on clients: they are worn like invisible port-manteaux; covering the entire body, limiting it, defining it and making visible its structures and its movements.

But of course the record does not cover the physical body, - although it may result in real physical limitations and visibilities - the record primarily concerns the social body: what the body has been doing, where it has been, to whom it belongs, with whom it belongs, what effects it has had, what parts of it are related to those effects, the statuses it holds and has held, both its social and geographical locations, what other individuals demand from it, and the records of the body's contacts with other institutions. Nor are these all of the demands made of the body by the fact of recording: date of birth, physical description, mannerisms, all of these and more are routinely entered and stored, passed from one professional to another in a circulation of knowledge which indexes the possibilities of the different participants to the fact of the record.

V. The retreat and return of the origin

The record is internally divided; it is a tabular grid of sequential data. The social body cannot be entered into it in any manner (this

would leave the body to define itself, its own exteriorities, limits and conditions). The structure which covers the body is minutely detailed and specified. It consists first, perhaps, of the relationship of the source from which the data is extracted to the body (familial, communal, professional, etc.). Then follows environmental circumstances:

> Mrs. X lived alone in a council house for the last five years (approx.) until November 1984 when she went to live with Mr. Y (her son) ...(I)

When the client's solidity in the present social environment has been established, when the client's body has been located, then begins the investigation of the client's past: Where does this client begin? Who are her/his progenitors? Where are they? What were their circumstances?

> Mrs. X was born in Manchester and had seven sisters ... Mrs. X's father died 40 years ago; her mother 15 years ago.

In this manner the client's origins are codified; described in the detail common to the record.

It appears that this practice begins at the end, with the client's present, and then retreats to the past, to the client's origin. But this is only so because the practice must establish the point to which it will return. Between these two points a line will be traced in discourse which describes the body's modality; how history has worked in it. And through the techniques and tactics which establish this modality moves a power as knowledge: a power which constitutes, objectifies the particular exteriorities through which this modality operates:

> ... the original in man (sic) is that which articulates him from the very outset on something other than himself; it is that which introduces into his experience contents and forms older than him, which he cannot master; it is that which, by binding him to multiple, intersecting, often mutually irreducible chronologies, scatters him through time and pinions him at the centre and duration of things. (O.T. 33I)

The two points have been determined for the record: the point of existence and the point of origin. Between them is drawn the line which connects the two, a graphical representation which approximates its points of contact to the historicity expounded by the record: 'Childhood' (as object) consists in the investigation of points of possible distance between the client and his/her origin: number of siblings, relationship to parents, presence or absence of parents, potentially indicative family attitudes and values:

> Mr. Y does not recall Mrs. X being considered a difficult or unusual child ...
>
> ... as far as he knows Mrs. X's parents got on quite well together ...

'Personality': what has the client done? Does s/he like people? Do people like him/her? What sorts of feelings does s/he give vent to? How? When? Under what circumstances? How is this judged? Who has judged it? When?

So the table continues its exposition: 'past illness', 'occupation', 'marriage', 'special needs'. And when the line has taken its shape, when the two points have been connected in the historicity peculiar to them, when the origin has returned to its present, then the record demands that the nature of the line be expressed, encoded, inscribed upon the object-body, delimited: a mode of analysis translated into the experienceable. This 'conclusion' states the line in terms of the institutional discourse which investigates and describes it; the line is named and located: it is of this or that nature, it belongs here or there (and, therefore, so does the body):"Mrs. X has become quite unmanageable at home."

Each distinct element in the table expresses the relationships of the participants to it: who knows, who says, who experiences. Also, who writes, who speaks, who can name, who can include or exclude the event; what is the relationship of the speech to the record? It must not be forgotten that when the 'facts' have been located in their appropriate spaces it remains for the professional to pass judgement on the evidence: whether it is valid, whether it is reliable; what conditions and circumstances, what openings and closings does the evidence display; how practice can maintain its continuity; in what way the object is moving, what forces are at work on the body (stress, historical repressions, physiological deterioration), and, of course, what forces are at work on the professional (manipulation, projection, transference, dependency):

> Mrs. Y seemed to me to be genuinely fond of his mother ... Mr. Y is, I believe in some financial difficulties ...
> As Mr. Y gave me all the information contained in the social history, I cannot vouch for the validity of its contents ...

From one small part of one small document emerges a vast and active multiplicity of relations, a set of domains and directionalities which disperse historical and social data through a predetermined grid and in so doing combine their categorizations into a display of

antecedents and consequences, transparencies and solidities, movements and stages. In short into an em-bodiment of regularization.

VI. Incre-mentality

What, then, is the specific nature of the incorporation of 'empiricities' referred to as the field of a discursive analysis? So far I have merely detailed the textual determinations operating on professional practice and judgement; a set of determinations already familiar in sociology through the work of Goffman, Cicourel and others. The fact that institutional worlds exert pressures upon their individual participants in terms of both their implicit 'methodologies' and their 'world-views' is not a new or revealing insight. What distinguishes this particular form of discursive analysis from an ethnomethodology or its related domains?

The fact is that the distinction lies precisely in what it is that is being analysed. An ethnomethodology may display for us a 'body' of 'rules' or procedures but it cannot (or will not) account for their existence or for the particular configurations which they manifest. For 'rules' or procedures do not simply or neatly arise out of an ongoing negotiation between participants to a single reality. Procedures, techniques, and tactics deploy, through their specific configurations and regularities, the direct possibilities of the participants to the realities which result from their use. The relation between a configuration of techniques or a combination of strategies and the constitution of power relations within the objective field of action is certainly a relation around a reality. But that reality contains within its objective determinations definite domains of economic, social and political possibilities. The movement of an 'economy', or a 'polity', the position of individual humans to the sum total of power domains characteristic of a given social formation is manifested precisely through the sum total of the possibilities deployed through discourse. 'Economics' and 'politics' do not exist independently of the activities of 'economicing' or 'politicing', for it is precisely as domain figures that power relations are established. This is the 'deployment-function' of discourses: the distribution of the form of power through society. It is what erects or defines in its materiality the reality of power for individuals. Discourses, as systems of dispersion, distribute the places which contain individuals. Across and between those spaces is

conducted the war of knowledge on whose surfaces history moves: surfaces of 'mind' (the idea, the consciousness, the soul, and the myriad couplets transgressing and transforming its purity: 'will', 'desire', and in our own time 'intelligence', 'purpose', etc.); surfaces of 'matter' (the body, the tool, and its cuplets: strength, technology, environment, etc.); surfaces of the 'absolute' (God, energy, etc.); or surfaces of the 'relative' (heroes and villains, fortune and misfortune, etc.).

Within these historicisms are placed the causes and the relevances of human action: its determinations and its effects, the consequences it announces by its discursive facticity, the definiteness of its temporal location and the ensuing correlation of its social, political and economic possibilities. It is within discourse that these effects and consequences are located, worked, combined, checked, aggregated and em-bodied; given over to a space occupied by an individual: 'This is your space, this is what defines it, this is what can be done in it'. But also, of course, 'This is my space, this is what defines it, and this is what can be done in it'. These spaces are ours on the condition that they belong to no one else, on the condition that the partitions are maintained as definite partitions between individuals, that the individual's space is not transgressible by other individual spaces.

In Discipline and Punish Foucault deals with the physical partitioning of prisoners in the disciplinary institutions of the 19th century. His thesis is that in the figure of the prison and the means by which it effected the 'correct training' of the prisoner's body the incorporation of a power based on discipline emerged as a means of controlling bodies within every sphere of society. The 'panopticization' of life achieved through the body of the prisoner generated a precisely adapted code for the regulation of behavior. This 'code', for Foucault, manifested itself in all the institutions concerned with the training of the body and of the mind (e.g., schools). In Power/Knowledge (1980) Foucault suggests that the same principle is in operation in the field of social work (p. 62), and that it may be extendable throughout the range of interventive practices in our society. But it is not the partitioning of the physical body, in any simple sense, which occurs within the domain of social intervention, although one of the effects of the principle of partitioning is in fact real physical limitation - as in the case of

probation or, more extremely, psychiatric internment and the 'care' orders placed on 'children at risk'. What is more significant about the principle of partitioning is the social 'siting' of 'fixing' of an individual's reality. A reality which consists of productive possibilities of determination - self or public - and, of course, possibilities of negation (the very real power to say 'no').

This partitioning and its concomitant principle of enclosure works by the power of ranking or the definitional concept of the 'increment'. That is, the spaces occupied by individuals are differentiated incrementally through the yardstick of gradation. A hierarchy of plus and minus stages upon which the spatial differences display their own uniqueness (see sheet on 'Types of Depression'). In the gradation of depressions the professional is required to attend to a multiplicity of symptoms, of 'signs' which, far from being located within the individual, in fact traverse his/her exteriorities: his/her expressive patterns, his/her physical and physiological processes, the outside of her/his existence. It is these exteriorities which are sited and determined, fixed in the increment of the table. Now this fixing is not an independent activity, the single symptom does not itself inform the professional of its place within the grid. Each symptom is minutely detailed only in so far as and only on the condition that it will aggregate and will relate to the sum total of the conditions of its existence. Depression is a pattern, the table tells us as much. Or, rather, it is a pattern for action, not a pattern for truth in any nominal sense. The truth of a particular depressive arises, in this table, through the incremental movement of a deviation from something which the table does not state. Or, at least, it does not incrementalize its inverse correlate. The field of the non-symptom, the 'un-symptom' is not addressed although it displays its referential capacities. Activity can be 'normal' but it can only be incrementally abnormal. The 'normal', the table's unexpressed alter condition, appears as precisely that which is un-measured; that to which measurement per se does not apply; the remainder standing behind or beyond the possibility of measurement; what is left when all of its 'otherness' has been extracted.

This peculiar situation appears to state that we are the reverse side of the things we state we are not - or at least the things we stage humanity is not. A posture adopted by many contemporary analysts whose avowel is the deconstruction of the said for the purposes

of revealing the reality of the unsaid. But in fact we find that the opposite is the case. For the table does not state all the things which 'normal' human beings are not and then counterpose them to the unstated but implicit things which humans really are. There is no appeal beyond or even inside the text to an 'other' condition which, by way of contrast, ratifies the distance between two displays of what could possibly be in the lives of humans. What the table in fact does is to explicitly state the surfaces and signs through which a life - anyone's life - can move. The table produces a 'case' out of the very real attributes of very real humans. The table states the contours of life in the most explicit terms possible. There is no court of appeal, no 'other' reference from which a truth can be dislodged, made, once and for all, to stand on its own feet and speak the wisdom which our own practices refuse, by their very nature, to reveal. This table literally contains the reality of our existences.

Nor are existences static. Tabulation recognizes this much, at least. The function of the table is to establish the places and surfaces in and on which movement progresses. The detail of life must be known if its trajectory is to be mapped in the modality peculiar to practices. The places of historical movement have already been examined. The function of the Return of the Origin in codifying the historical incrementality of the individual's life embodies a temporal pattern which announces the forces at work in the individual, but also between individuals. Within the spaces in which time moves exists an objectivity and facticity whose specificity, whose nature, accounts for the constitution of a body: a body consisting of proportions and extensions, a body which has its places, a body that can be judged. Now, of course, this 'history' is simply one more place; it is not the locus of the body, although it is the locus of the body's time; it is not the locus of the body's economy, although it temporally economizes the body; it is not the locus of the body-politic, although it politicizes the body's temporality. The history, of itself, is none of these. These are merely aspects of the body - surfaces for the body to exist in and through. The body has many more surfaces of existence: domicility, wealth, ownership of properties, debts, official labels and identities (e.g. national insurance number), work-status, state benefits, manifestation of 'official' (i.e. legal and statutory) duties and/or rights, etc... The body's economic particularity and political status are stated (i.e.

eligibility for work or voting, etc.). Moreover, these surfaces (of which the foregoing are only examples) are constantly redeployed throughout a body's contacts with institutions. An institutional discourse is precisely that: a system characterized by specific techniques through which bodies are constituted in their uniqueness, a uniqueness predicated upon the systematic dispersion of techniques of knowledge, tactics for the deployment of a series of regularities, regularizations within which a set of active possibilities can be codified. For discourses themselves demand that bodies must speak, they must speak an institutional language of the body and in so doing define their own bodiliness, the pecularity of their own existence; an existence which, as we have seen, moves not inside them but on their outsides, on their aspects and proportions. Here, in its material functioning, is the 'reality' of power; not in its location or possession, not even in its differential quantative distribution, but in its distributive nature. Power is not that which having been accrued can then be exercised. On the contrary, power is that which having been exercised can perform the function of acquisition - the acquisition of domains of application, the organization and establishment of definite materialities. Power does not arise from a certain materiality, or set of such. Materialities are precisely the names given to the particular manifestations of relations of power. The material world does not consist of its own definition, it does not stand outside of and free from the effects of a particularity of constitutive practices. The materiality of a world cannot be divorced from the places which define what it is, the openings through which the world can be viewed; openings which are simultaneously economic and political, 'real', not in the sense of 'meaningful' but in the sense of 'effective', openings which 'really' can be painful, difficult, and meaningful.

In the processes of this constitution can be seen the co-existence of a whole series of domains: the juridical, in the form of legal and statutory obligations; the pedagogical, in the form of prescriptive definition; the economic, in the form of the distribution of resources; the psychological, in the form of the external tabulation of signs geared to the indexing of a state of internal existence; the sociological, in the form of the generation of statuses from a body's contacts with other bodies. These are the strategies of determination; these are the 'methodologies', now appearing as far

from the realm of the implicit as the definition of 'normality' appeared from the realm of the unsaid. Power moves across surfaces. We need look no further for its effectivity, for its constitutive domination and subjection than the explicit and manifest level of practice. Did Marx not say as much in 1845:

> All social life is essentially practical. All the mysteries which lead theory towards mysticism find their rational solution in human practice and in the comprehension of this practice. (Theses on Feuerbach)

Discourses deploy practices, distribute modalities, and regulate activities. All techniques which measure or assess, or in any way are concerned with the revelation of properties, are essentially distributive. Their 'function', or 'purpose', is not to determine the absolute quality or quantity of the thing measured. Rather, their utilization is inextricably bound to the figure of comparative location: the siting of a property/quality - determined, maybe, by the aggregation or interaction of smaller characteristics - in relation to a series or table within which the measured object can be placed. Through this placing, this fixing, can be determined the appropriate or necessary (in distributive practices is there a difference?) forms of action which allocate to the thing measured the political, economic and social resources which move the object along lines of powers. Such techniques, then, are distributive in (at least) a double sense: they concern the distribution of qualities/-properties/characteristics which attributively fix and locate an object; they also concern the distribution of domains which open and/or close possibilities for the future location of the object. It is the distributive character of such techniques which justifies the use of the term 'discourse' to refer to the overall relationships between practices and their objects.

Techniques of measurement/assessment can do nothing but institute these distributive possibilities since it is not the object itself which is assessed but the signs by which its particularity is determined. Table I does not describe the measurement of a male or female depressive; nor does it measure the differing depressions themselves. Table I guides measurements of qualities which depressions display in terms of the subject of the assessment - the male or female individual who eats, sleeps, toilets, acts (physically and mentally) and self reflects.

Power moves through bodies. Power is what gives bodies their multiple character; or, rather, it is the fact that multiple

characters are analyzable as bodies which displays the operation of power. For bodies are not silent monuments, they are not solidities out of which characters can be extracted or 'discovered'. Bodies are the result of the multiplication of characters. It is this observation that allows us to infer the existence of a body in a diverse set of locations: the biological body; the social body (in both of its senses); and in this process of inference to reduce to the experienceable what is constructed as the analyzable. Is not the identification of a body an analytical act? The product of an organization and a massing? It is the attribution to, the locating upon, an element of experience a regularity of analysis that produces the fact of 'a body'. Is attribution, location, not itself an act? - And if it is an act does it not necessarily involve a power? The 'power to act' is a familiar phrase, and we know the identities of the potentiaries whose actions weigh upon bodies: the doctor, the jailer, the teacher, and all the myriad functionaries who combine the juridical, the pedagogical and the medical. But we must not forget that bodies are the concern of us all. They are 'things' to which everyone has recourse (our own bodies) and location in (our body of acquaintances). By what techniques, under what conditions do we em-body the characteristics, the elements, the fragments, the analogies and differences, and the contradictions through which our realities unfold?

VII. Distribution, action, and practice

It is through practice that the particularities of order are realized, are made 'real', tangible, usable, observable, and thereby relatable: medical practice makes possible the classification of diseases and thereby the ordering of illnesses; penal practice realizes the ordering of criminality as a specific and concrete domain; psychiatric practice reveals the ordering of insanity; and teaching practice the classification, gradation and organization of the double figure of intelligence/ignorance; etc..

How does this practice proceed? It proceeds through action. It is impossible to conceive of an action (a speech, a task of work, etc.) which does not possess economic, political, and social implications, consequences and antecedents; the fact of activity intrinsically concerns all of these domains. It is through the introduction of the facticity of action that it can be distinguished from practice. 'Action' describes the technical processes involved in the

doing of an act. The actual technicalities of performance: how records are written, how contact is maintained, how interactions are begun and concluded, how information is collected, how it is channelled, etc.. 'Practice' describes the particular configuration/-formation of domains inscribed in those processes: the particular 'shape' or 'order' of the political, economic, etc., conditions. Practice therefore relates to legitimacy, validity, reliability; in short, to prescriptive conformity, to the discursive regularization of action. Action, therefore, corresponds to the level of technique, but only in so far as its conditions of practice are considered at the tactical level; in terms of the directions which action takes in its spatial and temporal dimensions and in the dimension of its social movement: the objects upon which action is targetted, the pathways through the social body which action takes on its journey to finitude. For all knowledge and all action are concerned with a finitude, with an endpoint which is the locus of their motivation (see above on historicity). It is precisely on the basis of this finitude that techniques can multiply (disperse) and regroup (combine) properties, features, etc., into 'bodies'/'objects'.

Now we begin to approach the notion of 'systems of dispersion' (AK. 37). As a first hypothesis I shall say that a system of dispersion refers to the regularities which the distribution of practices, objects and characteristics assume. The notion incorporates a concern for the conditions upon which distributive possibilities are based. Furthermore, it is a dynamic concept in as much as it is not simply reducible to the analysis of similarities and differences between objects, qualities, etc.. Rather, a 'system of dispersion' refers to the practical prescriptions which the distributions of objects, 'bodies' etc. take:

> Whenever one can describe, between a number of statements, such a system of dispersion, whenever, between objects, types of statement, concepts or thematic choices, one can define a regularity (an order, correlations, positionings and functionings, transformations), we will say, for the sake of convenience, that we are dealing with a discursive formation. (AK. 38)

It is a commonplace in structural analysis to define an element in terms of its place/location within a system, and structuralism's methodological efficacy derives from its ability to reveal regularity and its accompanying 'rules' or 'codes'. But the equation: signifier + signified = sign, upon which all structuralist and semiological analysis ultimately rests, contains one crucial omission for the

understanding of the organization of human life. Namely, the material effects which the application of a formative/transformative power produces on the object upon which the power moves. For existence is reflected, not in the sign, as semiology would state, but in the act whereby the reality of the sign inhabits the reality of the object; where two spaces are subsumed into one, into the fact of their equivalence. It is the space which displays the existence or the reality of any object (see above on 'incrementality'). It is only through the space that an object can move, can exert its possibilities, can 'reflect' the fact of its existence.

Hence the need for an analysis which attends both to the order which a system of dispersion displays and to the formative/transformative power strategies which ordering (as process) generates. The establishment of an order between elements places those elements in 'domains of power' (HS. 82) characterizeable by the specific forms which the relations of power take (e.g. sovereignty, pedagogy, juridicality, etc.). It is these relations of power, the particular forms which they exhibit, which make possible the description of systems of dispersion in terms of a theory of discourse. For discourses are characterized by 'enunciation' ("... the act whereby an utterance is produced." Nowell-Smith; 1981, p. 233).[2] 'Enunciation' is determinate speech; not the langue of structuralism, or the exegesis of hermeneutics, but the particular manifestation of the relation of the system to the subject, the 'enunciator', which, for Bellour (1979, p.94):[3]

> ... marks both the person who possesses the right of speech ... and the source (instance) towards which the series of representations is logically channelled back.

This particular relations defines for the discoursing subject her/his enunciative position with respect to all the other systemic elements: his/her 'enunciative modality' (the sovereign subject, the pedagogic subject, the juridical subject, etc.). To be more precise, one needs to attend to the subject's modalities. Discourses do not circumscribe singular forces, they are not defined by a particular modality which is 'proper' to them. They exhibit a plurality of modalities which are set in place on conditions which particular configurations of elements make possible.

VIII. Subjects-modalities, history-knowledge

If discourses are characterized by a plurality of modalities, a plurality of possible positions which the subject can take in relation to discourse, and if these possibilities are themselves determined by the total configuration of discursive elements, then it is absurd to speak of the subject as the locus or origin of all discourse. On the contrary, discourse is the origin of all subjects. This observation is particularly pertinent in a consideration of history. 'Man" may, or may not, be the subject of history, but this is true only in so far as history is conceived as discourse: as a system which contains a place from which movement can be said to issue. That this place has been filled by 'man' (and not 'woman'; not yet, anyway) in historiography does not by that fact indicate 'man's' superiority over the worlds in which he lives and dies. The fact is that history is not and never has been made by 'man'; that unitary figure who represents the end of evolutions. It is only because 'man' is figured as the end of evolution, i.e. as the return of his own origin, that he can be said to be the locus and the meeting-point of history. 'Man', as discursive subject, is both his own origin and his own finitude: 'He' is where it begins and ends in discourse. But history is created by all people - whether they are 'men' or not, whether they are black- or brown- or white-skinned, whether they are old or young, able or infirm, rich or poor. What is at stake in histories, especially histories of the present, is not their attributability to a locus of creation and/or movement, but the forms through which they are enacted: the distribution of powers and their characteristics, the modalities in which individuals are invested with powers, the dispersions which techniques of order generate. The subjection of a body to a historicity (or historicities) is, thus, both a question of technique and of history; for it is in the constellation of forces peculiar to practice that bodies are technically and historically produced.

IX. Concluding remarks

What is the relationship between this 'subject' and the historicity peculiar to objects? How do the conditions of the subject engage with the order in which objects are defined in their materiality? The answer to these questions lies precisely in the subject's relation to discourse. For discourses are systems of dispersion, not only of objects, but also of the modalities peculiar to them. Discourses

disperse subjects and objects in relations of dependence to one another; specific relations in which the definition of an object of discourse is related to the functioning of the subject: the subject as 'healer', 'teacher', 'master', or, in the above example, 'assessor'. In the discourses on wo/man, both the subject and the object of discourse are the same figure: humanity (or, perhaps, humanities). The dispersion of techniques and tactics, inclusions and exclusions, characters, proportions and judgements through domains of knowledge and practice is itself the dispersion of the very qualities which humanity assumes. The regularities which systems of dispersion exhibit are the atoms of the order in which the figure of humanity traces its history: its mode of historical movement: its historicity.

Notes

1. This is an actual case record from a psychiatric social work situation. The examples are from a case history and are not, therefore, exhaustively representative. Names and places have been changed to protect the identity of the participants.
2. Nowel-Smith, G. (1981), 'A note on history/discourse', in Caughie, J. (ed.), Theories of Authorship', RKP, 1981.
3. Bellour, R. (1979), Alternation, Segmentation, Hypnosis, Interview with Janet Bergstrom. Camera Obscura. No. 3/4 pp.71-103.

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TABLE I

TYPES OF DEPRESSION

	MOOD	SELF-CONCEPT	GUILT	ACTIVITY PHYSICAL	ACTIVITY MENTAL	BODY FUNCTIONS SLEEP	BODY FUNCTIONS APPETITE	BODY FUNCTIONS BOWELS	OTHER
PATERN I 'Mild' 'Unhappiness' 'Grief' - at losses of various sorts, not only people.	Sad:declared or hidden.* 'Waves' of despair.	Mild or brief changes in self-image	Waves of guilt, with normal intervals.	Normal or restless: 'searching'. **	Normal loss of interest. Increased anxiety.* Hallucinations in grief.**	Trouble getting off: then O.K.	Poor - temporary. Weight loss rare	Normal	Increased rate physical illness and morality
PATERN II 'Moderate' 'Neurotic' 'Reactive' "Exogenous'	Sad:declared of exaggerated.'	Mild persecutory ideas, although considered by patient unfair. * Irritable.	May be absent, may create a great fuss. **	Normal or restless.**	Increased anxiety.** pointless fears, e.g. of dirt, * rituals, * agitation. **	Trouble getting off: then light. Nightmares. **	Poor: weight loss unusual.	Normal although complains a lot.**	Multiple complains of physical symtoms with no obvious basis. However, increased ra of true illness & death.
PATERN III 'Severe' 'Psychotic' 'Endogenous' sometimes 'Agitated'	Sad: declared, hidden, or even 'smiling'.	Delusions of disease, sin death and poverly.** Persecutory ideas considered justified. ** May be irritable	Profound about everything. ** Self-neglect. ** SUICIDE RISK	Reduced: 'retarded', or increased. 'Agitated'. **	Auditory, accusatory, hallucinations. ** Apathy. Preoccupied with gloomy thoughts.	Gets off Ok., then early morning walking, wandering. **	Poor: weight loss common	Constipation, double incontinence. **	Low blood pressure, slo-pulse, low temperature, may become insensible 'stupor'. High risk other physical illness and death from 'natural causes'

* these symptoms likely to be dictated or affected by the sufferer's personality.

** these symptoms likely to result in elderly resident being called 'confused', and some of them occur in dementia.

N.B. THIS CLASSIFICATION IS AN AMALGAMATION OF SEVERAL DIFFERENT APPROACHES AND IS A PRACTICAL GUIDE ONLY.

THE FIGURE OF THE HYPOCRITE: SOME CONTOURS OF AN HISTORICAL PROBLEM

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Summary

This paper aims to bring to the attention of historians of psychology and the social sciences some features of the development of the understanding of hypocrisy over the past 300 years. Subject to broad, penetrating and sustained attention, particularly during the eighteenth century, the figure of the hypocrite has evaded the gaze of contemporary scholars- perhaps, it is suggested, because hypocrisy as then was no longer exists. Tracing with wide brush strokes the shifting perceptions of the hypocrite over the period covered in literary, religious, political and philosophical texts, some more particular features of an historical development are revealed. During the 1700s, hypocrisy became a political phenomenon (where it had previously been treated in theological discourse) and a phenomenon of pressing importance (where earlier it had found usage as a blunt and ineffective weapon in sectarian conflict). As an important feature of social life (at least as this was reported and analyzed) hypocrisy was found in various forms, never more visibly than in the masquerade, a kind of display involving acting and dissimulation each of which was designed to project false appearances. Hypocrisy, indeed, was frequently judged to arise from the breach between appearance and essence and all means to screen 'true' thoughts, emotions and beliefs behind physical or plainly corporeal masks were assumed to bolster the general level of hypocrisy in society. In addition to beginning the exploration of this hitherto neglected political/psychological character, this paper treats certain historiographical problems and suggests some sources and methods for further research.

1. Hypocrisy: Why? and How?

In 1740, the 28 year-old Crown Prince Frederick of Prussia published a slim volume of commentary with the unequivocal title of Anti-Machiavel, a book written in French for style, printed in Holland for precaution, and published anonymously for discretion. Such safeguards, long in the planning, had little effect: the book was swiftly translated into the major European languages, reprinted half a dozen

times in the next couple of years, and the thin cover of anonymity was almost immediately blown off by scandal.

As we would expect from its title, the book is filled with outraged denunciations of Machiavellian politics and psychology; from it opening comment that 'The Prince is in point of morality what Spinoza's work is with regard to faith', the text is undiscriminating in its use of evidence and makes up for its slightly ill-tempered aspect with vigorous assaults against widely-recognised enemies like intrigue, treachery, dissimulation and unscrupulous double-dealing.[1] Impossible to summarize fairly, the central portion of the work would seem to institute an almost perfect mirror-reversal of the statements in Machiavelli's famous, or rather notorious, eighteenth chapter of The Prince, entitled 'In what ways Princes should keep their word'. Frederick's arguments here reduce to the claim that the ruler must keep faith with his subjects, subjects who are not (as Machiavelli depicted them) foolish and feeble, but fully able to detect fraud, resist pretense, and see across that ideological distance separating reality from its various representations. The lynchpin of Frederick's counterblast is the recognition, as he phrases it, that

> To judge of men by their words and professions would be the way to be always mistaken; we therefore compare their actions with one another, and then with their words; and against this repeated examination, falsity and deceit will find no refuge.[2]

This is no doubt a strong statement, but as I hope to illustrate in the following discussions, it is a statement of intent rather than of fact. Moreover, even the intention was swiftly revoked: no sooner had he written the statement than Frederick retracted it. On 31 May 1740, he became King, and the former anti-Machiavellian who'd learnt his craft well in studying the ruses of his erstwhile adversary now proceeded to practice these himself. In between seizing Silesia and provoking the War of Austrian Succession, Frederick summoned his philosophical alter-ego Voltaire and dispatched him across Europe with instructions to seize and burn all the copies of Anti-Machiavel he could lay hands upon.

As I have suggested, the suppression was unsuccessful indeed, in a moment of the purest Machiavellian cunning, Voltaire altered the book and proceeded to issue it under his own name. But more importantly, the kinds of issues raised by the text circulated throughout Europe for much of the century in treatises of politics, but also, by virtue of the perceptual, epistemological and linguistic problems

raised, in substantive treatises of philosophy, science, history and what we should term psychology. The subjects of falsity and hypocrisy refused to rest. Forty years after the publication of Anti-Machiavel, but more directly inspired by d'Alembert, the prestigious Prussian Academy announced for the topic of its annual essay competition the question 'Is it useful to deceive the people?', and drew more responses, and responses of a higher caliber, than ever before. These responses have yet to be fully examined by historians, and once we add to them the writings on the subject by, amongst many others, Rousseau, Voltaire (the real Voltaire!), Condorcet, Swift, and Adam Smith, we are faced with a splendid mass of work which has hitherto been neglected.

The aim of the following essay is in some senses a modest one: to begin unearthing a subject which has for too long lain buried. Before proceeding, however, it may be worth asking whether this aim is of interest to historians of psychology and the social sciences. Better still, one might ask first of all, whether it is actually hypocrisy which is the subject of the writings I shall deal with. In a simple and direct sense, it is indeed hypocrisy which is the focus of attention for the reason that this is the term used by the authors themselves, even when the issues being discussed would seem to be broader and to spill over into the terrain of treachery, or when they appear to be narrower and to concern individual acts of dishonesty. This does not mean, of course, that one may assume a common or equivalent area of debate behind all uses of the term 'hypocrisy'; rather, that term would seem to mask different kinds of approach to what is perhaps a fairly stable problem.

Once we begin addressing questions concerning the definition or sense of 'hypocrisy', we run into a familiar problem: the subtle, and not so subtle, changes which words have undergone over the past centuries. In this particular case, we need to be especially careful in recuperating the past, and lost, meanings of the term. One of the claims I make in the following pages is that the content of 'hypocrisy' was secularized during the eighteenth century, as hypocrisy became a term of moral-political discourse rather than of theological debate. Another, not unrelated, claim is that during the same period, hypocrisy became a subject of great, serious, pressing and fundamental interrogating, whereas it had earlier been a point of merely rhetorical reference. Both these claims appear stark once we consider

the character and status of hypocrisy today. One might conjecture that in the twentieth century the hypocrite has become banalised and, what is perhaps worse, psychologised.

Some words of explanation and defense: during the 1700s, as I shall show hypocrisy signalled the breach between appearance and essence (or between action and word). What does it signify today? If anything at all, hypocrisy suggests a dislocation between thought and feeling. The hypocrite used to betray his principles in public: his modern counterpart betrays his emotions in private. Moreover, the betrayal is no longer a spark which sets alight a fierce debate and critique: hypocrisy today prompts no more than gentle self-appraisal and reflection. Instead of the finger being used to expose hypocrites in our midst, it now points inwards as part of an exploration of the interplay, within ourselves, of emotion, thought, belief, feeling and such like. This, apparently, is how we are to go about finding out the truth about those falsehoods we have to tell. It suggests that hypocrisy has lost its political thrust to psychological pop.

An implication is that nothing has come to fill the space vacated by hypocrisy. If one looks closely, it would appear that a constellation of rough and ready terms now do the job: terms like sincerity, authenticity, credibility, and that apparently close etymological cousin of hypocrisy, hype, the subject of a recent study which defines it as 'a force that makes a mockery of the human essence.'[3] Such terms are interesting, and interestingly like hypocrisy, in a number of ways. For one thing, they are what might be termed our ordinary vices, as typical and (perhaps) inconsequential as disloyalty, tyranny and cruelty were four centuries ago when Montaigne termed these 'nos fautes ordinaires'.[4] But whereas hypocrisy, because it was so typical, attracted great attention in the eighteenth century, sincerity, authenticity and kindred terms are today, and perhaps for the same reason, the stuff of at best vulgar pop-psychology and sociology.

Also worthy of note is the fact that hypocrisy, like its modern 'equivalents', has been a term of approbation as well as one of the lesser evils in a long moral calculus. I give examples in the following pages of writers who thought it correct in certain circumstances to be hypocritical. No doubt many since have thought 'hype' to be a perfectly good and proper form of sales-talk or tall story-telling (one thinks in this regard mainly of Americans like Mark Twain and

Walt Whitman, the latter at his most hypish in 'Song of Myself'). On the other hand, many exist today who recoil at the gushing sincerity of modern politicians. 'Hypocrisy', 'hype', 'sincerity', and 'authenticity' are at least Janus-faced, and probably composed of a great many surfaces.

The third, but not the last, similarity between hypocrisy and sincerity/authenticity/hype is that each possesses that special quality of having both public and private dimensions. As a result, and far more clearly in the case of hypocrisy, serious political problems were posed by such terms in debates about democracy, since democratic theory and practice have notorious difficulties settling the boundaries between the public and the private spheres of conduct. Whereas sincerity, say, may easily be banalised into an element of (bad) psychological discourse, hypocrisy resisted easy depiction as either a psychological or a political phenomenon alone. It actually seemed to straddle the divisions implicated by the notions of politics and psychology. To consider writing on hypocrisy therefore offers a very useful entry into debates about political character, about how much human psychology can or cannot be the basis for political institutions, about how much political systems can determine psychological make-up, and such like all of these, issues of interest to the historian of the human and social sciences.

II. The Death of Hypocrisy?

To begin the exploration of hypocrisy in history is to launch into largely uncharted, and no doubt deep and turbulent, waters. There are very few contemporary studies of the status and character of hypocrisy; none which treat it in a broad social, political, religious, philosophical or scientific setting. This necessarily makes my own work tentative, difficult, but I hope of some use to those who will have come across the term in their reading or wondered why it has such a ubiquitous presence in a variety of discourses in early-modern England. To note this presence raises an interesting issue. In the past hypocrisy seems to have attracted great attention because it was (purportedly) an almost universal form of behavior and belief. today, when so little is written about hypocrisy, when the term hardly serves as a description of vice (or virtue), it might be thought that what it describes has disappeared from view. This may well be so. That our century can boast so few hypocrites - in literature, for

example, the titular heroes of Thomas Mann's Bekenntnisse des Hochstaplers Felix Krull (1954) and Sinclair Lewis' Elmar Gantry (1927) are the only two hypocrites which spring to my mind, and neither of them is English - suggests that another standard or scale of ethical behavior has assumed dominance. Perhaps hypocrisy/antihypocrisy has been replaced by insincerity/sincerity; more likely is that it is credibility which now serves. Better still, one might say with Lionel Trilling that it is authenticity which is today most valued.[5]

The search for words uncovers another feature of the moral landscape, for authenticity does not fill a place on a scale of belief and behavior: it actually thrives in an atmosphere devoid of all energetic convictions. The air which sustains it is quit unendurable for hypocrisy; as Nietzsche wrote:

> Hypocrisy has its place in ages of strong belief: in which even when one is compelled to exhibit a different belief one does not abandon the belief one already has. Today one does abandon it; or, which is even more common, one acquires a second belief- one remains honest in any event... I greatly fear that modern man is simply too indolent for certain vices: so that they are actually dying out.[6]

The disappearance of strong belief erodes the calculus of morality. Even the lie is now recognized to exist only in particular environments, so we are all contextualists who judge that any falsehood may only be so described in relation to another (equally unfixed) truth.[7] It was once prophesied that the lapse into moral relativism would inaugurate a period of ungovernability and would engender the dissolution of compact and convention in human affairs. This has turned out not to be so. There are those, certainly, who continue to utter the same prophesies (many of them from position in government), but for the most part we seem to accept rather nonchalantly that hitherto essential ethical polarities like truth/falsity, honesty/dishonesty no longer articulate social conventions. A paradox is raised: we all don masks which serve to disrupt scales of moral behavior and belief, and by so doing we all become hypocrites. But beneath these masks are yet more masks, and by this token there is no means to measure the extent of our hypocrisy (say, against a 'real self'). The separate 'real' of 'self' which might be thought to lie behind appearance actually resides in our imaginations. The fabalist Jorges Luis Borges has described the dilemma in looking behind the face of Shakespeare to find 'only a bit of coldness, a dream dreamt by no one.' He continues:

> History adds that before or after dying (Shakespeare) found himself in the presence of God and told Him: "I who have been so many men in vain want to be one and myself." The voice of the Lord answered from a whirlwind: "Neither am I anyone; I have dreamt the world as you dreamt your work, my Shakespeare, and among the forms in my dream are you, who like myself are many and no one."[8]

Here, it will be noted, Borges puts the announcement that we are 'many and no one' in the mouth of God, and this reinforces the distance that already separates modern conceptions of honesty, self, and authenticity from those in earlier times when the battle between hypocrisy and its enemies was waged on the plains of religion.

If one consults the Oxford English Dictionary for a definition of hypocrisy, one finds that it is 'assuming a false appearance of virtue and goodness, with dissimulation of real character or inclination'. Furthermore, the definition links hypocrisy with religious beliefs. To unmask one's religious opponents has always been a favorite weapon in theological warfare: sects launch at one another accusations of hypocrisy, an the atheist and believer level at one another equivalent abuse. This may be familiar, but it raises a problem about how one is to describe, or at least set limits to, the term in play. When the OED defines hypocrisy in terms of the couples truth/falsity and reality/appearance, it is being quite specific; on the other hand, it remains less so in not elaborating on the content of the terms of those couples. Clearly, to be too precise would be to restrict unduly the range of historical enquiry and to become enmeshed in essentially trivial pursuits of a single definition of hypocrisy, as some contemporary philosophers have done. But what of the description which is too broad? This, I think, has plagued debate in the theological context.[9]

It is true that the simplest position on lying and truth is that God forbids all falsity, all deception, all hypocrisy. Augustine and some subsequent theologians have articulated this view.[10] One truth, then-one Truth. But even so, things immediately become more complicated. As Montaigne put it, 'If, like truth, the lie had but one face, we would be on better terms. For we would accept as certain the opposite of what the liar would say. But the reverse of truth has a hundred thousand faces and an infinite field.'[11] Augustine recognized this and therefore immediately introduced divisions into his scheme. There emerged those deceptions that were not, strictly speaking,

falsehoods; those falsehoods that were not quite lies; those lies which could be pardoned. And so it went on. Aquinas adding to the list with officious lies, jocose lies, and malicious lies, amongst many others.[12] Hypocrisy in such discourse assumes the characteristic of being highly technical and hugely diffuse. Thus attempts are continuously made to pin down what is a shadow haunting the genuine religious mind. In applying the finest distinctions to this spectre, the theologian merely adds to and boosts hypocrisy.

To strive for religious perfection, to demand ever greater displays of religious fervor, must actually produce hypocrisy. The stricter the requirements and exigencies of belief, the more likely real or imputable pretense becomes. So there is no better hypocrite and producer of hypocrites than the aggressively combative Puritan like Ananias in Ben Jonson's The Alchemist (1610-12). On the other hand, so freely do the accusations of hypocrisy run that the term itself assumes too broad a character. The religious hypocrite is frequently the believer of one faith as seen by the believer of another; he or she is being judged wrong, false, mistaken and incorrect. This, we must say, is not hypocrisy as such, for hypocrisy must apply to intentional falsity, to deceit, betrayal, treachery. This accepted we enter a realm in which hypocrisy assumes far more power and dynamism than it did in the religious discourse of the seventeenth century. Hypocrisy, I would like to suggest, leads its most energetic life in the political world. Hypocrisy occurs when people become hypocrites and when hypocrisy becomes political. Hypocrisy, in turn, becomes political in the course of the eighteenth century; that, at any rate, is what I would like to offer as a working hypothesis to take to the reading of texts of that period.

III. Hypocrisy alive in Politics.

It may be that some will invoke the work of Shakespeare as an example of earlier descriptions and denunciations of political hypocrisy. To respond to this briefly is difficult, but a number of points can be made. For one thing, the background to Elizabethan politics was formed by great interest and loud horror of Machiavellian 'politics', a politics of restricted compass and narrow 'policy' objectives.[13] The term 'policy' captures the full run of Machiavellian intrigue, manipulation and treachery, and that term may without great loss be resolved into the twin components of force and fraud. In other words,

'politics' to many Elizabethans suggested a landscape bounded by acts of force and fraud, and this seems to be just the kind of landscape in Shakespeare's plays (plays which frequently resort to debate in terms of force and fraud).[14] The political figure here, and in much later Elizabethan and early Jacobean drama is the Machiavellian villain who struts the stage in innumerable guises, committing every conceivable crime, revelling in stratagems.[15] Next, it may be noted that not only is 'politics' translated into 'policy', but it is further reduced to individual actions; even in Coriolanus (written c. 1608), the broader political context never unfolds, for Brutus and Coriolanus during the long drawn-out election campaign which fills most of acts II and III represent none but themselves. Hypocrisy, then, functions here in narrow policy terms, as well as on the individualistic level. It is goal-centered, aim-orientated, not a general moral and behavioral category. It characterizes the innate ambition, not the social force.

As hypocrisy develops during the eighteenth century, it sheds its religious-general and policy-individualistic nature and becomes a prominent feature of the socio-political world. It might be said that hypocrisy itself had been growing in England since the Glorious Revolution of 1688; as the country sought to settle down to a recuperative life, so moral deviation was the target of attack. By satire, or by outright denunciations, the figure of the hypocrite was at the center of many diverse censures. Acting itself was fiercely assaulted, a point worthy of mention when we recall the revealing connotations of the word 'hypocrite'. The Greek _ , from which the word is derived through the Old French, meant primarily the acting of a part on the stage, from , to give an answer, to speak dialogue. So hypocrisy offstage had enormous resonance as a term of approbation when the theatre itself was a source of derogatory imagery.

That the attacks on hypocrisy occur within the general context of political insecurity, and hypocrisy itself falls into a wider debate about the social and moral order (it seeming to support while at the same time undermining the status quo), may be seen in many different ways. The figure of the hypocrite in the eighteenth century could not conceivably have the same comic proportions as say, the bumbling

Dominic in John Dryden's The Spanish Fryar (1681). Moreover, in the 1700s the hypocrite assumes far greater importance within any particular drama: he becomes the titular character holding together the strands of the drama, as in Colley Cibber's The Non-Juror (1717) and William Congreve's turn-of-the-century Double Dealer (1694). Here the hypocrite is the main focus of the plot, the moral point of the tale. The same could, of course, be said of Tartuffe in Molière's earlier play, but this perhaps reinforces my point about the politicization of hypocrisy. For the odious hypocrite in Molière's comedy (Tartuffe feigns extreme piety), is reborn and given a political twist by Cibber in The Non-Juror, an adaptation of Le Tartuff (1664), and again by Isaac Bickerstaff in The Hypocrite (1792). Tartuffe insinuated himself into the household of the credulous Orgon; in Cibber's play, the hypocrite is a traitor figure in the midst of society at large, a perfect symbolic representation of the subversive Jacobite cause.

I shall return again to the figure of the 'political hypocrite', but first I would like to consider some further contours of eighteenth-century hypocrisy. I have tried to suggest that hypocrisy during this period assumes greater significance in society than it had done previously. I would now like to add that hypocrisy finds itself accepted as a form of belief and behavior in arenas from which it would before have been fiercely and promptly ejected. Take a famous spokesman against all forms of dissimulation and conceit like Jonathan Swift. Like his predecessors, Swift continues to picture hypocrisy in predominantly religious terms, or rather, in anti-religious terms; to him, 'only Christianity is without hypocrisy; it appears to be what it really is; it is all of a piece.' Like others, Swift saw the hypocrite brought prominently into view on the stage: here the figure was made in the process of acting itself and by unmasking the theatre, so too, the presence of acting could be unveiled. Two more novel attitudes may, however, be discerned. First, Swift begins the shift into properly political terrain and takes with him his denunciation of hypocrisy. As Milton describes the archangel Uriel being deceived by the flattery and dissimulation of Satan, so Swift extends this to picture even the best Christian disarmed by the hypocrisy of political sheriffs and kings.[16] There is no longer that comforting thought that God, an audience of one, looks down in consternation

from the heavens at the strutting, posturing, declaiming of his childish actors below. This so-called political move is not to my knowledge explored in any detail by Swift and remains less developed than the second attitude to which he gives voice. This is to picture hypocrisy, if not as a necessity, then at least as being 'preferable to open vice'. This represents, I believe, a fundamental about-turn in moral priorities and provides immediate impetus to the discussion and the fact of hypocrisy in the eighteenth century.

Writing about the proposals he made in 1709 in his Project for the Advancement of Religion, Swift considers the objections that to make 'Religion a necessary Step to Interest and Favour, might encrease Hypocrisy among us.' To this he readily assents, but then continues:

> if One in Twenty should be brought over to true Piety by this, or the like Methods, and the other Nineteen be only Hypocrites, the Advantage would still be great. Besides, Hypocrisy is much more eligible than open Infidelity and Vice: It wears the Livery of Religion... and I believe, it is often with Religion, as it is with Love, which, by much Dissembling, at last grows real.[17]

This sentiment, I hasten to add, is not exclusive to the eighteenth century, but it is during this period that it finds its fullest radiance and resonance. Certainly, the importance of traditional virtues - honesty and openness - was still recognized, particularly in the long wake after 1688 when the stability of England seemed under threat from Jacobites, from corrupting economic changes, and from the growth of an idle surplus population.[18] The necessity of virtue was loudly declared in Cato's Letters (written 1720-23), and by John Gay, Henry Bolingbroke and others. But another voice may be heard, resoundingly in Bernard Mandeville's Fable of the Bees (1714) in which society is depicted as no longer dependent, as social theorists had thought in the previous century, on the wide dispersion of virtue - or, on the ability to recognize a public interest and act upon it - but on vice. The social order was guaranteed by the free and unhindered practice of pride, selfishness, lust, fraud and hypocrisy.[19] That same questioning of some central tenets of Christian morality could be discovered in countless other, less provocative, texts. Here is a typical example from the Gentleman's Magazine:

> The old maxim, that "honesty is the best policy", has long ago been exploded: but I am firmly of the opinion, that the appearance of it might, if well put on, promote a man's interest, tho' the reality must destroy it. I would therefore recommend it to persons of all

> locations... to put on now and then the appearance of a little honesty... To deceive behind the mask of integrity, has been deemed the most effectual method.[20]

The writer here does not elaborate the purpose for which he advocates hypocrisy and the reason for this is plain: the act of putting on masks is no longer restricted to a narrow circle but now recommended to 'persons of all locations'. Hypocrisy, the wearing of masks, the actor's Larvatus prodeo, has here become a counter in the play of communication: man-as-actor, the perpetually disguised, now forms part of the public political vocabulary. To some degree, the masquerade itself symbolized this. One of Joseph Addison's contributions to the Spectator in 1711 had already fulminated against the midnight masque, declaring that 'the whole Design of this libidinous Assembly seems to terminate in Assignations and Intrigues.'[21] In a society which clung hard to rigid class demarcations, masks and other theatrical ceremonies allowed men and women to seem to be what they were not. Our records about the political use of masks amongst the lower classes during this period are scanty, but the work of E.P. Thompson has showed a world fissured into plebs and patricians but joined in its adoption of a multitude of forms of dissimulation. The gentry may have relied on symbolic, iconological forms of hegemony, but this reliance was continuously tested and contested by what might be termed a counter-theatre. This was not the play of studied poses and stylish gestures, but the theatre of sedition and threat, of effigy burning, boot hanging and charivaris.[22] Again, I stress that the wearing of masks was not an eighteenth-century innovation; though used only on special occasions or banquets in classical times, they were deployed as a means of dissimulation in the Renaissance period. According to a Toulouse man of letters:

> Chacun veult feindre et colorer sa ruse.
> Trahison gist souz beau et doux langage.
> Merveille n'est si tout le monde abuse,
> Car chacun tend à faulser son visage.[23]

What I would suggest, however, is firstly, that masquerades and imposture through false appearances were far more widely practiced during the eighteenth century than before, and by a larger segment of the population; and secondly, that masquerades assume a far more threatening, seditious presence than they had previously.

This last point returns us to the question of the political character of hypocrisy. The actions and beliefs of the hypocrite assume a native political dimension by virtue of that person's situation in a

moral and political order widely castigated for its corruptions and venality. That situation, it must be said, is one in which the person consciously and willfully immerses himself or herself. In the famous speech which Shakespeare writes for Jaques,

> All the world's a stage,
> And all the men and women merely players:
> They have their exits and their entrances;
> And one man in his time plays many parts,
> His acts being seven ages...'[24]

there is no connotation of man-as-hypocrite. As I have tried to establish, hypocrisy involves intention and here the 'acts' through which man passes are stages through which he must pass. An eighteenth-century 'equivalent' of this Shakespearian theme of the seven ages of man is William Hoarth's Rake's Progress (1735) in which role-playing is also examined. But the difference between the two is crucial, for in the latter there is no inevitability to the progress. In each of Hogarth's scenes, the merchant's son who acts the role of the aristocrat is faced with choices: in the first he can face or evade self-recognition, in the second he can opt for pleasure or wisdom, and so on. The attack on role-playing is as clear here as it was muffled in As You Like It (c. 1599). Hogarth can launch his political attack on masking because he recognizes that social beings have alternatives available. But what, it might be asked, were those alternatives? One of the most candid answers may be found in the work of Henry Fielding, Hogarth's friend and ally, in his now-neglected essay on the knowledge of the characters of men, a piece of work of great pertinence to the understanding of attitudes to human personality and the measurement of emotion and sensibility.[25]

Hypocrisy, declares Fielding, is 'the Bane of all virtue, Morality and Goodness', and to combat it is 'to arm the honest, undesigning, open-hearted Man, who is generally the Prey to this Monster.'[26] The monster in question is a political animal, and Fielding wrote his critique with an eye on the jostle for places which shuddered through the English political scene after the fall of Walpole in 1742, by which time Fielding had been enlisted into the ranks of the opposition. How hypocrisy lives - how it might be destroyed - are issues of a fundamentally political and moral nature to Fielding: affectation, deceit, lying and hypocrisy are not characteristics of 'fashionable' society alone, but play a central role in organising life up and down the land. So it is with urgency that Fielding declares against those

'great Arts, which the Vulgar call Treachery, Dissembling, Promising, Lying, Falsehood, etc. but which are by GREAT MEN summed up in the Collective Name of Policy, or Politics, or rather Pollitricks.'[27] The problem is that 'few of none should have stood up the Champions of the innocent and undesigning, and have endeavoured to arm themselves against Imposition.'[28]

Here then, in a sentence, is the constituency which must form the new antihypocritical army. It is a small grouping, for the honest have, so it seems, little to gain when hypocrisy has the upper hand and deceit can proffer advancement on the unscrupulous and designing. 'The whole world,' Fielding laments, 'has become a vast Masquerad, where the greatest Part appear disguised under false Vizors and Habits; a very few showing their own Faces, who become, by so doing, the Astonishment and Ridicule of the rest.'[29] In a socio-political world in which the citizen failed to make adequate moral distinctions or even to judge interests, the hypocrite could flourish. The anti-hypocrites, try as they might, were powerless to defend the innocent; this is why in the universe drawn by Fielding in his essays and novels, the hypocrite is always pitched in losing battle against the simple, candid, open-hearted character. Parson Adams, for example, only gradually discovers how his own simple-mindedness is being used in the service of others; it is this kind of essential imbecility which Fielding is forced to resort to as a 'Match for Cunning'.

IV. Some Closing Remarks

The purpose of this brief and exploratory paper has been to open up to further investigation a phenomenon which has thus far been all but ignored by historians. I believe that further research may reveal its use as a sort of deep probe into a forgotten layer of cultural life. I have stressed certain features of hypocrisy: its presence in religious discourse and its narrow political connotation before the eighteenth century, and its general moral an political significance during the 1700s. I have only touched the surface of what I believe is a large mass of historical material dealing with hypocrisy; one has only to think of works of literature of the period which deal with or prominently figure hypocrisy to obtain an indication of the importance of the phenomenon. The novels of Fielding [especially Roderick Random (1748) and Ferdinand Count Fathom (1753)]; the plays

of Richard Brinsley Sheridan [especially The School for Scandal (1777)] and Oliver Goldsmith [especially She Stoops to Conquer (1773)] are just a few nuggets from what historians will find to be a rich seam of information.

It will be noted that I have dwelt on the early and mid-eighteenth century and stressed what I believe to be a prominent aspect of hypocrisy, namely its political character. There is much to be said, and far more to be researched, on the hypocrite as a psychological type. Elsewhere I have touched on the problems encountered during the eighteenth century by those who sought to recognize hypocrites by their appearance, and this is an area of investigation which would repay further study by historians of psychology and characterology.[30] My impression is that the hypocrite as a distinct personality, rather than a vehicle for the articulation of political, moral and social beliefs, occupies an increasingly prominent place in the fiction of the late eighteenth and early nineteenth century. As examples, I would cite the character of Falkland in William Godwin's Caleb Williams (1794) as well as Jane Austen's depiction of Wickham in Pride and Prejudice (written in the 1790s, published 1813) and Henry Crawford in Mansfield Park (1814). Once we enter the nineteenth century hypocrisy begins to span the gamut of villainy, from sharping and suave pretence to plain forgery; it comes to be depicted and denounced by a great chorus thundering through the decades, with Charles Dickens, T.H. Huxley, J.S. Mill, Thomas Carlyle and Arthur Clough at its head. In fiction, the hypocrite assumes truly human proportions: the figure is psychologically real and politically complex. Though the eighteenth century probably witnessed more hypocrisy and more writing about hypocrisy, the nineteenth brought into being the fully three-dimensional hypocrite. Today we have no responses to hypocrisy of an intellectual caliber to touch those of Rousseau, Voltaire, Swift, Fielding, Helvétius, Price or Hutcheson (to name but a few prominent eighteenth-century authors on the subject). On the other hand, our failure seems all the greater when we look to our age to match the fictional hypocrites created in the Victorian era-characters such as Pumblechook, pecksneiff and Uriah Heep in Charles Dickens' Great Expectations (1861), Martin Chuzzlewit (1843-44) and David Copperfield (1849-50\0; Becky Sharp in William Makepeace Thackeray's Vanity Fair (1847-48); Bulstrode in George

Eliot's Middlemarch (1871-72); and Melmotte and Slope in Anthony Trollop's The Way We Live Now (1875) and Barchester Towers (1857).

In the 1700s, hypocrisy seems to have been a landmark by which society found its bearings. In the 1800s, it was a fiction by which truth could be measured. Today, the phenomenon of hypocrisy and the figure of the hypocrite should stand as subjects for further inspection. I hope that this brief survey will prompt such research.[31]

Notes

1. Frederick II (1741, p. v, actually published in 1740). The bibliographical history of this text is extremely complex, but is deciphered in Benoist (1915, Fleischauer (1958), and Von Galéra (1926). One problem, indeed is the very title of Frederick's text, which in some versions reads 'Anti-Machiavel' and in others 'Examen du Prince de Machiavel'. The review of the text in Nouvelle Bibliothèque (1740, pp. 291-99) refers to it simply as 'Anti-Machiavel (1743, p. XIX), this was a common form of designation applied to works critical of Machiavelli's views from the sixteenth century onwards. A preliminary search through contemporary reviews of the first edition bears this out; e.g. Bibliothèque raisonnée des ouvrages des savans de l'europe (1740, pp. 372-93), Nouvelles privilegiées (1740, pp. 442-44).
2. Frederick (1741, pp. 199-200; English translation, 1741, pp. 193-94).
3. See Aronson (1983; a shortened version appeared a year later in Britain).
4. Montaigne (1580, I, XXXI, p. 240).
5. Trilling (1972).
6. Nietzsche (1972, p. 77); elsewhere Nietzsche describes masks and their necessary uses (1970, pp. 118-20) and of the need for great men to be able to lie rather than to tell the truth (1968, pp. 451, 505).
7. Contextualism exists in both its explicit and implicit forms; for examples of each, see respectively, Bonhoeffer (1955, p. 329) and Bok (1980).
8. Borges (1981, pp. 284-85).
9. Hypocrisy has been defined on the plane of motives and moods by Ryle (1949, pp. 172-73), in terms of will and principle by Hare (1963, pp. 79-80), and by reference to advantage and self-interest by Szabados (1979, pp. 195-210). Other philosophical studies of hypocrisy recently published include Kittay (1982), Margolis (1963) and Rotenstreich (1956).
10. See on this Bok (1980, chapter 3).
11. Montaigne quoted in Bok (1980, p. 3).
12. See Bok (1980, p. 34).
13. See Prax (1958, pp. 109-44); Orsini (1946), and Rabb (1964, pp. 51-76).
14. The discussion of 'force' and 'fraud' appears in Machiavelli (1532, pp. 297-308) and Machiavelli (1531, pp. 310-12). Shakespeare refers to the terms in many places, e.g. Henry VI, Part One, IV, iv; Henry VI, part Three, IV, iv; Rape of Lucrece, line 1243.

15. See Rabb (1964) and Meyer (1897).
16. Swift (1939-74, IX, p. 288 and V, pp. 116-17).
17. Swift (1939-74, II pp. 56-57). On Swift and hypocrisy, see Ehrenpreis (1983, pp. 282-89). On the Project, see Quinlan (1956). A study which investigates Swift's seriousness in the advocation of hypocrisy as an acceptable social norm in his work is Kay (1975).
18. On this, see Dickson (1967) and Kramnick (1968).
19. See Horne (1978, chapter 1) and Goldsmith (1985).
20. Gentleman's Magazine (1756, p. 335). For other samples see the same publication: 1751, pp. 79-80; 1747, pp. 229-31; 1740, pp. 117-18; and 1737, pp. 549-51 and 553-55.
21. Addison, Steele and others (1711-12, pp. 26-27; see also pp. 43-46). For further perspectives, see Rogers (1985, pp. 40-70) and Castle (1983-84).
22. See Thompson (1974, p. 395; 1978, p. 143; 1971, pp. 135-36; and 1972). Also interesting on the Wilkes riots as displays of oppositional ritual is Brewer (1976, pp. 182-200).
23. Quoted in Davis (1985, p. 151). Elias has some pertinent remarks in (1983, chapter 3) as has Greenblatt (1984).
24. As You Like It, II, vii.
25. Fielding (1743, pp. 153-78).
26. Fielding (1743, p. 4).
27. Fielding (1743a, II, v); see Irwin (1941, pp. 48-49) which treats Fielding's use of the theme of the trickster/hypocrite/-politician.
28. Fielding (1743, p. 153).
29. Fielding (1743, p. 155, my emphasis).
30. See Shortland (1985).
31. Three excellent sources are Berkshire Review (15, 1980), Daedalus (Summer 1979), and Shklar (1984, chapter 2).

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THE IMPACT OF PSYCHOANALYSIS IN CONTEMPORARY SPAIN (1940-1984)

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Psychoanalysis is a name for a complex reality. Created as a specific theory about some specific abnormal and pathological behavior, it has developed as a new interpretation of human nature, with all sorts of social, and cultural implications. As such, its evolution has been affected by political changes and regimes, either in a positive or negative way, depending upon the circumstances.

Spain has proven not to be an easy place for psychoanalysis. Although in the first decades of our century, the psychoanalytic ideas evoked considerable interest among intellectuals, and great attention was paid to the works of Freud, in an early translation into Spanish, things deeply changed after the implantation of a new political regime, in Spain, headed by General Franco, at the end of the Spanish Civil War in 1939.

We have elsewhere offered an overview of the introduction of Freudian thought in Spain before the war, (Mestre, Carpintero, Perez Delgado, 1984; Carpintero y Mestre, 1984), and have there shown the critical reception those ideas provoked in education, psychiatry and psychology, philosophy and law, and general culture, during the twenties and thirties in Spain. Now, we intend to offer a first draft of the evolution of psychoanalytic theories in more recent days, that is under the General Franco's regime and in the first years of the restaured Spanish democracy. This approach intends to be a quantitative analysis of published works by Spanish authors, to be followed by a more detailed study of their respective contents.

Main consequences of Spanish war on the psychoanalytic group.

In the thirties, under the newly established Spanish Republican government, many progressive movements and tendencies found their way into cultural and ideological fields in Spain. Painters such as Picasso and Dali, film directors such as Bunuel, dramatic authors such as García Lorca, philosophers such as Ortega and Unamuno, had

created an impressive amount of creative work only to be compared with the Golden Age of the Baroque Spain in the XVIIth century.

During these years, the impact of psychoanalysis theories became more prominent, as we have shown in our previous work, and a small group of physicians, lawyers and teachers began to incorporate those theories into their respective subjects. The most active member of the group is, without any doubt, the psychiatrist Angel Garma, who had been trained by Theodor Reik in Germany, and began to publish true psychoanalytic works in medical journals, soon followed by his first book El psicoanálisis, la neurosis y la sociedad (Psychoanalysis, Neurosis and Society) (1936) with some personal contributions.

It is worthy to note that most of the intellectuals, professors and authors that were influenced by the psychoanalitic ideas are to be found among the vast amount of distinguished persons who were forced to go into exile, when the war ended. This is the case of the philosopher Ortega y Gasset, the psychiatrists Emilio Mira y Lopez, Gonzalo R. Lafora, Angel Garma, the lawyer Luis Jiménez de Asúa, the educators Lorenzo Luzuriaga and Luis de Zulueta, who among many others, had to leave Spain. Most of them took refuge in Latinamerican countries.

Another consequence was the disappearance of some well-known journals that published articles about psychoanalytic questions. Such was the case of Archivos de Neurobiología (edited by Lafora, Ortega and Sacristán), the Revista de Occidente (edited by Ortega), the Revista de Pedagogía (edited by Luzuriaga) that had gained a great respect among intellectuals. This change deeply affected the possibilities of information exchanges about those subjects, and the horizon for psychological theories became more limited.

Last, but not least, the translation of Freud's complete works, that had greatly contributed to the dissemination of those ideas among Spanish readers, accompanied by a brilliant introduction written by Ortega, was out of print, and for some years those works were beyond the Spaniards' reach. Only in 1948, nine years after the end of the war, a very expensive hard cover edition in two volumes, put an end to this situation, although the works kept being difficult to find for the great majority. As a matter of fact, one of us remembers quite cleary how that two-volumes edition of Freud's works were kept in a special section of a University library, and only lent under very special conditions to the students, at the end of the

fifties, when more liberalized airs seemed to gain ground in the Spanish regime under the influence of recent changes in foreign policy, such as the new U.S.-Spain treaty, and the return of foreign ambassadors to Madrid. It is necessary to wait until 1966 to find the first paperback edition of one of Freud's works - The psychopathology of Everyday Life-, that was soon followed by other works, and turned out to be an overwhelming editorial success.

Till that moment, the gap created by disappearance of that old edition was in part filled with some rare Latinamerican translations, that were sold as smuggled goods in progressive bookshops.

Spanish works on psychoanalysis (1940-1984)

In order to evaluate the impact of psychoanalysis in the Spanish society, and more specifically in its science and culture, research upon the works dealing with that subject has been carried out, including all those which made some mention in its title to psychoanalytic terminology.

The works in that research have been grouped in two categories, one including entire books, the other for journal articles, book chapters, congress communications or brochures. In this way 58 books and 114 articles have been detected.

It is not without interest the temporal distribution of those publications, that is offered here (Table 1; fig. 1).

TABLE 1. Temporal distribution of works on psychoanalytic subjects in Spain (1940-1984).

Years of edition[1]	1942	1947	1952	1957	1962	1967	1972	1977	1982	TOTAL
Books	1	1	4	1	7	6	9	16	13	58
Articles	5	10	7	13	16	14	25	20	8	118

(1) The central year of each five-year period is indicated

The median of the distribution for books is placed between 1972 and 1977, while for the articles it is in the 1967 period. The number of articles doubles that of the books, but in more recent years, the tendency of articles to exceed the number of books seems to reverse. Although a continuous growth appears in the distribution, it is in the seventies when more than a third of all the works (38.3%) have appeared. Some important changes in Spanish culture may be put in close connection with these findings. In 1968 a program for a license in psychology was established for the first time in the universities, after many years of struggle for its existence at the academic level (only two professionals post-graduated schools had previously existed). This change was inmediately followed by the appearance of some journals, that brpigjt new possibilities for people working in psychology (Peiro & Carpintero, 1983).

The subject-matters of the works

In order to determine the trends of the specific influence of psychoanalitic theories upon the Spanish researchers, we have carried out a content analysis of the various terms included in the title of their works.

After counting every meaningful term appearing in titles (articles, prepositions and adverbs were omitted at this end), we have established the frequency distribution and the number of occurrences with statistical significance (at level $p \neq 0.05$), according to the sociometric procedure for content analysis we have presented elsewhere (Carpintero & Peiro, 1981). Briefly stated, terms appearing in titles are considered as "positive elections" to be associated with the main descriptor (the psychoanalytic topic), and a psychometric matrix is elaborated, through which the statistical significance of term occurrences are determined.

The study has been done separately for books and articles. Among books, the terms distribution has shown only three terms with statistical significance: "psicoanálisis" (n=30), "Freud" (n=13), and "psicoanalítica" (n=4). (See Table 2).

TABLE 2. Frequency distribution of terms in titles

Number of ocurrences	Number of different terms a) in books	b) in articles
1	97	197
2	18	37
3	7	12
4	1	4
5	-	1
6	-	1
8	-	2
10	-	1
13	1	1
22	-	1
30	1	-
Total	125	256
X occurr. per term.	201/125=1.61	395/256=1.54
f of terms (p=0.05)	3.64	3.73

TABLE 3: Most frequent terms employed in book/article titles

a) Books (n=125)		b) Articles (n=256)	
Psicoanálisis	(30)	Psicoanálisis	(22)
Freud	(13)	Freud	(13)
Psicoanalítica	(4)	Psicoanalítica	(10)
		Psicología	(8)
		Psicoterapia	(8)
		Freudiana	(6)
		Neurosis	(5)
		Historia	(4)
		Inconsciente	(4)
		Personalidad	(4)
		Psiquiatría	(4)

The most employed terms in book titles are the most common ones that could be found in this kind of works. That seems to fit well with the situation in which the only common factor to the works under study is their psychoanalitic approach or interest; the concrete aspects that have been considered in those books are restricted to individual pieces of work. It is interesting to note that in those books the specific reference to Freud is very high, and it points to a certain identification of psychoanalysis with freudian thought among Spanish authors.

At the sometime, a more complex image seems to emerge from the articles. The high frequency of "psicoanálisis" also shows here that that is the most common dimension to these works; and here as well the "Freud" term appears in a very high position, confirming the supposition of the strong connection, almost an identification, of the freudian line in psychoanalysis with the whole movement (esspecially if "Freud" and "freudiano" (freudian) are taken together).

Four great disciplines are in close contact with our topic: Psychology, Psychotherapy, Psychiatry and History. It is clear that the first three are fields in which psychoanalysis has had its greatest impact, as an alternative school of thought or a sort of "paradigme" that was offered to researchers. The presence of "history" here may be explained in a double sense, psychoanalysis as an interpretative guide to the knowledge of the past, that implies a psychoanalytic approach to their subject for the historians; or psychoanalysis as a historical topic to be studied by historians of psychology and related people; both cases appear in the studied sample.

Among the high frequency terms in articles are also included three relevant psychological concepts: "neurosis", "unconscious" and "personality". All of them are basic terms in order to build an adequate image of the psychoanalytic theory. They also incorporate the psychopatological dimension ("neurosis") through the original problem that gave birth to the freudian theory, and one of the main constructs of the theory ("unconscious"). "Personality" here seems to indicate that this is one of the thematic fields where psychoanalysis has reached wider acceptance. Contrasting with what seems to be the nuclear interests among Spanish authors is the absence among these terms of some very common concepts in psychoanalitic works, in current literature, such as "art", "society" or "Oedipus Complex";

and other terms such as "sexual" or "dreams", also show frequencies under the significance level adopted. To summarize, we find in books and articles a freudian line to dominate, and whose main interest lies in, psychological questions.

Authors working on psychoanalytic topics.

The Spanish contributions to psychoanalitic literature are due to the efforts of a considerable number of researchers, and intellectuals. They represent an interesting sample of differents groups that were attracted to psychoanalysis for a great variety of reasons - its psychological values, its therapeutic meaning, its anthropological content, among others.

The great majority of the authors made only one contribution, according to our data file, for the whole period. When these two kinds of contributions are considered - books, and articles - it comes to be 176 pieces of work, that have been published by 114 authors. That means an average of 1.54 works per author, for a studied period of more than four decades (44 years), which seems to be a very low level of contribution.

In this concrete field the same characteristic distribution of productivity that appears everywhere in science is also found here. The presence of a small and very active group of researchers, and, on the other hand, a vast majority with a single contribution (Table 4), is from our data clearly established.

TABLE 4: Productivity of authors on psychoanalysis

a) Books		b) Articles	
Number of works contributed	Number of authors	Number or works contributed	Number of authors
1	32	1	56
2	4	2	7
3	2	3	3
6	2	4	1
		5	2
		7	1
		8	1
		9	1

The existing correlation between scientific productivity and quality of work is a well-known fact (Cole & Cole, 1973). In any case, here we are interested in establishing those authors that could be considered as prominent names in the dissemination of psychoanalytic ideas among Spaniards. (Table 5)

TABLE 5: Most productive authors in psychoanalytic topics

a) Books	Contributions	b) Articles	Contributions
Angel Garma	6	José Molina-Nunez	9
Juan José Lopez-Ibor	6	Juan José Lopez-Ibor	8
		Pedro Fernandez-Villamarzo	7
		Angel Garma	5
		Juan Rof Carballo	5

Here we intend to present here some biographical data, in order to gain a more comprehensive knowledge of their real significance in the Spanish culture of to-day.

Both groups taken together, only two names seem to have published books and articles at a very high rate: Juan José Lopez Ibor and Angel Garma. They are both well-known psychiatrists. Garma (Bilbao, 1904) as it has been previously said, was the leading psychoanalist in the pre-war Spain, but he was forced to exile, and after establishing himself in Argentina, began to spread the new ideas there, and was one of the founders of the Argentina Psychoanalytic Society, in 1942. Many well-known Argentinean psychoanalists did their training analysis with Garma, such as Raskovsky, and Pichon Riveire, and he also had time to write books and articles that spread these theories in Latinamerican countries. Among the books that should to be mentioned are: Psychoanalysis of the Dreams (1940), Sadism and Masochism in Human Behavior (1943), Psychoanalytic genesis and treatment of gastric and duodenal ulcers (1945), Psychoanalysis: Theory, Clinical practice and Technique (1971, 2e), where he offers many insights into the application of psychoanalytic theories to the treatment of psychosomatic troubles.

Juan José López-Ibor (Sollana, Valencia, 1906), on his side, became head of the Psychiatry Department at Madrid University soon

after the war. When, as a result of the war, the psychiatric society previously existing lost its force and entity, Lopez-Ibor founded a new Spanish Society for Neurology and Psychiatry substituting the older one, with the support of the new Spanish government. He also founded a new journal - Actas Luso- Espanolas de Neurología y Psiquiatría that came to fill the empty space created by the disappearance of Archivos de Neurobiología, that Dr. G.R. Lafora and his group had been editing. In his well-known book La agonía del Psicoanálisis (1951) (The agony of psychoanalysis), he presented a synthetic view of that theory from a critical point of view, accepting its dynamical dimensions, but at the same time rejecting its anthropological basis. Psychoanalysis, he stated, not only has ignored the specific dimension of humanism in man, but has denied it; and for this reason, he tried to integrate these ideas with some elements taken from existential philosophy (Heidegger), as well as with other factors coming from a Christian Weltanschauung. According to his ideas, the "id" would act as the leading part of personality, together with the need for pleasure, a need for self-development or self-actualization that is supposed to be in the human unconscious. In different places he stated that psychotherapy should be considered as a sort of pedagogy, capable of offering a reconstruction of the meaning of life for patients.

José Molina-Nunez, psychiatrist, also belongs to the generation of Garma and López-Ibor. It was probably from Dr. Lafora that Molina received his theoretical impetus. After his training he did a lot for the institutionalization of psychoanalysis in Spain. As such, he founded "Pena Retama" Center for Psychoanalysis (Madrid), that gave support to the first Spanish psychoanalytic journal, Revista Espanola de Psicoterapia Analitica (Spanish Journal of Analytic Psychotherapy), founded in 1968. In his works, he maintained the validity of the dynamic approach to therapy, and he was against the tendency to translate psychodynamic concepts into somatic or biological ones. He died around 1972, and at that time, some of his colleagues (among them his widow, M. Prado) took the direction of his enterprises, thus carrying on his work.

A former Molina-Nunez's disciple, Pedro Fernandez-Villamarzo, a psychoanalist, and priest, founded in Madrid another Center, the Oskar Pfister Center, as a nucleus of therapeutic activities and research. Most of his work centers on the psychoanalytic

interpretation of religion and culture, also, dealing with general topics of psychoanalysis.

The last name to be considered here is that of Juan Rolf Carballo (Lugo, 1905) one of the leading intellectuals of contemporary Spain, physician, and a true humanist. According to his view, psychoanalysis, as a way of inquire, has discovered new essential dimensions of man, such as transference, regression, and the pre-verbal world.

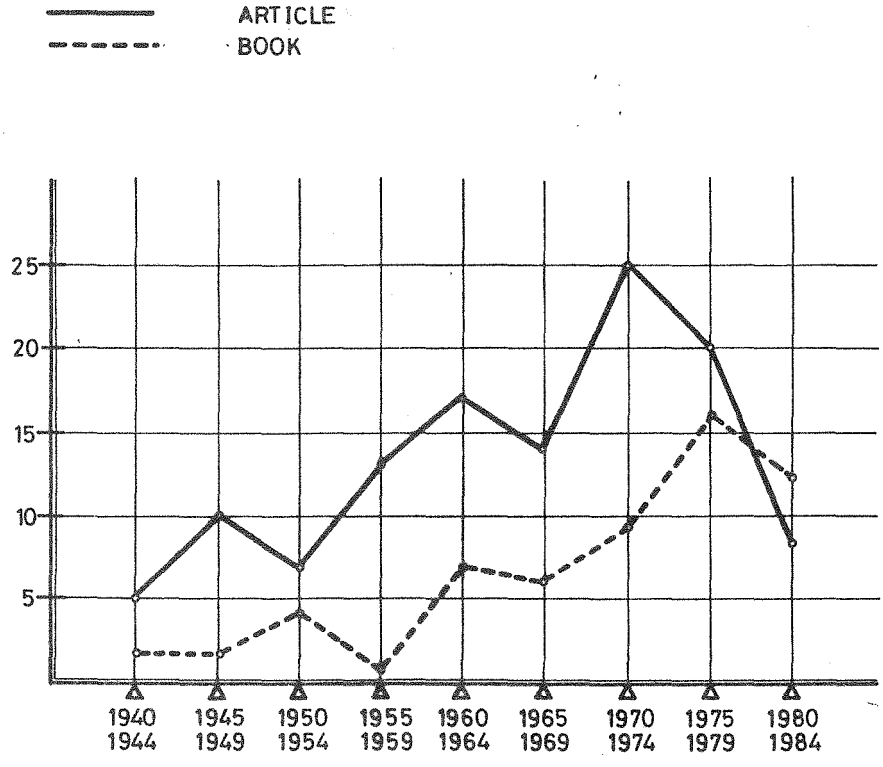
In considering such dimensions, the data coming from recent research in neurology and biology should be taken into account. Rof has always tried to integrate positive scientific thought with psychoanalitic suggestions. He has paid a lot of attention to the significance of the lymbic system for the knowledge of emotional behavior, or the split-brain research for the knowledge of the pre-verbal phenomena; he also pointed out the fact that the mother-son relationship is a dyadic structure that plays an essential role in the social and mental development of mean, a sort of biological counterpart of the psychoanalytic transference. In his works, Rof has offered many interesting points of view about art and literature, biology or philosphy, and he must be credited as one of the authors that has paid more attention to psychoanalytic ideas in Spain, esspecially in his well-known essays on psychosomatic medicine.

In conclusion, this small group of productive authors could be characterized through some common traits. All of them seem to have received a medical training, and are interested not only in theoretical but also in practical psychotherapy. They also belong to a common age level (all of them seem to have been born in the first decade of our century), and they seem to have been influenced mainly by the Freudian contributions.

Furthermore, most of them have been involved in activities directed at giving institutional support to psychoanalytic theories in Spain, mainly through the creation of specialized centers for therapy and research, and of journals for the dissemination of the new ideas. With the only exception of Lopez-Ibor, all these people have been maintained outside the Spanish establishment - and some of them, as Garma, forced to live in exile.

The impact of psychoanalytic theories in Spanish culture after the Civil War is a question to be further explored, in order to gain

Graf. I: TEMPORAL DISTRIBUTION OF WORKS ON PSYCHOANALYTIC SUBJECTS IN SPAIN (1940-1984).



a deeper understanding. The present contribution only has intended to offer a first and cursory overview of the topic.

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DE QUELQUES PARTICULARITES DE L'HISTOIRE DE L'HYPNOSE ET DE L'HYPNOTISME EN FRANCE

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Summary

One hundred years after Freud's visit to Charcot, there is a renewed interest in hypnosis in France. This interest is expressed on the one hand in research designed to produce hypnotic phenomena and on the other hand in criticisms addressed to psychoanalysis. In this last case, an attempt is made to show that Freud's rejection of hypnosis resulted in some crucial questions being left unanswered, especially concerning the theory of transfer.

My purpose is to show that:

Charcot's recourse to hypnosis was aimed at giving hysteria a proper status in psychiatry. But research into hypnosis developed at the Salpétrière soon became an unending task, thus repetitive, destined to a failure which may serve as a lesson today.

With Bernheim suggestibility became a universal property of the human mind and a therapeutic means. Janet, the heir of these two trends always claimed to be the founder of psychoanalysis. Nevertheless his psychotherapeutic practice of suggestion made him think of the "rapport" as a "need for guidance" in hysterics, which then places the therapist in the position of "director".

Freud did not restrict himself to the rejection of hypnosis and suggestion in his practice: he always distinguished between the two and differentiated suggestion from transfer. Therefore the matter concerns a conceptual break which has been a regular point of discussion in psychology.

Cent ans aprés la rencontre de Freud et de Charcot, on assiste actuellement en France, après plus de 80 ans d'un sommeil presque total, à un réveil de l'intérêt pour les recherches sur l'hypnose et la suggestion - d'ailleurs souvent confondues - en même temps que paraissent plusieurs ouvrages consacrés à la genèse de la théorie freudienne, à l'histoire du mouvement analytique, et que sont édités

ou réédités des textes appartenant à l'histoire de la psychiatrie. On trouvera en annexe une bibliographie (non exhaustive), d'articles ou d'ouvrages récemment parus, entièrement consacrés à la question ou qui lui accordent quelque actualité.

Le récent développement des travaux relatifs aux origines de la psychoanalyse et les formes que prend ce regain d'intérêt pour l'hypnose me semblent liés entre eux, et - indication qui restera ici incidente - corrélatifs de la situation complexe et tout à fait particulière de la psychanalyse dans la configuration actuelle du champs des sciences humaines en France.

Or, cette résurgence des recherches sur l'hypnose s'accompagne d'une absence de nouveauté qui a pour contrepartie - même lorsque les références obligées sont évoquées - la méconnaissance d'une histoire particulièrement édifiante en France. En même temps, la référence explicite à Freud y prend souvent la forme insistante du rappel de la dette impayée que la psychanalyse aurait contractée vis-à-vis de l'hypnose, créance dont l'occultation, voire la négation aurait pour effet de laisser en suspens des questions vitales pour la psychanalyse, à savoir celle du fonctionnement de l'institution analytique, celle de la transmission de la psychanalyse, c'est-à-dire, finalement, la question du transfert (1).

Rappelons que, dès le départ, c'est aussi un avis de débit que les psychiatres français ont adressé à la théorie freudienne (en même temps d'ailleurs qu'ils la critiquaient). Cette revendication s'incarne en la personne de Janet, héritier des deux principaux courants de l'hypnotisme en France, celui qui a pour chef de file Charcot à la Salpétrière, et celui de Nancy, dont les représentants les plus éminents sont Liebeault et Bernheim.

En effet, si à la fin du dix neuvième siècle il n'est pas de grand nom de la psychologie (cf. Wund, James, Delboeuf etc...) qui n'ait consacré un chapitre de son oeuvre à l'hypnose, en France elle a occupé le devant de la scène psychiatrique durant deux décennies, dans ce que les historiens s'accordent à appeler "lutte" entre les écoles de la Salpétrière et de Nancy. La confrontation des ouvrages consacrès totalement ou partiellement à cette histoire (cf. par exemple Barrucand (1967), Ellenberger (1974), Roudinesco (1982)), fait apparaître son extrême complexité et laisse à penser que le débat reste ouvert. C'est dire qu'il ne saurait être question ici que d'évoquer ce qui, dans cette histoire, me paraît permettre de

comprendre pourquoi la question du "rapport magnétique" (2) ne peut être posée autrement - quand elle l'est - que sous la forme d'une pratique de la manipulation, à des fins de démonstration ou d'éducation.

Selon Barrucand (1967), c'est à Mesmer et au courant fluidique, donc organiciste, qu'il faut rattacher Charcot et l'Ecole de la Salpétrière, tandis que Bernheim et l'Ecole de Nancy out comme précurseur l'abbé De Faria donc le courant psychologiste du magnétisme animal.

Toutefois, cette dinstinction, même si elle peut sembler légitime d'un point de vue descriptif, réduit les questions à une querelle de points de vue (physiologique contre psychologique). Rappelons que lorsque Charcot commence ses cliniques sur l'hypnotisme en 1878, c'est pour reproduire expérimentalement les phénomènes hystériques et démontrer ainsi la nature dynamique, c'est-à-dire non organique, de ce qu'il appelle cependant lésion, bien qu'elle ne soit pas décelable du point de vue anatomo-clinique. Barrucand, qui sur ce point reprend à son compte les termes d'un biographe de Charcot, qualifie ici de "défaillance" la démarche de Charcot dans une carrière de neurologue par ailleurs digne de la plus grande admiration. C'est pourtant là ce que Freud (1983, p. 72) considère comme l'apport majeur de Charcot dans son approche de l'hystérie: "En édudiant les paralysies hystériques qui surviennent après des traumatismes, il lui vint à l'idée de reproduire artificiellement ces paralysies qu'il avait préalablement différenciées avec soin des paralysies organiques et il se servit à cette fin de patients hystériques qu'il mettait, en les hypnotisant, en état de somnambulisme. Par une démonstration sans faille, il parvint à prouver que ces paralysies étaient le résultat de représentations qui dominaient le cerveau dans des moments de disposition particulière". En effet, pour Charcot, il s'agissait de faire de l'hystérie une entité nosographique à part entière. Ainsi, dans la Leçon du mardi 28 février 1888, à propos d'un cas de paralysie hystéro-traumatique, il déclare: "Ou'on nous parle après cela de l'hystérie Protée insaisissable (3). C'est le contraire qui a lieu (...) oui, en pathologie, le déterminisme règne partout, même dans le domaine de l'hystérie" (p. 101). C'est dans le même mouvement qu'il identifie l'hystérie masculine.

Mais il est vrai que si Charcot rend son identité à l'hystérie et fait de l'hystérique un malade, et non plus, comme on le voulait à

l'époque, une simulatrice - ce qui permet à Freud de comparer cet acte à celui de Pinel "déchaînant les fous" - le corps de l'hystérique devient durant deux décennies, à la Salpétrière, le lieu d'étranges vagabondages. Les Richer, Binet et Fere, Gilles de la Tourette, Richet etc... sont, de ce point de vue, devenus rétrospectivement indiscernables, tant ce qui frappe est la répétitivité, déjà soulignée par Wundt (1893) (4), de leurs expériences, plutôt que leur apport spécifique: on étudie les excitations sensorielles susceptibles d'induire sélectivement l'un ou l'autre des trois états de l'hypnotisme (léthargie, cetalepsie, somnambulisme) et on repère sur le corps les "plaques hypnogènes" qui, par pression, déclenchent ces états. On produit, à volonté, des anesthésies, des hyperesthésies, des modifications de la physionomie, des hallucinations positives et négatives, des vésications etc... C'est un corps qu'on peut, pour une de ses moitiés, mettre en catalepsie, pour l'autre, en léthargie. A l'aide d'un aimant on peut transférer la paralysie (par exemple) du côte atteint au côté sain du corps. En contrepartie, l'hystérique y gagne une exaltation de l'intelligence et de la mémoire, ou encore le don de télépathie ou celui de voir à travers les objets opaques.

Le répertoire, loin d'être exhaustif, de ces expériences indique ce dont il s'agit: c'est de susciter ce à quoi l'hystérique est toujours disposé, c'est-à-dire à "donner à voir" (5), afin de constituer l'hystérie comme entité clinique ayant un subtrat physiologique et une étiologie héréditaire; de l'étiologie sexuelle, répérée depuis l'antiquité, il ne reste qu'une trace, le privilège que garde la région ovarienne en tant que zone hystérogène. Il va de soi que dans cette perspective, la question du "rapport magnétique" ne se pose pas. Charcot l'indique on ne peut plus clairement dans la Leçon du mardi 24 janvier 1888 "Ils (les hystériques) sont très difficiles à manier mais cependant ils sont bien commodes (...) quand on a l'habitude de ces sujets là, on sait s'en servir". Quand à la suggestion, qui peut, éventuellement, être introduite dans l'arsenal thérapeutique, c'est un effet de l'hypnotisme, et l'auto-suggestion une caractéristique des hystériques. C'est pourquoi, même lorsque Charcot écrit, à la fin de sa vie, "La foi qui guérit" (à propos des miracles) (1893), il ne se rallie nullement, contrairement aux apparences, aux thèses de Bernheim: pour que la "faith-healing" s'exerce, et que le miracle ait lieu, il faut "des sujets spéciaux et des maladies

spéciales" (p. 87), c'est-à-dire des hystériques, qui sont "suggestibles au premier chef".

Il est possible, comme l'indique Ellenberger (1974, p. 86) qu'une meilleure connaissance de l'histoire du magnétisme animal aurait évité à Charcot et à ses élèves d'avoir l'impression d'explorer une terra incognita sans s'apercevoir qu'eux-mêmes suscitaient sa géographie. Il est vrai, en effet, que cet interminable repérage "scientifique" des manifestations de l'hypothisme -donc de l'hystérie- semble, retrospectivement, excéder largement son projet.

Non moins exhorbitante est, finalement, la position de BERNHEIM, qui, inversant la proposition de Charcot, fait de l'hypnose un résultat (parmi d'autres) de la suggestion: le sommeil prétendu hypnotique est le résultat d'une suggestion au même titre que, par exemple, les hallucinations, et la suggestibilité "une propriété physiologique de l'esprit humain" (Bernheim, 1897, p. 65), un dynamisme automatique, limité par les facultés supérieures du cerveau, c'est-à-dire contrôlé par la raison, laquelle "fait contrepoids à l'imagination et à l'automatisme cérébral" (bid. pp. 65-66). Ainsi, il n'y pas d'hypnotisme; il y a suggestion dès qu'une idée acceptée par le cerveau, car, toute idée tendant à se faire acte, actionne les centres automatiques, lesquels "actionnent les fibres nerveuses qui doivent réaliser cette idée "(ibid., p. 64). La suggestibilité est donc une notion psychologique greffée sur une physiologie empruntée à Jackson. Tout un chacun et donc suggestible, mais cette suggestibilité est variable selon les individus. Certains le sont excessivement parce que "l'influence modératrice du contrôle n'a pas le temps ou la force de se faire inhibition "(ibid., p. 67). En ce qui concerne l'hystérie, son status est, à tout le moins, problématique: ce n'est pas "une maladie autonome, c'est une réaction émotive, c'est un épiphénomène qui peut s'ajouter aux diverses maladies" (Bernheim, 1913, p. 160). Il s'agit d'une "disposition à faire des crises de nerfs" due à "un appareil nerveux symptomatique hystérogène (ibid., p. 77). Ainsi, si la suggestibilité est une fonction normale du cerveau humain, l'hystérabilité est une "aptitude", une "idiosyncrasie" et une exception, et, s'il se trouve que les hystériques sont particulièrement suggestibles, il convient de deistinguer hystérabilité et suggestibilité. De l'étiologie sexuelle il ne reste plus trace; même l'ovaire se voit refuser toute "vertu hystérogène spéciale (ibid., p. 396). Bien que tout ceci évoque quelque peu la vertu

dormitive de l'opium, Bernheim déplace la question du côte de la thérapeutique: lorsqu'il dénonce, en 1891, "l'hystérie de culture de la Salpétrière", c'est en ajoutant que les médecins suggestionnent l'hystérique à leur insu. C'est dire que la question du "rapport magnétique" n'est pas davantage abordée par Bernheim que par Charcot : de même que les cellules cérébrales sont actionnées par l'idée, "le sujet (est) actionné par l'affirmation" (1897, p. 71). Le traitement consiste, dans la plupart des cas, à déclencher la crise hystérique puis à l'enrayer par suggestion, afin de pouvoir affirmer au malade que le médecin qui a provoqué la crise peut empêcher sa venue, l'objectif de la cure étant d'éduquer le sujet en lui apprenant à inhiber ses crisis. Il est intéressant de noter à ce propos que "L'Hystérie" (1913) se termine sur des conseils relatifs à l'éducation des enfants et à la prophylaxie de l'hystérie chez ces derniers et que Bernheim, examinant la méthode psychanalytique de Breuer et Freud, dénie à l'abréaction tout efficacité théapeutique pour affirmer que celle-ci est due à "une éducation qui apprend aux sujets à dominer l'émotion de leurs réminiscences" (p. 246). Donc, pour Bernheim, il s'agit d'utiliser une propriété normale, la suggestibilité, afin de (re)faire une éducation. La méthode doit, d'ailleurs, être adaptée à chaque individualité, "insinuation douce" pour les uns, "persuasion vigoureuse" pour les autres. De la position qu'il occupe, d'où il tire ce pouvoir, nulle mention explicite. Tout au plus évoque-t-il ses rares échecs thérapeutiques et ses nombreux et faciles succès, qu'il attribue à sa tranquille assurance. L'envers de l'image qu'il nous présente ne réside-t-elle pas dans son obstination à affirmer, avec Liegeois, que sous une influence mauvaise, un sujet peut commettre des crimes, si toutefois le sens moral lui fait suffisamment défaut? Rappelons que c'est particulièrement la thèse des "crimes par suggestion" qui fit que le débat entre les deux écoles fut porté sur la place publique et, notamment, auprès des tribunaux, Charcot et, surtout, Janet niant énergiquement que de tels crimes fussent possibles. C'est, d'ailleurs, l'influence maléfique qu'un être peut exercer sur un autre qu'évoque l'hypnotiseur dans l'imagerie populaire et dans la littérature.

Ces deux courants aboutissent à Pierre Janet, dont les conceptions sont beaucoup trop complexes pour être présentées ici. Je m'en tiendrai donc à souligner ce qui me semble directement lié à mon propos.

Janet garde de Charcot sa conception de l'hystérie comme entité morbide ayant une étiologie héréditaire. Il reste pratiquement le seul à pratiquer l'hypnose après que tous l'ont abandonnée considère toujours qu'être hypnotisable est un signe d'hystérie: d'hystérie, donc l'hypnotisme, est un effet mental consécutif à un rétrécissement du champ de la conscience dû lui-même à un défaut de synthése psychique libérant l'automatisme des tendances premières (on peut voir ici aussi l'influence de Jackson via Ribot).

L'hypnotisme permet d'atteindre les pensées subconscientes, c'est-à-dire celles qui, du fait de la faiblesse psychique, ne sont pas maintenues dans le champ de la conscience personnelle.

Mais, tout comme Bernheim, Janet attribue principalement à la suggestion l'efficacité de sa méthode thérapeutique, l'analyse psychologique, conçue comme la dissociation des systèmes de souvenirs subconscients mis à jour sous hypnose, lesquels doivent être réintégrés dans la conscience ou remplacés par d'autres grâce aux injonctions de l'hypnotiseur, afin de "reconstituer la personnalité". Contrairement à ses prédécesseurs, Janet pose explicitement à diverses reprises (1889, 1898 par ex.) la question du "rapport magnétique" et lui consacre un article en 1897. S'il ne lui échappe pas que les sentiments "excessifs" de l'hypnotisé pour l'hypnotiseur sont le plus souvent ambivalents et résultent d'un déplacement, il ne s'attarde pas sur ces remarques et s'emploie à mettre en évidence trois états successifs consécutifs à un somnambulisme provoqué à des fins thérapeutiques: un état de fatigue, suivi d'une période "d'influence somnambulique", durant laquelle on observe un état de santé apparent, puis d'une période de "passion somnambulique" durant laquelle, l'influence cessant de se faire sentir, et les symptômes réapparaissant, le sujet demande à être hypnotisé le plus tôt possible par <u>son</u> hypnotiseur, comme un "morphinomane qui attend sa piqure" (1897, p. 119). Janet écarte une explication qui consisterait à attribuer cette demande à un besoin de suggestion (c'est-à-dire de guérison momentanée) pour faire de la suggestion une conséquence de la "domination de l'hypnotiseur", elle-même consécutive à un "besoin de direction" particulièrement important chez les malades hystériques ou psychasthéniques. Le problème étant de prendre la direction complète de l'esprit du malade tout en lui apprenant à se passer de cette domination. Il s'agit en dernière analyse, d'un problème "d'éducation de l'esprit" (ibid., p. 141). C'est donc à l'état mental des malades

qu'il faut rapporter ce "besoin de direction", et Janet, fermant le cercle, reprend à son compte la proposition du général Noizet (disciple de l'abbé de Faria) "d'appeler le magnétiseur un directeur" (ibid. p. 140). Ainsi, conclut-t-il, "ce petit phénomène curieux du "rapport magnétique" n'est pas insignifiant, il nous permettrait d'étudier un élément important des relations sociales, de comprendre l'origine de la dépendance et de la hiérarchie" (ibid. p. 143).

On sait que Janet revendiqua toujours la paternité de la découverte de l'inconscient (ou, plutôt, du subconscient) tout en adressant à la "psycho-analyse" maints reproches, dont celui d'exagérer les procédés thérapeutiques qu'il avait lui-même découverts sans voir qu'il s'agissait non d'une "exagération", mais d'une démarche radicalement différente.

En effet, contrairement à l'affirmation réitérée de quelques uns des partisans d'un "retour à l'hypnose" - dans la pratique thérapeutique, dans l'expérimentation ou, surtout, au nom de questions forcloses par la psychanalyse - Freud, dès lors qu'il a rendu hommage á Breuer et à la méthode cathartique, ne s'en tient pas à renvoyer l'hypnose et la suggestion du côté de la préhistoire de la psychanalyse ni de celui des énigmes auxquelles la Science devrait renoncer à s'intéresser. Certes, il affirme maintes fois que la psychanalyse est née de l'abandon de l'hypnose et de la suggestion, mais dans le même temps, il assigne aux questions qu'elles posent une fonction de repérage théorique et clinique, tout au long de son oeuvre.

Ainsi, Freud distingue-t-il toujours l'hypnose de la suggestion. On peut voir là l'héritage de Charcot: l'hypnose est un état, la suggestion l'effet d'une parole. En 1890, donc antérieurement à leur abandon comme méthodes thérapeutiques, il définit l'hypnose comme un "état psychique singulier qui présente une grande analogie avec le sommeil" (p. 14), cependant que "la parole de l'hypnotiseur - qui fait surgir les effets magiques que l'on vient de décrire - est appelée la suggestion". Cette distinction est, ultérieurement, répercutée au niveau des procédés thérapeutiques, dans la distinction entre méthode cathartique et thérapie par suggestion. En 1904 (a) par exemple, après avoir attribué à l'état hypnotique "l'élargissement du conscient" qui permet l'abréaction, il distinque la méthode cathartique de tous les autres procédés parce que son efficacité thérapeutique "ne repose pas sur un ordre suggéré par le médecin" (p. 2).

En outre, il revient à plusieurs reprises, dans des textes fort éloignés les uns des autres dans le temps, sur l'épure qu'a constitué la technique hypnotique pour la psychanalyse en "nous faisant connaître certains processus de l'analyse sous forme schématisée et isolée" (Freud, 1914, p. 106). Il semble cependant plus fluctuant quant aux relations réciproques qu'entretiennent hypnose et suggestion, puisqu'en 1891 il indique que certains résultats thérapeutiques ne peuvent être obtenus par la suggestion à l'état de veille et donc que la suggestion à vertu thérapeutique est un effet de l'hypnose, cependant qu'en 1916 (par exemple) il reprend à son compte l'idée de Bernheim selon laquelle l'hypnose est un effet de la suggestion. Entre temps, bien sûr, la psychanalyse a vu le jour, mais l'hypnose reste un état psychique et si, dès l'article de 1890, Freud centre son propos sur la question du "rapport magnétique" et compare l'attitude de l'hypnotisé à l'égard de son hypnotiseur à celle de "l'enfant à l'égard de parent aimés" et l'état hypnotique á l'état amoureux, c'est sur cette comparaison qu'il fait retour en 1921, pour donner de la relation hypnotique ce que LACAN appelle "la définition structurale la plus assurée (1964, p. 245): c'est une relation dans laquelle "l'objet est mis à la place de l'idéal du moi" (Freud, 1921, p. 178).

Si donc il y a un état hypnotique caractérisé finalement par la relation à l'hypnotiseur, et dont il faut repérer théoriquement le statut pour le distinguer de l'état amoureux, autre est le sort que Freud fait subir à la suggestion.

D'une part, il caractérise à plusieurs reprises, à l'aide de comparaisons devenues célèbres, les effets de la thérapie par suggestion à ceux de la méthode analytique: la suggestion procède, comme la peinture selon Leonard de Vinci, per via di porre, et la technique psychanalytique, comme la sculture, per via di levare (Freud, 1904, b. p. 13), ou encore: la thérapeutique par suggestion agit comme un procédé cosmétique qui "cherche à recouvrir et à masquer quelque chose de la vie psychique", alors que la thérapeutique analytique agit comme un procédé chirurgical qui "cherche à remonter jusqu' à la racine des symptômes" (Freud, 1916, p. 428).

D'autre part, ces considérations préludent en règle générale à un exposé des motifs qui ont fait qu'il a "très vite renoncé à la technique par suggestion, et, avec elle, à l'hypnose" (Freud, 1904, b, p. 15), en raison de ses effets peu durables et surtout en effet

de ce qu'elle ne permet pas de reconnaître la résistance. Et c'est quand il articule la résistance et le transfert que Freud revient le plus fréquemment sur la question de savoir dans quelle mesure les effets du traitement sont dûs à la suggestion: "assez souvent, le transfert suffit, à lui seul, à supprimer les symptômes morbides, mais cela temporairement et tant qu'il dure seulement. En pareil cas, le traitement ne peut être qualifié de psychanalyse, il ne s'agit plus que de suggestion. Le nom de psychanalyse ne s'applique qu'aux procédés où l'intensité du transfert est utilisée contre les résistances" (Freud, 1913, p. 103). Un court passage d'un article datant de 1923 nous fournit sur ce point un aperçu extrèment complet et subtil de l'état de la question à ce moment là pour Freud. Il s'agit de savoir dans quelle mesure les rêves produits par le patient pendant la cure sont influencés par la suggestion du médecin, ce qui revient à se demander "à quel point le patient, dans l'analyse, est accessible à la suggestion" (p. 557). Il va de soi, dit Freud, que le contenu manifeste du rêve et celles de ses pensées latentes qui correspondent à des formations de pensées préconscientes sont influençables au même titre que tout ce qui concerne la cure analytique en tant qu'elle fait partie de la vie éveillée, et que le rêve élabore les incitations qui en proviennent. Ici Freud reprend une idée déjà exprimée par ailleurs: les effets de suggestion ne sont pas plus évitables dans la cure que dans d'autres circonstances de la vie quotidienne. Mais, en ce qui concerne les motions de désir que le rêve contient, quelle peut être la part de l'influence du médecin dans les rêves qui viennent confirmer les constructions relatives aux expériences vécues dans l'enfance par le patient, que l'analyste élabore à partir des symtômes et associations de ce dernier et qu'il lui communique? Ces rêves de confirmation qui "tirent la jambe derrière l'analyse "sont-ils "fantasmés sur l'incitation du médecin, au lieu d'être amenés à la lumière à partir de l'inconscient du rêveur" (ibid. p. 558)? Qu'il s'agisse de véritables rêves de confirmation, ou, au contraire, de rêves de complaisance résultant de la suggestion, c'est las poursuite de la cure qui apporte la réponse. Mais ces rêves de confirmation, quand il n'y en a pas d'autres, proviennent de la résistance, ou, plus précisément, de patients chez qui le doute joue le rôle de résistance principale: en d'autres termes, le patient produit des rêves de confirmation pour pouvoir en douter et exprimer ainsi son ambivalence vis-à-vis du médecin. Donc

la suggestion est le produit inévitable du transfert, de même que c'est ce dernier, dans sa partie positive, qui permet le retour de l'oublié et du refoulé dans les rêves. D'où il s'ensuit que "Si quelqu'un prétend que la plupart des rêves utilisables dans l'analyse sont des rêves de complaisance et doivent leur origine à la suggestion, il n'y a rien à y redire du point de vue de la théorie analytique" (ibid. p. 560). Enfin, c'est le transfert qui permet à la compulsion de répétition de s'exprimer, donc de venir à bout du refoulement, mais pas avant que "le travail de la cure, venant à la rencontre, n'ait relaché le refoulement" (ibid. p. 560) (6). En résumé, le transfert, auquel sont inhérents les effets de suggestion, est le moteur de la cure et le travail analytique, à la différence de la thérapie par suggestion directe qui le laisse intact, doit porter sur le dévoilement du transfert lui-même.

Ainsi, les quelques indications qui précèdent visaient à montrer que, dans l'ensemble des considérations relatives à ces questions qui parcourent l'oeuvre de Freud de 1890 à 1937, celui se préoccupe non seulement d'indiquer, mais encore de théoriser la rupture opérée avec l'hypnose et la suggestion. Il ne s'agit pas de prétendre qu'après Freud il n'y a plus rien à dire des questions évoquées ici, ni qu'elles soient par avance réglées. Malgré le caractère limité, voire allusif, des exemples présentés ci-dessus, on peut pressentir qu'il y aurait lieu d'y revenir, mais c'est précisément parce que l'oeuvre de Freud se présente comme ouverte à la révision qu'elle nous y incite.

Notes

1. A titre d'illustration, il me semble intéressant de remarquer que le soustitre de l'ouvrage de Chertok (1984). Fait délibérément écho au titre de l'ouvrage de Roudinesco (1982).
2. Rappelons qu'à l'époque du magnétisme animal on désignait par ce terme la circulation du fluide entre magnétiseur et magnétisé.
3. Ici, Charcot répond aux critiques de Bernheim.
4. "Quiconque s'est tenu au courant de la littérature de l'hypnotisme sait que, quand on a pris connaissance de cinq ou six observations répondant aux différents degrés de l'état et de ses effets subséquents, ou les connaît toutes" (Wundt, 1893, p. 133).
5. La fonction du regard dans la clinique de CHARCOT a été soulignée par plusieurs auteurs, dont, notamment, Roudinesco, (1982).
6. Ici, Freud se cite lui-même (Freud, 1920, p. 59).

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HOW TO STUDY PSYCHOLOGY: ADVICE OF A JESUIT

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Summary

How one became a psychologist in earlier times has been a favorite topic in the history of psychology. The present paper presents a letter, which the noted Jesuit experimental psychologist Joseph Fröbes (1866-1947) wrote to his fellow Jesuit, Julius Bessmer (1864-1924) to guide his preparations for the study of experimental psychology at Leipzig University. The relationship between Neo-Thomistic philosophy and experimental psychology during the second half of the 19th century was discussed in some detail. The letter is an informative and interesting commentary on major scholars and important textbooks in (1) neuro-anatomy, (2) general psychology, (3) psychopathology, (4) forensic psychiatry, and (5) physiology at the turn of the century.

How one became a psychologist in earlier times has been a favorite topic in the history of German psychology. The classic collection of autobiographies, initiated by C. Murchison (1930-1936), is a storehouse of interesting and useful information about the student years of an older generation of psychologists, including William Stern (1871-1938), Carl Stumpf (1848-1936), Karl Marbe (1869-1953), Otto Klemm (1884-1939) among others. The later works of J. Pongratz, W. Traxel, and E.G. Wehner (1972) present autobiographies of more recent leaders in the field (i.e. Hans Biäsch, Charlotte Bühler, Heinrich Düker, Edwin Rausch, etc.). W. Bringmann and N. Bringmann (1983) have written at length about graduate study in experimental psychology in Wilhelm Wundt's Leipzig laboratory. P. Behrens (1984) has compared the formal Ph.D. requirements in psychology at Leipzig University under Wundt with those at John Hopkins University under G.S. Hall. M. Sokal (1980; 1981) has described in rich detail the "psychological education" of James McKeen Cattell (1860-1944) in Germany and England between 1880 and 1888.

Extensive information about the operation for the Institute for Psychology at Berlin University under Stumpf and Köhler was provided by M.G. Ash (1985). Even U. Geuter's dissertion (1982) about the development of psychology under National Socialism reports many original facts about the development of training programs and requirements for psychologists in Germany.

These publications focus primarily on objective descriptions of conditions in 19th and early 20th century German universities and the general impact on German and some American students of psychology. They provide little insight about personal elements and choices in the course of study. It is the purpose of the present paper to share a previously unpublished letter by Joseph Fröbes (1866-1947), the noted Jesuit, in which he gave advice to another Catholic clergyman on how to organize his studies of experimental psychology at Leipzig University under Wilhelm Wundt (1832-1920) during the summer semester of 1904 (Fröbes, 1903). Fröbes' letter is of special interest because it pays little attention to the idiosyncracies of Wundt and his colleagues at Leipzig, but, instead, tries to base its recommendations almost exclusively on the perceived academic strength and weaknesses of the eminent professors and scholars.

Neo-Thomism and Experimental Psychology

In their historical account of Catholics in Psychology, Misiak and Staudt (1954) noted that both psychology and Roman Catholic philosophy experienced major reforms at about the same time in the latter half of the 19th century.

> The year that witnessed the establishment of the first psychological laboratory, which marked the birth of the new science of psychology, is also well remembered and considered significant in the Catholic Church as the birth year of Neo-Thomism. On August 4, 1879 Pope Leo XIII issued the encyclica Aterni Patris, asking for the revival of the philosophy of St. Thomas Aquinas, as the remedy for the confusion and sterility of the philosophy of that period...it is in Aterni Patris that one should see the reason which prompted catholic philosophers to recognize the new experimental psychology. (p.34)

The encyclica encouraged Catholic scholars to take advantage of the discoveries of modern science and to advance with it rather than against it (Misiak and Staudt, 1954):

> In this message, Leo XIII directed attention to the progress that had been made in the natural sciences ...Moreover,...he reminded Catholic thinkers that faith is never opposed to truth and that therefore truth ought

> to be accepted wherever it is found. "Every wise thought and every useful discovery, wherever it may come from, should be gladly and gratefuly welcomed, said the pope. There could be no better invitation than this for the appraisal and acceptance of scientific psychology which had just emerged as these words were uttered.

The Belgian Roman Catholic clergyman, Desiré Felicien Francois Joseph Mercier (1851-1926), was the earliest and most eminent of the bridge-builders between Catholicsm and modern experimental psychology. His most important contribution to Thomistic philosophy was the organization of the Institut Supérieur de Philosophie ou l'École Saint Thomas d'Aquin at the University of Louvain i Belgium in 1889. The program of the new research center included philosophy, history, philology and natural science. A course in experimental psychology was first offered in 1891. Armand Thierry (1868-1929), a 24-year-old priest with advanced degrees in law, science, and medicine, was the permanent professor of psychology and director of the experimental laboratory. Thierry studied under Wundt in Leipzig, earning his Ph.D. with a dissertation On the Geometrical Optical Illusions (1896). Mercier studied briefly under Jean Martin Charcot (1825-1893) in Paris and published several volumes about the origin of modern psychology (1897). For him psychology was not a dead field of book knowledge but a "living science", which included "the results of cytology, histology, embryology, animal psychology, child psychology and abnormal psychology" (Misiak & Staudt, 1954, p.44). Mercier was apparently highly impressed with Wilhelm Wundt. According to Misiak and Staudt (1954), Mercier called Wundt "...if not the creator, yet surely the most vigorous promotor of psychophysiology..." and elsewhere he identified him "...as the learned initiator of experimental psychology..." (p.48). Although he opposed Wundt's agnosticism and his psychological "voluntarism", he believed him to be an ally of Thomistic philosophy, because Wundt maintained that psychology was a part of philosophy. Other Roman Catholic priests, who followed Mercier's general orientation, were the Englishman of Irish descent, Michael Maher (1860-1918), the Frenchman, Emile Peillaube (1864-1934), and the American, Edward Aloysius Pace (1861-1938).

Relations between Catholic theologians and philosophers and representatives of the new experimental psychology were less cordial (Misiak & Staudt, 1954) in Germany. Johannes Peter Müller (1801-1858), the noted pioneer of sensory physiology and psychology, experienced no serious scruples in integrating his traditional

Catholic faith with his experimental methodology. The philosopher Franz Brentano (1838-1917) was initially ordained as a Dominican priest in 1864 prior to an appointment as professor of philosophy at Würzburg. He left the church in 1873, because he no longer felt able to accept "contradictions of reason and supernatural revelation".

Carl Stumpf (1848-1936), who had been a student and close friend of Brentano's at Würzburg, left the seminary before ordination for personal reasons. His autobiography suggests that he gave up the practice of catholicism, but did not formally convert.

It must not be forgotten that the political situation for German Catholics was most problematic during the Chancellorship of Otto von Bismarck (1815-1898), which covered the years from 1871-1890. The "Kulturkampf", or struggle between the Central Government of Germany and the church, greatly limited the influence of the church in the educational field (see Schmidt-Volkman, 1962). When the Roman Catholics claimed the right to choose and license their own teachers, Bismarck attacked the teaching orders and insisted that the state had the right to train and license all teachers and priests. The anti-Jesuit legislation of 1872 abolished the residences of the Order in Germany and imposed significant residential and occupational restrictions on its members. Whereas most restrictions of the Kulturkampf were abolished by 1879, the last anti-Jesuit bills were not repelled until 1917.

Joseph Fröbes (1866-1947)

Joseph Fröbes, the author of our letter, was born in August 26, 1866, in the small village of Betzdorf in the Rhine Province of the Kingdom of Prussia (1936):

> ...Through the exigencies of his vocation as an engineer on the Cologne-Fiessen Railroad, my father was forced to move to my birthplace, and shortly thereafter to Giessen on the Lahn. My earliest memories take me back to that town - to the narrow strip of the lofty railroad embankment ad the Lahn, the marvels of the neighboring mill, the distant but oft-visited garden, and, beyond, the church. Somewhat more definitely I remember the public-school, which I entered at the age of five...(p.121)

Although the parents were not rich, they tried to provide their son with a good catholc education:

> They moved to a little town in the neighborhood of Darmstadt, which boasted a grammar school managed by the Catholic clergy. I entered this seminary at the age of eight and one half years; but the following year this institution was suppressed because of the current

> religious antagonism (Kultur Kampf). Finally, as a reward of extensive inquiry, which lasted the entire period of grammar school years, I was placed in a Jesuit boarding school near Feldkirch in Austria. Here I continued my studies. (p.121)

Fröbes obviously liked the new school and its regimen:

> The five school years that followed (1877-1882) now appear to me as the most beautiful memory of my life. The demands on my powers of learning were by no means slight...Furthermore, the hours of recreation with the compulsory English games, which were at that time almost unknown in Germany, and the regular tramps in the mountains taxed my strength heavily. (p.122)

His favorite subjects were English, which was taught by a native speaker, and mathematics. Fröbes completed the final year of high school in 1882 at the age of only 16. Shortly afterwards, he became a novice in the Jesuit Order and spent two years in religious training. Fröbes also completed an intensive course in Latin and Greek which later enabled him to write several psychological works in Latin (1927, 1933, 1937). Next, came seven years of advanced courses in philosophy and specialized theological studies. As a hobby, Fröbes improved his skills in Italian and Spanish! He taught mathematics, physics, and chemistery at his alma mater Feldkirch from 1889 to 1894. Fröbes immersed himself in theology the next four years at The English Jesuit College, Ditton Hall, and at Valkenburg College in Holland. After 17 years of preparation in the Jesuit order he was given the assignment of teaching philosophy at Valkenburg college. Fröbes did not take his job lightly as we learn from his autobiography (1936):

> On an initial three-year appointment I taught all systematic subjects in the following order: logic, critique of reason, ontology, cosmology and theodice. (p.127)

Fröbes reports that he experienced the most difficulty in reconciling traditional Catholic philosophy with the new field of experimental psychology:

> Most of all did psychology seem to require a thorough revision. Here was a newly discovered science which no one as yet found time to combine with the old philosophy.

To overcome his deficiencies, Fröbes spent three semesters at Göttingen University under Georg Elias Müller (1850-1934), where two of his brothers were also studying (1936):

> Georg Elias Müller, the first and most profound critic of Fechner's Psychophysics...was at that time at the height of his influence...Especially, he arranged my program of lectures to give me the greatest help in my psychological studies. (p.128)

Fröbes took the regular laboratory course for medical students, Müller's required introductory lectures and experimental courses in psychophysics, reaction-time and color perception. He also reports having learned much as an experimental subject! Müller assigned him a thesis topic in psychophysics and painstakingly supervised the entire project, even the writing of the manuscript. The project was published in 1904 in the Zeitschrift für Psychologie.

Fröbes made excellent use of his brief time at Göttingen. He enrolled in a variety of courses in applied psychology, psychiatry, history of modern philosophy and physical chemistry. His teachers included such luminaries as the Phenomenologist, Edmund Husserl (1858-1938) and the future Nobel Prize winner, Walter Hermann Nernst (1864-1941).

After finishing the summer semester of 1904 in Leipzig, Fröbes resumed his regular appointment at Valkenburg, where he remained until his retirement at the age of 70 in 1934. A diligent researcher, he established a psychological laboratory and found time to write several books and numerous scientific articles. Fröbes was honored with a visiting appointment in experimental psychology at the major Jesuit University - the Gregoriana - in Rome between 1924 and 1925. His recognition as a scholar came with the publication of his Lehrbuch der experimentellen Psychologie (1917). This work has appeared in three revised editions of about 1,300 pages each (Fröbes, 1920, 1922, 1923, 1929). The handbook was again updated in 1935 with a special supplement by its author. According to Richard Pauli (1886-1951), the work by Fröbes was regarded in 1930 as "the best presentation of currently available knowledge." (Pauli, 1930, p.23).

Advice of a Jesuit

Fröbes letter in 1903 contains a description of his own preparations for the next year's summer semester. He and the unnamed recipient of the letter were planning to study at Leipzig (today **Karl-Marx-Universität**, WGB). The four-page handwritten letter is a mixture of Latin and German writings.

Fröbes must have written the letter shortly before the beginning of the Christmas vacations of 1903, while he was still a student at Göttingen and resided at "8 Tower Street" with his two brothers. The respectful salutation "Dear Reverend Father" indicates that he is

writing to another priest who may well have been older than he. The recipient may even have been one of his superiors in the Jesuit Order. We also learn from the letter that Fröbes has corresponded with his fellow priest as early as December 1. He is pleased that his respected colleague is planning to study with him in Wundt's Leipzig Laboratory:

December 13, 1903
Göttingen, 8 Tower Street

Dear Reverend Father:

> Since your letter of December 1 contains a number of questions, I am more or less compelled to answer more quickly than usual. First, I am happy and excited that your Reverence is looking forward to going to Leipzig. I certainly don't plan to overburden you as your "mentor". Apart from psychophysics, we are both at about the same level of training. However, we shall be able to study together next semester, and that is always easier and better than [working] alone. We can consult each other and try to understand Wundt or, rather, refute him. One remembers more from such personal interactions than from the dry and lonely study of books. Be prepared that our recreation will be somewhat boring, as far as I am concerned. I can't make up my mind whether walks outside the city [of Leipzig, WGB] will offer much more than "countryside itself" (as Kant might say).

The next section of the letter refers to an article which the addressee of the letter had sent to Fröbes in manuscript form:

> Thank you very much for sending me your article. It appears that we have a comparable knowledge of the subject matter. I worked through Edinger some time ago and read the short sections about anatomy in Wundt and Ebbinghaus. To give a competent judgement, I would have to compare each of [your] claims with Edinger, but that would take me too much time. Therefore, I have restricted myself to read your manuscript carefully in the expectation that any totally abnormal claims would attract my attention. Physiology and even more anatomy are only minor subjects for me, and I must be satisfied with a summary knowledge of all that is needed for empirial psychology.

In 1903 the study of psychology had not yet become an independent field of study but remained a subfield of philosophy. Consequently, medical specialties, like physiology and anatomy, were regarded as an important basis of psychology, although they ranked only as minor subjects. Fröbes reports that he acquired his anatomical knowledge

primarily through his reading of the appropriate sections in the writings of Wundt, Hermann Ebbinghaus (1850-1909) and Ludwig Edinger (1855-1918). The corresponding sections can be found in the first volume of Wundt's Physiological Psychology (1902), the first volume of Ebbinghaus' Principles of Psychology (1902), and in Edinger's Lectures on the Structure of Central Organs in Human and Animals (1900).

Fröbes next approaches the necessary preparations for Leipzig semester:

> Let me now turn to the question of how you could prepare yourself for Leipzig. My general answer is that you know more than enough about the introduction [to psychology]. On the other hand, the more one has read through the better it is, because one will find it easier to understand the lectures and pay attention to what matters most. The actual work has to be accomplished by individual study.

Fröbes provides his colleague with detailed recommandations for his preparation of his studies in neuro-anatomy, psychology, psychopathology, and physiology.

> Specifically, for the anatomy of the brain, Edinger is very good. More than it contains, is not needed for applied work. Unfortunately, the anatomy of the sense organs is missing. I used the sixth edition published in 1900. The main difference in comparison to the older editions is that the latest edition includes many lectures about "comparative anatomy". I don't remember how far the different editions are separated in time. However, in this subject the reading of a textbook is only part of one's preparation. One also needs to see the subject matter itself or at least models [of the brain, WGB]. I hope you will have an opportunity to do this.

The physician and neurologist, Ludwig Edinger, received his M.D. at Strassburg University under W. von Waldeyer (1836-1921), and, in 1881, qualified as a university teacher at Giessen. In 1883 he opened a private practice as a specialist for the diseases of the nervous system in Frankfurt. Edinger, who had continued his neurological research together with clinical work, was given a "personal" professorship for Neurology in 1914 at the newly established University of Frankfurt. His most important accomplishment was the organization of a special neurological institute. His Lectures contain the best presentation of his views. Fröbes still believed that lecture material had to be supplemented by anatomical preparations and a thorough understanding of brain models. Less than twenty years later, Giese (1922) stated in his Occupational Guide for College Students almost matter of factly that:

> A lecture in the anatomy is always followed by work in the dissecting room. Psychologists should be involved in both because they will gain little understanding from the lecture alone. Only personal work on corpses will lead to full understanding. (p.34)

Fröbes strongly endorsed the textbook of Ebbinghaus as an introduction to the field of psychology:

> As far as psychology is concerned, I prefer Ebbinghaus. According to Professor Müller, his is the best book in this subject matter. I am certain that he provides the best introduction because of his clear and pleasant style (his occasional materialistic-monistic fantasies can easily be overlooked). His refutation [Heimleuchtung] of extreme empiricism (i.e. space perception, Wundt) is very good. Even the coverage of factual content is better than Wundt's. I shall save the latter for Leipzig itself. I expect that he will be easier to understand when one lives in the atmosphere of his views.

In 1903, Hermann Ebbinghaus held a professorship at Breslau University. He had just published the first volume of his Principles of Psychology (1902). Fröbes reported in his autobiography that he attended Wundt's introductory lectures in psychology at Leipzig (1936). Wundt impressed him very much, because "...despite his 70 years, he handled his material with astonishing freshness of outlook" (p. 129). More importantly, "his lectures were accompanied by demonstrations, a thing that was not possible in Göttingen at that time" (1936, p.129).

Fröbes also enjoyed courses in psychiatry and abnormal psychology and was not at all biased by his theological training in his choice of subject matter:

> In the field of psychopathology I only know Störring, whom I like well because of his habit of trying to explain phenomena psychologically. I suspect that Krafft-Ebing will contain more of the facts which will be more useful than theories. I am currently attending an interesting lecture course by Cramer on "Forensic Psychiatry" with demonstrations of 3-4 patients each time. One gets quite a different idea of an illness than one had previously. Undoubtedly, there must be something like that in Leipzig. At least Father Kobylecky told me that he enrolled in a similar course there.

The philospher, psychologist and psychiatrist, Gustav Störring (1860-1946), had first studied theology and philosophy before becoming a physician. After serving for a number of years as an assistant at Leipzig University Hospital, he was approved as an instructor of philosophy at Leipzig in 1896. He next directed a private psychiatric sanatory for five years before accepting professorships at Zürich,

Strassburg and Bonn. Störring sought to integrate psychiatric and psychopathological information with the subject matter of experimental psychology. In particular, he was interested in utilizing his knowledge of abnormal behavior for an understanding of normal behavior.

The psychiatrist Richard von Krafft-Ebing (1840-1903), whom Fröbes also recommended, is best known for his extensive scholarly and popular writing about sexual deviations. His Textbook of Psychiatry (Krafft-Ebing, 1903) contained a large number of fascinating case histories, since he conceptualized psychiatry as a descriptive science.

Fröbes enthusiasm for the lectures of August Cramer (1860-1912) a professor of psychiatry and neurology at Göttingen, developed from the attractiveness of the subject matter of the field of forensic psychiatry and from Fröbes own didactive preference for teaching methods that combined theoretical materials with examples and practical illustrations.

"Father Kobylecki", who had told Fröbes about similar courses in psychiatry at Leipzig University, was the Polish philosopher Stanislaw Kobylecky (1864-1939), who had joined the Jesuits in 1888. He studied psychology in Leipzig, Freiburg, and Göttingen under Wundt, Edwald Hering (1836-1918), Johannes von Kries (1853-1928) and G.E. Müller. In 1905 he earned the Ph.D. under Wundt with a dissertation on the perception of "sudden changes in pressure". Kobylecki left the Jesuit Order and continued his studies in America in 1912. For him psychology was an experimental science and a natural science which could not be fully understood without recourse to aprioristic postulates.

Fröbes also included the names and works of two additional scholars should the recipient of his letter have more time to prepare than anticipated:

> If you have more time to look at other authors, apart from the three mentioned by you, I could recommend physiology, the territory between psychology and anatomy; Hermann and Tigerstedt are two names which immediately come to mind.

The physiologist, Ludimar Hermann (1838-1914), qualified to teach at a university in 1865 and accepted his first appointment in Zürich. His next appointment was at Königsberg in 1884, where he had impressive research facilities. As one of the leading physiologists of his time, he was granted honorary degrees from Oxford, Dublin, and

Königsberg. His major field of interest was nerve physiology. He later carried out important research on the physiology of voice and speech and in the fields of optics and acoustics. His Textbook of Physiology (1900) had been published in twelve editions when Fröbes recommended it to his colleague.

The Swedish physiologist, Robert Tigerstedt (1853-1923), completed his initial work in philosophy (1873) before earning his medical degree in 1881. He taught at the University of Stockholm and later at Helsinki. He specialized in the physiology of circulation, the physiology of muscles and nerves, the physiology of metabolism and the physiology of sensation. His famous textbook, which was translated into several languages, was published in 1903 in a second edition.

In the closing section of his letter, Fröbes briefly comments on the living and study conditions in Leipzig:

> At the present time, I am not sure what lectures will be offered in Leipzig. Apart from lectures, I shall try to work in Wundt's institute as an experimental subject. I shall write Wundt's assistant around the first of year. I am not sure whether you want to be involved too. Perhaps I can take a physiological seminar under Hering, if it fits in my schedule. (One has books at home, only seminars at university). Our apartment will be in Reudnitz - 25 minutes from the University (there is a street car connection). We are to stay with nuns, and this should be convenient for celebrating mass. I shall apply for admission [to Leipzig University, WGB] soon, but must first ask my superior for permission. Merry Christmas and a Happy New Year!
>
> Cordial Greetings to Father Springer and Kälin.
>
> Yours truly, Jos. Fröbes SJ

Fröbes' autobiography informs us that he did indeed serve as an experimental subject in Wundt's laboratory. The project dealt with "tactual illusions", and the experimenter was non other than Charles Spearman (1863-1945), who was completing his dissertation research at Leipzig in 1904.

Discussion

The recipient of this very informative and interesting letter by Joseph Fröbes is unknown. There are, however, a number of clues that we shall follow up. The salutation at the top of the letter tells us that the recipient is a Catholic clergyman, who obviously was

interested in studying under Wundt during the summer semester of 1904. According to Misiak and Staudt (1954, p.191), Father Thomas V. Moore (1877-1969) went to Leipzig in the spring of 1904 to study under Wundt "where he hoped to discover how experimentation could be done in the field of thought processes." He was an American by birth and a member of the Paulist Order which he had joined in 1896. He had received his Ph.D. under Pace at Catholic University in Washington, D.C. in 1903. A second possibility is Father Julius Bessmer (1864-1924), another Jesuit, who had been one of Fröbes theology teachers at Ditton Hall in England between 1894 and 1898. The solution of our problem becomes slightly more complicated, when we recall the brief English salutation "Yours truly" at the end of the letter, which, of course, would be suitable for the American Moore, as well as Bessmer, the native of Switzerland, who possessed good language skills in English. The actual content of the Fröbes letter provides an additional clue in the manuscript, which the recipient had sent to him for critical comments. This article appears to have been the manuscript of a paper on "Gehirn und Seele" by Bessmer which appeared in 1904 the Neo-Thomist publication Stimmen aus Maria Laach (Voices from Maria Laach). The 21-page article contains a number of anatomical illustrations and mentions scientists like Edinger, Ebbinghaus, and Krafft-Ebing, whom Fröbes had mentioned in the presented letter. Finally, the last but most useful clue is that Fröbes asked his colleague to communicate his Christmas and New Year's wishes to another priest, Father Karl Kälin (1870-1950), whom Bessmer may very well have known, since they were both natives of Switzerland.

Additional information was derived from a record of student addresses for Leipzig University, which had been prepared by the Archive of Karl-Marx University. Here the following addresses were listed for Moore, Bessmer, and Fröbes during the summer semester of 1904:

Moore, Thomas
Immatriculation: May 4, 1904
Address: Erdmannstrasse 13 c/o Kenley.

Bessmer, Julius
Immatriculation: April 22, 1904
Address: 5 Rudolfstrasse c/o Jost

Fröbes, Joseph

Immatriculation: April 22, 1904

Address: 5 Rudolfstrasse c/o Jost

Fröbes and Bessmer, who belonged to the same religious order, functioned like a team. They were enrolled on the same day and also sublet the same apartment. Father Moore belonged to a different religious organization. He registered at the university about two weeks before Fröbes and Bessmer and lived by himself.

In conclusion, we believe that Father Julius Bessmer was the recipient of the Fröbes letter. They knew each other well, since Bessmer had been one of Fröbes' theology teachers at Ditton Hall College in England (1895-1898). They shared a common interest in the complex relationship between psychological and physiological variables, both knew English well, had common friends and acquaintances, registered the same day at Leipzig University and shared housing in Leipzig during the summer semester of 1904.

There is another question. Why did Fröbes go to Leipzig at all? He was obviously highly satisfied with the training he was receiving at Göttingen University under Müller. Fröbes mentions in his autobiography that he became interested in studying psychology by reading Wundt (1936):

> A casual examination of Wundt's textbook and periodical showed conclusively that there was a new realm which could not be conquered without technical study; with this in mind I sought and received permission to interrupt my teaching activities and to seek sources for my research at the University. (p.127)

In fact, Fröbes initially planned to study in Leipzig under Wundt but personal reasons changed his mind:

> Up to this time I had thought only of Wundt, to whose laboratory students gravitated from all corners of the earth. But as two of my brothers were then studying at Göttingen, I learned that there too the new science was well represented. The convenience of living with them there influenced me to cast my lot temporarily for Göttingen, and what at that time seemed to be a mere measure of necessity later proved to be a kind dispensation of Fate. (p.127)

Fröbes spent his last semester of academic study at Leipzig to familiarize himself with Wundt's system by attending his famous introductory lectures and he was, of course, not disappointed:

> The fourth semester of my two years of leave I used for a pilgrimage to Leipzig, the birthplace of the new psychology. By far the most important offering in this University was Wundt's major lectures in psychology. Despite his

> 70 years, he handled his material with astonishing freshness of outlook; his lectures were accompanied by demonstrations, a thing that was not possible in Göttingen at that time. (p.129)

Müller appears to have been an outstanding teacher of individuals and small groups of highly motivated students, who wanted to become researchers in the field, yet he could not compete with the showmanship of Wundt, who attracted hundreds of students from all faculties and from many foreign countries to his formal lectures and demonstrations every semester.

It has been a pleasant revelation that one could study at Göttingen under Müller and then successfully continue one's studies at the rival laboratory of Wundt without suffering undue consequences. The common devotion to experimental psychology superceded any personal competition and idiosyncracies.

Notes

1. The Fröbes letter (see Figure 1) was discovered during preliminary dissertation researches in the Munich archive of the German branch of the Jesuit Order (Provinzialarchiv, Oberdeutsche Provinz, S.J., Seestrasse 14, 8000 München, West Germany).
2. Student Directory, Karl-Marx-University Archive, ˜01 Leipzig, Germany Democratic Republic.

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Göttingen, Turmstraße 6. 13/12. 03

Lieber hochwürdiger Pater!

Da Ihr lb. Brief vom 1/12 allerlei Fragen enthält, bin ich wohl oder übel zu einer schnelleren Antwort genöthigt, als ich sonst wohl gelei[illegible] hätte. Zunächst freut es mich, worauf ich einigermaßen gespannt war, daß Ew. Hochw. selbst mit Freude nach Leipzig gehen. Von einer „Mentorärei" ist übrigens keine Rede. Das einzige Gebiet der Psychophysik abgerechnet, stehen wir so ziemlich auf gleichem Niveau. Indessen werden wir im nächsten Semester zusammen studieren können, und da geht es immer leichter & besser, als allein. Wir können uns dann berathen, Wundt zu verstehen und zu widerlegen suchen; und von solchen mündlichen Verhandlungen bleibt doch schließlich mehr im Gedächtnis, als von dem trockeneinsamen Bücherstudium. Auf einige Langweile in der Rekreation müssen Sie sich allerdings gefaßt machen, wenn stens von mir aus. Und ob die Spaziergänge außer der Stadt viel mehr als „Gegend um sich" (um mit Kant zu reden) bieten werden, kann ich nicht mit Bestimmtheit entscheiden.

Figure 1. Title page of J. Fröbe's letter to J. Bessmer (12-13-1903).[1]

JEAN PIAGET AND PSYCHOANALYSIS.
A HISTORICAL AND BIOGRAPHICAL NOTE (UP TO THE 1930's).

Fernando Vidal
University of Geneva.

From Autobiography to Biography

In an interview done in the 1970s, Jean Piaget declares that he had been psychoanalyzed by "one of Freud's students," in Geneva towards 1921. "It was marvelous," he says "to discover all one's complexes." Nevertheless, after eight months, the psychoanalyst "felt it wasn't worth wasting an hour a day on a man who wouldn't accept the theory," and discontinued the analysis.[1] Piaget did not name his analyst, but several indications (in particular her being eastern European, and Piaget's comment that she had been sent to Geneva "to disseminate the doctrine") pointed to her identity. It was finally confirmed that she had been Sabina Spielrein, a former patient, student and lover of Carl Gustav Jung.[2]

In his autobiography, Piaget reveals that his mother's "rather neurotic temperament" and "poor mental health" considerably troubled family life; and he adds:

> it was this disturbing factor which at the beginning of my studies in psychology made me intensely interested in questions of psychoanalysis and pathological psychology. Though this interest helped me to achieve independence and to widen my cultural background, I have never since felt any desire to involve myself deeper in that particular direction, always much preferring the study of normalcy and of the workings of the intellect to that of the tricks of the uncounscious.[3]

For two reasons, neither this significant passage, nor the fact that Piaget went through analysis with Spielrein seem to have been exploited. The first one may be a praiseworthy reluctance to engage in psychobiographical speculation. The second reason, however, may be that Piaget's narrative itself seems to diminish the importance of psychoanalysis, and Piagetian "historiography" is still very largely based on his autobiography.[4]

In his autobiography, Piaget indicates that he discovered psychoanalysis in Zurich (where he spent a semester in 1918-19). There, he read Freud and the journal Imago, and occasionally attended lectures by Jung and Oskar Pfister, a Protestant minister very

interested in the applications of psychoanalysis to education and "Seelsorge." At the same time, he followed the courses of the psychiatrist Eugen Bleuler, who was well known for his elaboration of the concept of autism and his description of schizophrenia. At this point in his autobiographical narrative, Piaget observes that the initiation to psychoanalysis and psychiatry "made me sense the dangers of solitary meditation; I decided then to forget my system lest I should fall a victim of 'autism'."[5]

Piaget's remark and the pathological label function both as an apologetic tool and as a defense against non-rational enthusiasms; they send us, however, directly to the context in which Piaget actually came in contact with psychoanalytic theory. By "my system," Piaget refers to the ideas elaborated in his philosophical essay and confession Recherche (published in 1918), where he had already described metaphysics and mysticism as legitimate ways of knowledge, and had expressed this conviction in his poem-essay The Mission of the Idea, published in December 1915. Mission had been composed at a time when Piaget was an active participant in the Swiss Christian Students Association;[7] and his initial contact with psychoanalysis took place precisely in the context of that association.

Psychoanalysis and Religion

Piaget hints at the circumstances of his first contact with psychoanalysis in a 1945 homage to Jung, where he recalls a 1916 talk given by Théodore Flournoy, supposedly on Freud and Jung.[8] Flournoy was not only professor of experimental psychology at the University of Geneva and founder of the journal Archives de Psychologie, but also a pioneer of the scientific exploration of the unconscious and a main figure in the introduction of psychoanalysis in the French-speaking world.[9] As a pragmatist and a religious man in the tradition of liberal Protestantism, Flournoy shared with his friend William James the idea that "whatever it may be on its 'farther' side, the 'more' with which in religious experience we feel ourselves connected is on its 'hither' side the subconscious continuation of our uncounscious life."[10] Freud, on the contrary, did not consider the idea of a "farther side;" for him, religious ideas were only illusory fulfilments of the oldest, strongest and most urgent wishes of mankind.[11]

In 1913, Flournoy had helped Jung break with Freud. His talk, entitled in fact "Religion and Psychoanalysis" and given at the

annual conference of the Swiss Christian Students Association, highlights the role played by religious questions not only in the rupture Jung-Freud, but also in Piaget's first encounter with psychoanalysis. Flournoy did not hesitate to declare himself favorable to the Zurich psychoanalytic school represented by Jung. In his 1915 work Une mystique moderne, he had observed that "the general spirit of the Zurich school must be much more attractive to the religious people of our land than that of the Vienna school, since it attaches more importance to the points of contact and the relations of mutual support that exist between Psychoanalysis and Religion (Christianity in particular)."[12]

In the notes on which he based his 1916 talk, he opposed the Freudians' "Judaic Notion of Religion" to the Zurich school's "Christian Ideal;" and he asserted: "we have to get used to the equivalence of a double vocabulary: mystical and psychoanalytical."[13] Elsewhere, he pointed out "the contrast between the purely psychobiological and narrowly positivistic tendency of the Viennese school, and the Zurich school. Without giving up scientific rigor, the latter shows a much greater open-mindedness, a more delicate sensitivity, a much more lively concern for moral, religious, social, pedagogical questions."[14] Finally, according to a summary of the Christian students' conference, Flournoy "explained the phenomenon of the religious feeling from a physiological and psychological point of view: the religious feeling would be the supreme blooming of human personality, the ideal sublimation of our diverse instinctive pressures."[15]

Flournoy's approach is representative of the fact that, in Switzerland, psychoanalysis was first welcomed by liberal Protestant intellectuals, many of them pedagogues and pastors, for whom religion had to be discussed in other terms than Freud's rationalistic materialism.[16] Nevertheless, there is no evidence that the young Piaget's apprehension of psychoanalysis was ever primarily oriented by a concern for religious questions, even if already his earliest statements about it (from Recherche) bear the mark of the Zurich school (emphasis on the notion of "complex", critique of Freud's alleged "pansexualism") and, in particular, of his search for a scientific morality devoid of metaphysics. In fact, it would seem that Piaget did not actually study and explore seriously the psychoanalytic theory until his stay in Zurich.

Piaget in Zurich

Little is yet known of Piaget's stay in Zurich, even if something can be now added to the sketchy autobiographical informations usually reproduced. According to administrative documents of the University of Zurich, Piaget was enrolled in the department called "Philosophy I" during the 1918-19 winter semester (October 1 - March 1).[17] In connection with experimental psychology, Piaget mentions Arthur Wreschner and Gotthold-Friedrich Lipps. These two professors also taught topics in philosophy, pedagogy and "Entwicklung des geistigen Lebens." In a letter to a zoologist written in November 1918, Piaget claimed to be in Zurich "to study the statistical and biometrical methods applicable to mollusks."[18] It is true that, in a 1920 article, he calculated what is today called a rank-order correlation coefficient (a common statistics in psychology) between the vertical distribution and the variation of certain mollusks.[19] It is also true that his claim was consistent with his search for a biology different both from the natural history he had practised earlier, and from the Mendelian avantgarde.[20] Nevertheless, in the light of Piaget's later ways of rendering his "system" scientific, his recollection that the experiments of Lipps and Wreschner seemed to have "little bearing on fundamental problems" appears to be equally accurate.[21]

In an October 1918 letter to his teacher and mentor, the philosopher Arnold Reymond, Piaget wrote: "My impressions of Zurich are excellent. I am going to start working with Lipps. Up to now, I have mainly done psychiatry at the Burghölzli."[22] The Burghölzli included the university's psychiatric clinic. Its director, Eugen Bleuler (mentioned in the previous section), had been placed by Flournoy among the Zurich psychoanalysts who followd a "Christian ideal." Given the date of Piaget's letter, and assuming that he actually arrived in Zurich at the beginning of the semester, he could not have, at that time, followed more than a few courses in psychiatry.

Bleuler taught a psychiatrical clinic, as well as general and specialized topics in psychiatry (which the course catalog does not specify). The policlinic included hypnosis and other "exercises." Other courses were given on diagnostics, child and adolescent psychopathology, psychotherapy; and there was a "Repetitorium über Nervenkrankheiten, mit Demonstrationen." I do not know which courses Piaget followed; psychoanalysis, however, surely occupied a prominent place

in his curriculum. Zurich was the home of the controversial Jung, but it was also the home of Pfister, zealously devoted to the Freudian cause in spite of some "Swiss" disagreements with the founding father; it also housed the Burghölzli, first institutional setting of psychoanalysis. A few years earlier, Freud had pointed out that most of his followers and co-workers arrived to him by way of Zurich. This city as a "focus of infection" of a "psychical epidemic" from which the young Piaget did not escape.[22bis]

Piaget as "Swiss Connection"

Probably in the Fall of 1919, Piaget arrived in Paris. Pierre Bovet - a friend of his family who was director of the Rousseau Institute of Geneva, a promotor of the "école active", and a liberal Protestant involved with moral education and with the possibility of applying a Christian-oriented psychoanalysis to education - had given him a recommendation for Théodore Simon, co-worker of the late Alfred Binet.[23] At some point, Simon told Piaget: "Psychoanalysis is little known in France. It is only studied by our psychiatrists. A sketch of psychoanalytic trends in pedagogy would be interesting." The result was the December 1919 talk "Psychoanalysis in its relation to child psychology", at the opening of which Piaget recalled Simon's request and commented: "As a good Swiss, which I am proud to be, I could hardly believe him".[24]

Piaget showed a good knowledge of the different trends of the psychoanalytic movement, and a keen awareness of the need for methodological tact. He followed the "Swiss critique of Freud's 'pansexualism', and described the Zurich school as the trend that worked at reconciling Freud's ideas with Adler's way of breaking the vicious circle of censorhsip and civilisation. More generally, he emphasized the pertinence of psychoanalysis for pedagogy and for moral education. At the same time, however, he elaborated a critique aimed at removing the apparently rigid boundary between psychoanalysis and the psychology of intelligence. He observed that the conscious and the unconscious are everywhere entangled, and argued that they are continuous with each other. The unconscious mechanisms of the mind are the first stages of its conscious activity; there is only a difference of degree between the extreme forms of autistic and rational thought, and both have an uncounscious component (their mechanisms) and a conscious one (their product). Finally, Piaget

called for a study of the correlation between the development of the unconscious and the development of intelligence.

The only known reaction to Piaget's talk came from the psychoanalytic camp itself. Oskar Pfister, who knew Piaget personally, published a very favorable review in the Journal Imago. He criticized Piaget for identifying the uncounscious with all non-conscious psychic events, and for believing that Freud only recognized psychic energy of a sexual origin. Nevertheless, both in retrospect and for the psychoanalytic movement in 1920, the most significant part of Pfister's review was the conclusion:

> Except for such details that do not hurt the work as a whole, Piaget deserves compliments for his conscientious and meticulous presentation, extremely valuable for its connection with most careful personal researches and with a great originality of thought. Moreover, the fact that the speaker dared present the new depth psychology to an illustrious but unprepared and sceptical audience witnesses to the moral seriousness and personal courage of the young scholar [Gelehrter], from whom the psychoanalytic movement can certainly expect important contributions.[25]

Thus, the young Piaget was in fact among those who pioneered the introduction of psychoanalysis in France. A later offical history could consider him as an agent of the dangerous "Swiss sieve", but at the time of his talk, he could only appear as a realization of Freud's own strategies and predictions: psychoanalysis would enter France through the door of Frenchspeaking Switzerland.[26]

Psychoanalysis and Piaget's Theory

While in Paris, Piaget started to develop his own research domain, at the crossroads of logic, epistemology, sociology, and the genetic psychology of intelligence. His first articles attracted the attention of Edouard Claparède, founder of the Rousseau Institute, who invited him to join the staff. Piaget arrived at the Institute in 1921 - the same year as Sabina Spielrein. If Spielrein's mission really was to convert Geneva to psychoanalysis, then her task was no longer of great use. Indeed, for example, the Rousseau Institute had already offered a course on the significance of the findings of psychoanalysis for child psychology and moral education. Claparède's laboratory at the Institute periodically housed the "Psychoanalytic Group" of Geneva; and no sooner had Spielrein arrived, than Claparède ("toujours galant", as Bovet noted) fixed up for her the small consulting room where several members of the Institute volunteered for analysis.[27]

In the 1920s and 30s, Piaget's discussions of psychoanalytic conceptions concerned the developmental parallelism between affect and intelligence, and always formed a sort of counterpoint with more "piagetian" ideas. Piaget, however, did not elaborate on the relations between psychoanalysis on the one hand, and morality and religion on the other. Freud had pointed out that the "theological prehistory" of the Swiss contributed to explain their attitude towards psychoanalysis, and in particular their refusal to "sexualize" ethics and religion.[28] Such an observation might apply to Piaget, even if he did not approach morals or religion from a psychodynamic point of view. Perhaps his silence on the relationship between psychoanalysis and religion should be interpreted as an instance of the "Swiss" refusal, but the fact is that his approach was not merely dissident; it was radically different from the psychoanalytic way of thinking. At the time, Piaget was elaborating a psychological axiology based on a developmental hierarchy of values that included religious attitudes. On the one hand, he stressed the viewpoints he characterized as "empirical Kantism" and "critical rationalism", and focused on the possibility of finding a common ground between logic and genetic psychology.[29] On the other hand, his project was not to describe the psychodynamic mechanisms whereby morality grows and works, but to demonstrate the parallelism between the progress of moral judgment in the individual and in the history of European thought, and between moral and intellectual individual development.[30]

Naturally, the domains in which Piaget was working overlapped; thus, for example, "egocentric" thought would appear at the bottom of the hierarchy in the evolution of both intelligence and moral judgment. At least in his writings, Piaget appears interested in psychoanalysis mainly for its relevance to the study of "logic" (i.e., "the sum of the habits which the mind adopts in the general conduct of its operations").[31] This fact is illustrated by the talk he gave at one of the first meetings he attended as a psychologist: the seventh international congress of psychoanalysis (Berlin, 1922). There, Piaget explored the analogy between the thought of the child, and what the psychoanalysts called "symbolic", "autistic" or "undirected" thought. To explain the analogy, Piaget drew more on Pierre Janet (whose courses he had followed in Paris) than on Freud. Moreover, he refused to speak of "questions of origin, which still remain

obscure", and declared himself faithful "to the spirit of the numerous psychoanalysts who have a sense for problems, and are prudent in their research".[32]

In spite of such distance taken in regard to the psychoanalytic developmental theory, Piaget himself pointed out how much he owed to psychoanalysis, which, he wrote, "in my opinion has revolutionized the psychology of primitive thought".[33] In The Language and Thought of the Child (1923), he elaborated the concept of "egocentric" thought, and defined it as the type of thought intermediate between autism and intelligence. Autism is subconscious, individual, incommunicable through language, and governed by the laws of symbolism and immediate satisfaction. Intelligence, on the contrary, is conscious, adapted to reality, communicable, and governed by the laws of experience and logic. Egocentric thought, in turn, does not communicate itself, yet seeks to adapt itself to reality.

Piaget's first book on child psychology abundantly illustrates the inspiring and orienting influence of psychoanalysis, and its role as one of the major theoretical landmarks that helped Piaget define his own domain. As already mentioned, Piaget constantly composed a sort of counterpoint between his ideas and the psychoanalytic theory. For example, about ten years after the Berlin congress, at a meeting of French-speaking psychoanalysts, Piaget's discussion revolved around his concept of "schema", and led to a critique of the Freudian notions of the symbol, of memory, and of the unconscious.[34]

Piaget and the Practice of Psychoanalysis

Obviously, I have only skimmed over the topic of psychoanalysis as a landmark of Piaget's early intellectual landscape and as counterpoint of his evolving ideas. In fact, I might have remained on the surface of things from a less obvious point of view. After all, Piaget himself suggests that, at the beginning of his studies in psychology, his interest in normal thought was accompanied (perhaps even dominated) by an intense curiosity about the "tricks of the unconscious". Questions arise: How far went the connection between Madame Piaget's "poor mental health" and her son's concerns and development? What did Piaget mean when he said that this interest in psychoanalysis helped him "to achieve independence"? What did Pfister refer to when he preaised the young Piaget's "most careful personal researches"?

These questions can certainly not receive absolutely conclusive answers, but they can be explored in ways that avoid the risks of psychobiography. Piaget's personal researches must include, at least, the interpretations of dreams he carried out. One of these interpretations, presented in the 1919 talk on psychoanalysis and pedagogy, may lead to the two other questions.

To illustrate the psychoanalytic method, Piaget related the following dream, which he attributed to a 22-year-old student:[35]

> It was in a city, and I feverishly went through the streets, unsuccessfully looking for a room to live in. Suddenly, I see, at the window of a bus, and half-sideways [de demi-profil], a known figure smiling.

The interpretation of a dream, Piaget explained, must be based on the spontaneous associations suggested to the subject by different elements of the dream. In the present case, the subject was led to think of his mother. Piaget narrated the following association process:

> Indeed, for him [the subject], the choice of a room had been recently mingled with a question about which he was secretly in conflict with his mother. This conflict brings back other conflicts, and witnesses to an already long-drawn hostility. The subject thinks of the moral crises during which he tried to get rid of his maternal education and orient himself in a personal direction. In fact, at the time [of the dream], he is undergoing an analogous crisis.

Thus, Piaget concluded, the subject wishes to attain an "independent spiritual position"; such is the meaning of the search for a room, symbolically associated to the subject's intellectual research.[36] The associations aroused by the smiling figure suggest that the subject has doubts about the purity of his motives, and that he is afraid of discovering disreputable instincts behind his "negativism".

Piaget underlined that the interpretation of a dream is actually given by the dreamer himself. Moreover, he pointed out that although dream symbolism is overdetermined, the relations between a dream and the associations it evokes are real and manifest, to a certain extent, the subjects's personality. If we take into account such comments; if we keep in mind Piaget's emphasis on the need to practise some psychoanalysis to believe that its theoretical propositions are well-founded;[37] if we assume that he followed his own principle that no symbolic meaning should be attributed to an object without a solid documentary basis;[38] if we remember what he said about the psychodynamics of his relationship to his mother; if we do not forget about the central place occupied by "spiritual crises" in Piaget's

development; then we may conclude that this is a dream 22-year-old Piaget could have had, in 1918, at the time when he left his home for Zurich.[39]

Such a hypothesis suggests that psychoanalysis might have helped Piaget "achieve independence" by helping him not only to discover his complexes (in the later analysis with Spielrein), but also to realize through self-analysis the conflicts with his mother and their relation to his spiritual crisis. The self-portrait as a young man in the midst of moral and spiritual inner torments, given by Piaget in Recherche, is not oriented towards psychodynamics or family constellations; its framework is that of the confession, of the "journal intime", and of such novels as Romain Rolland's Jean-Christophe, rather than that of depth psychology. In contrast, Piaget's autobiographical statements about the relations between his intellectual development (including the interest in psychoanalysis) and his mother's "neurotic temperament" might actually be, in part at least, the result of a later self-analysis where the desire to gain a good understanding of the psychoanalytic method combined with the possibility of conceptualizing his own life; the whole, in turn, might have related to a belief in the effectiveness of the grasp of consciousness in the resolution of psychical conflicts.

Be it as it may, Piaget's "practical" interest in psychoanalysis continued after the 1919 Paris talk. As already pointed out, in 1921 he went through analysis with Sabina Spielrein. Now, if (as Piaget said) this analysis really lasted eight months at the rate of one session per day, then it is legitimate to suspect that its discontinuation was not simply due to Spielrein's realization that her patient would not adhere to the psychoanalytic doctrine. There is something too simple about Piaget's narrative; further research might contribute to pin down what, in this case, stands behind and beyond simplification. On the one hand, it seems clear that Piaget and Spielrein were quite close, personally and intellectually.[40] On the other hand, Piaget himself made at least two attempts at psychoanalyzing; these practical essays represent both the intellectual drive and the affective charge that animated his early attraction for psychoanalysis.

A few years ago, I met a Protestant minister who, while studying in 1924 at the Rousseau Institute, was offered a psychoanalysis by Piaget.[41] He accepted, and they fixed a two-month duration. Every

day, at eight in the morning, the "patient" went to Piaget's, lay on the couch, and talked while his "psychoanalyst" took down notes, without making interpretations. As the pastor told me, it was evident that Piaget "was not born a psychoanalyst, he was doing all that out of interest". Then, perhaps in 1925, Piaget tried to psychoanalyze his own mother. Madame Piaget, however, did not appreciate her son's interpretations, and the therapeutic attempt ended with a failure that strongly affected Piaget.[42] But risky trying was almost in the nature of those erarly and necessarily heroic times of psychoanalysis. As Piaget said about his analysis with Spielrein, "I was perfectly willing to be a guinea pig".[43]

The courage and moral seriousness that Pfister perceived in the promising "Gelehrter" were no flattery. Indeed, as emphasized by Gruber and Vonèche in The Essential Piaget, in 1919 psychoanalysis was still a taboo subject, and to approach it indeed required a nonconformist spirit. Nonetheless, neither in the environment of the Swiss Christian Students Association, nor in that of Zurich University and the Rousseau Institute was psychoanalysis unmentionable, or the uncounscious untouchable.

In Switzerland, psychoanalysis was at a crossroad between pedagogy and the psychology of religion, to which it could render important services. The two latter disciplines were oriented by democratic ideals; both emphasized individual experience, and personal and collective understanding; both favored the avoidance of conflict; both argued against the maintenance of an authority external to the individual. As Piaget himself pointed out in 1919, psychoanalysis could have a capital importance "from the moral point of view"; it could radically change pedagogy: "Il ne sera plus question de punir, mais de faire comprendre."[44] In 1932, The Moral Judgment of the Child would be animated by an analogous ensemble of values;[45] it grew out of the same framework as that of the reception of psychoanalysis in Switzerland. Yet, psychoanalysis is practically absent from that book. The thinker whose great psychoanalytic contributions Pfister anticipated had chosen to build, to a large extent at the very same Swiss crossroad where psychoanalysis stood, a theory that indeed made no room for uncomfortable, hardly controllable, even painful "tricks of the unconscious."

Notes

1. Bringuier (1980: 123).
2. Carotenuto & Trombetta (1980). The authors point out that Spielrein was never analyzed by Freud, and that there is no evidence of her having been sent to Geneva as propagandist.
3. Piaget (1952: 238). On p. 239, he attributes his "philosophical crisis" of adolescente "to family conditions and to the intellectual curiosity characteristic of that productive age." The "family conditions" relate to his mother's demand that he follow a course on religious instruction, and to the contrast between his father's attitude (he did not attend church...) and his mother's (she was a devout Protestant...).
4. Vidal & Vonèche (1983). For example, concerning the years studied here, Amann Gianotti & Ducret (1984) explicitly follow the second author's dissertation, "che prende appoggio e verifica le indicazioni auto-biografiche fornite dallo stesso Piaget" (p. 142).
5. Piaget (1952: 244).
6. Piaget (1918: 194 sqq.).
7. None of this is mentioned in Piaget's autobiography. For an overview, see Vidal (1985).
8. Piaget (1945).
9. Cifali (1983).
10. James (1958 [1902]: 386).
11. Freud (1927).
12. Flournoy (1928: 220, n. 1).
13. Flournoy (1916).
14. Flournoy (1913: 203).
15. Sainte-Croix 1916 (p. 8).
16. Cifali (1984).
17. Verzeichnis. It also includes the list of courses and professors.
18. Letter to Maurice Bedot, then director of the Museum of Natural History of Geneva, 1 november 1918. The letter is kept at the Museum.
19. Piaget (1920a).
20. Vidal et al. (1983).
21. Piaget (1952: 243).
22. Letter to Arnold Reymond, 16 October 1918. Letter kept with the Reymond papers at the Manuscript department, Bibliothèque cantonale et universitaire (Lausanne/Dorigny), and used with the kind permission of Madame A. Virieux-Reymond.
22bis. Freud (1914: 27). "Psychical epidemic" was the expression of Alfred Hoche, a Freiburg professor of psychiatry who attacked psychoanalysis.
23. Piaget recalls this fact in 1975 (p. 107).
24. Piaget (1920b: 18). I quote from the translation given by Gruber & Vonèche (1977: 54).
25. Pfister (1920: 295). I translate from the French translation (p. 91-92).
26. See, for example, Scheidhauer (1983). For the "Swiss sieve" ("tamis suisse"), see Mordier (1981). See also Cifali (1982).
27. See Bovet (1932), ch. 14, "Auto-suggestion et psychoanalyse éducatives".
28. Freud (1914: 61).
29. For example, Piaget (1921).
30. For the demonstration, see Piaget (1932).
31. Piaget (1923a). I quote from the English trans., p. 65.
32. Piaget (1923b: 301-302).
33. Piaget (1923a). I quote from the English trans., p. 21.

34. Piaget (1933). Piaget considered the symbol as an elementary thought form, rather than as the unconscious disguise of repressed impulses; he characterized memory as an active reconstruction of the past, rather than as the automatically formed reservoir of all our memories; and he defined the unconscious as the system of operations and active schemata on which memory bases itself to reconstruct the past, rather than as a memory on the basis of which schemata are formed. See the debate in Leuba (1934).
35. Piaget (1920b: 20-22) for the two next quotations; my translation.
36. ibid., p. 22.
37. ibid., p. 28.
38. ibid., p. 45.
39. Psychobiographical speculation can be thus avoided - but also such comments as: "Cet étudiant était-il Piaget lui-même? Etait-il un de ses camarades d'études? Impossible de répondre à ces questions". (Ducret, 1984 p. 705, n. 173).
40. Commenting on the idea that "dans le rêve le moi fusionne avec le monde et le faconne à sa manière", Piaget writes: "C'est là une thèse ... que nous nous sommes trouvé souvent tenir en commun avec Mme. S. Spielrein. Mme. Spielrein publiera prochainement une théorie très suggestive du symbolisme... Nous espérons reprendre ces hypothèses". (Piaget 1923b: 286, n. 1).
41. I thank Christiane Gillièron for helping me get in touch with Pastor Robert Jéquier, whom I interviewed at Le Locle in July 1981.
42. Recollection of the late Madame Marthe Piaget-Burger, whom I interviewed in 1981. She could not remember the exact year in which the analysis took place; I hypothesize that it took place during Piaget's stay in Neuchâtel (1925-1929), before his definitive return to Geneva.
43. Bringuier (1980: 123).
44. See Piaget (1920b: 49-50).
45. See Vidal (1986).

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DE QUELQUES ANALOGIES ENTRE G. STANLEY HALL ET JEAN PIAGET

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Summary

Some analogies between G. Stanley Hall and Jean Piaget. Both Stanley Hall and Piaget were leaders of the developmental psychology of their times. They also shared common psychological features: 1) sense of leadership; 2) imposition of their own point of view; 3) demand for strict orthodoxy on the part of their followers.

These common features can be explained first in terms of similar childhoods and adolescences in a strict Puritanical environment which cultivated the values of the so-called Protestant ethic and entered into contact with Positivism and liberal Protestantism. But it can also be explained by the similarity of social structures in the U.S.A. and Switzerland. The author feels that both ontogenetic and sociogenetic factors explain the resemblances of the two men.

Les ressemblances entre Granville Stanley Hall (1844-1924) et Jean Piaget (1896-1980) ne manquent pas d'être frappantes. L'un et l'autre se sont présentés en leurs temps respectifs comme les pères véritables de la psychologie génétique. L'un et l'autre furent des chefs d'école redoutables, imposant à leurs vues personnelles et tendant à ridiculiser les positions adverses. L'un et l'autre exploitèrent leurs collaborateurs intellectuellement et financièrement. L'un et l'autre enfin réussirent cette gageure de ramener le champ entier de la psychologie de l'enfant et de l'adolescent à leurs préoccupations personnelles pendant quelques décennies. L'influence de l'un comme de l'autre s'écroula assez rapidement après leurs décès respectifs.

Ces quelques analogies, rapidement esquissées, nous ont conduit à nous poser deux questions: 1) existe-t-il une similitude entre les enfances des deux hommes et 2) cette similitude est-elle attribuable à une sorte de splendide isolement culturel de la Suisse romande qui pratique, à la fois, des institutions politiques originales, une

confession religieuse minoritaire dans la francophonie et un conservatisme idéologique granitique?

En ce qui concerne la première question, les ressemblances et les différences sont nombreuses, mais au moment de la jeunesse les ressemblances priment les différences. En effet, si Hall est américain, paysan et pauvre, Piaget est européen, urbain et jouit d'une aisance bourgeoise. Si l'un ne connaît dans son enfance que la stimulation intellectuelle de l'école du dimanche et de la lecture familiale de la Bible, l'autre naît dans une famille d'intellectuels où la réligion réformée est considérée critiquement par le père.

Malgrés ces différences initiales considérables, le moment de la jeunesse sera vécu de manière assez semblable par les deux hommes. En effet, l'un et l'autre trouveront les cadres étroits du calvinisme puritain trop contraignants pour eux. Ils évolueront vers une forme de protestantisme libéral tentant de réconcilier la foi et la science.

Cette tentative de conciliation prendra les mêmes formes chez les deux auteurs comme nous allons le voir maintenant. Tout d'abord, l'un et l'autre sont interessés par l'histoire naturelle. Les biographes de Hall expliquent cet intérêt par un autre, plus profond psychologiquement, celui de connaître les origines (surtout celles des bébés). Chez Piaget, par contre, cet intérêt est le produit d'une socialisation ancienne dans la seconde enfance et d'une volonté de la part des éducateurs neuchâtelois d'éviter aux jeunes gens un intérêt trop clair pour la manière dont on fait les bébés. Mais ce qu'il y a de certain, chez les deux, c'est que la nature sert d'échappée belle à un quotidien triste et éprouvant (l'internat pour Hall, la famille por Piaget) et qu'elle devient rapidement por Hall, "un lieu d'extase poétique" et, pour Piaget, un endroit "dyonisiaque". Dans ces moments, l'un et l'autre sont partagés entre l'égotisme romantique et le rejet de soi propre au calvinisme, si bien que Hall écrit <u>The student's sin</u> (1866) où il oppose les dangers de la spéculation pure, riche de fantasmes et d'ambitions excessives, à la vie humble de la foi où l'âme s'oublie elle-même dans l'amour de Dieu. Il se pose ensuite, dans un autre essai <u>The Inventive Mood</u> (1863), la question de la relation entre une connaissance empirique l'imitée des phénomènes naturels et la saisie globale de leur signification ultime.

La réponse de Hall est simple: on peut concilier ces deux tendances apparemment contradictoires de l'esprit humain par un recours au sens moral qui s'opposerait aux idées "immorales" de personnages comme le Faust de Goethe, le Manfred de Byron et le Wallerstein de Schiller. Il ne faut pas vendre son âme au diable, mais il faut, comme Prométhée, faire descendre la connaissance du ciel sur la terre.

On retrouve, dans un contexte différent, les mêmes idées dans l'essai de Piaget (1915) La Mission de l'Idée, essai dont le titre est inspiré d'un vers romantique de Victor Hugo et qui vise aussi à régénerer la vision chrétienne en faisant appel à l'audace d'une nouvelle expérience spirituelle inspirée de la nature et de son observation comme idéal métaphorique du comportement moral.

Hall rencontrera les idées du protestantisme libéral qui mettent en avant l'attitude morale plutôt que la théologie proprement dite seulement entre 1867 et 1869, alors que Piaget les aura reçues plus jeune dans sa carrière. Tous deux penseront dans leur jeunesse que Jésus est une sorte de légende par laquelle les hommes expriment des vérités universelles issues de leur imaginaire. Par conséquent, ils forment tous deux l'idée d'un Dieu immanent à l'univers entier et co-naturel à celui-ci.

Dans ce moment d'intense recherche religieuse, l'un et l'autre pensent devenir théologiens et pasteurs. Hall sera étudiant à Union theological seminary, alors que Piaget ne réalisera jamais son désir d'approcher la théolgie au-delà d'une adhésion intense aux associations de jeunesse chrétiennes. De même, Hall montrera un intérêt évident por le grand innovateur évangélique allemand, Friedrich Schleiermacher (1768-1834), qui donnait une interprétation rationaliste et subjectiviste du christianisme. En effet, pour Schleiermacher, la foi n'est ni connaissance ni action, mais bien modification de la conscience immédiate de soi conduisant à un sentiment de dépendance absolue. Mais l'influence déterminante sur le développement de Hall à ce moment reste la pensée de Friedrich Adolf Trendelenburg (1802-1872), professeur de philosophie berlinois et chef du mouvement critique allemand qui s'était organisé en réponse à la fragmentation intellectuelle des années 1850 due au déclin de la religion et des systèmes philosophiques concurrement au succès des sciences naturelles. Trendelenburg espérait, par une critique de type kantien des limites de la connaissance et une critique historique de

la tradition philosophique, revitiliser la philosophie mourante. Trendelenburg pensait que la philosophie devait devenir empirique et répondre aux exigences de l'expérimentation pour pouvoir progresser, faute de quoi elle ne pourrait être qu'illusion ou simple sagesse. Cet intérêt pour l'empirie l'avait conduit vers la psychologie.

Au moment où Hall suit les cours de Trendelenburg, il n'est nullement impressionné. Ce sera seulement dans la décade suivante que les idées de Trendelenburg guideront Hall. Mais dès les années 1870, Hall a compris l'intérêt des préoccupations de Trendelenburg pour les processus et le développement.

Bien avant Léon Brunschvig en France, Trendelenburg avait tenteé de faire l'histoire de la connaissance le "laboratoire" du philosophe. Ce qui donnera lieu à cette conception "empiriste" et, sous l'influence de Haeckel, "récapitulationiste" de l'ontogenèse qui sera clairement le lot de Hall et, dans une mesure strictement formelle ou structurale, le cas de Piaget. Mais il est un autre aspect de la pensée de Trendelenburg qui influencera Hall et Piaget: la nécessité de résoudre "expérimentalement" les questions philosophiques. Le libre-arbitre, la genèse des connaissances, le jugement moral, autant de concepts philosophiques qui ont, pour Trendelenburg, une origine socio-culturelle et une histoire propre, qui ne sont, par ailleurs, plus passibles d'une simple réflexion dans un fauteuil mais qui requièrent une enquête empirique approfondie par des méthodes scientifiques telles que celles de la psychologie expérimentale.

D'autre part, Trendelenburg est convaincu que ce type de travaux ne peut être mené que par des Protestants seuls capables d'avoir la liberté de conscience nécessaire à une recherche sans a-priori, ni parti-pris et donc objective.

Une fois encore, le parallélisme entre Piaget et Hall est évident. Les idées de Trendelenburg sont parvenues à Piaget aussi via Sabatier et Arnold Reymond.

De même, les idéees d'Alfred Tennyson voulant réconcilier poétiquement la divinité et la nature ont conduit Hall à vouloir en élaborer un équivalent scientifique en réconciliant science et foi dans une sorte de psychologie empirique.

L'influence de Friedrich von Schelling sur Hall est aussi évidente, puisque Hall voit la conscience comme le produit de l'évolution de la nature; ce qui n'est pas sans rappeler l'idée fondamentale de la philosophie de l'identité de Schelling identifiant nature

et esprit au point de voir dans la matière de l'esprit dormant et dans l'esprit de la matière en éveil par une sorte d'hégelianisme de droite.

Piaget, quant à lui, n'ira jamais aussi loin. Il demeurera toujours soucieux de coucher les mêmes idées dans une forme scientifiquement plus acceptable. C'est ainsi que l'identité nature/esprit est exprimée par Piaget comme un des avatars du parallélisme psychophysique. Le souci de légitimité scientifique est constant chez Piaget. C'est pourquoi les idées de Trendelenburg sur le développement ne sont pas rattachées par Piaget au philosophe allemand lui-même, mais à l'évolutionnisme biologique et à la valeur de la méthode expérimentale.

Quoi qu'il en soit de la manière dont l'un et l'autre expriment leurs idées, Hall et Piaget vont trouver, après cette crise philosophique, leur voie dans la psychologie considérée comme une sorte de philosophie expérimentale justifiant leurs thèses métaphysiques personnelles. Ils deviendront l'un et l'autre, après avoir beaucoup promis et beaucoup tenu, les "grands" de leur génération: chefs de sectes dominateurs et intransigeants sous des apparences affables, expert aux intrigues universitaires et impitoyables envers ceux qui auraient pu leur faire ombrage.

La ressemblance frappante entre Hall et Piaget à près de 50 ans de distance pose une question autrement importante que celle de la structure de la réussite dans la cité scientifique qui n'est finalement accordée qu'à ceux qui lui ont tout sacrifié. Cette question est celle de savoir si la similitude entre Hall et Piaget est due au caractère non cumulatif du savoir en sciences humaines ou au splendide isolement culturel de la Suisse romande. En effet, il semble qu'à près d'un demi-siècle de distance, ce soit le même débat qui agite le milieu propice à l'épanouissement d'une psychologie de l'enfant: réconcilier la foi et la science, soumettre le débat épistémologique à l'investigation expérimentale de laboratoire par le truchement de la psychologie expérimentale, admettre que seule la science présente une vision cohérente du monde.

En réalité, ce que nous mesurons ici c'est le retard idéologique de la Suisse romande plutôt que le tout aussi réel piétinement des sciences humaines. En effet, alors que le monde anglosaxon à tout le moins et une bonne partie de l'Europe occidentale sont acquis au darwinisme vers 1914, la petite république de Neuchâtel en reste

encore à une biologie lamarckienne, sinon à son ancêtre pré-jacobin, d'histoire naturelle basée sur les collections à la manière de Rousseau, Linné ou Tournefort reposant toutes sur le regard comme instrument épistémologique.

Les idées théologiques ne sont pas plus avancées non plus puisqu'à un moment où, ailleurs dans le protestantisme, on dépasse l'opposition entre le protestantisme traditionnel et le protestantisme libéral par un retour au dogmatisme, Neuchâtel découvre le protestantisme libéral. Si bien qu'il n'y a rien d'étonnant à ce que Piaget reprenne la voie suivie par Hall. Ceci d'autant plus que, comme dans les Etats-Unis calvinistes de Hall, l'enseignement de la philosophie est encore, en Suisse romande, entièrement aux mains des théologiens qui ne peuvent concevoir d'autre philosophie qu'apologétique. Sous l'influence des idées socialistes qui ont pénétré dans le Jura neuchâtelois, une réaction violente se prépare durant les années 14-18 chez certains pasteurs contre cette conception de la religion et de la philosophie et Piaget le sent sans y adhérer complètement.

Comme on le voit, c'est plus la situation paradoxale de la Suisse romande qui est ici en cause que le caractère non-cumulatif des sciences humaines, puisque la Suisse romande partage avec la France une langue commune ou presque, avec l'Angleterre une théologie commune ou presque, et avec les Etats-Unis un système politique commun ou presque. Ce qui a pour effet, à la fois, une ouverture plus grande sur le monde que dans les pays plus monolithiques, mais aussi un repli sur soi-même couplé avec une méfiance plus grande pour les idées nouvelles, renforcée encore par le fait que la Confédération helvétique venait presque de se dissoudre en 1846-47 à cause des idées pendant la guerre du "Sonderbund" où les cantons catholiques s'étaient dressés contre les autres. L'effet durable de cette guerre civile sera, pour le pays de Piaget, de refuser assez systématiquement le débat idéologique au profit du compromis pratique et ponctuel. C'est là un autre aspect de la proximité entre la Suisse et les U.S.A.

Le retard idéologique de la Suisse romande semble donc rendre suffisamment compte des ressemblances frappantes entre Piaget et Hall. Mais alors, comment se fait-il que malgré cette hystérésis culturelle romande, Piaget ait pu connaître un tel succès professionnel? Normalement, il aurait dû être complètement ignoré par la

psychologie scientifique de son temps, puisqu'à beaucoup d'égards, il reprenait Hall.

Deux remarques s'imposent ici. Tout d'abord, une similitude dans la genèse des idées ne signifie pas nécessairement une ressemblance des produits finis. Les écarts entre les théories de Hall et de Piaget sont assez significatifs de ce point de vue pour ne pas avoir besoin d'y revenir ici. D'autre part, des similitudes de comportement comme chefs d'école n'impliquent pas non plus une pure répétition dans les théories. Il ne faut donc pas tirer des ces ressemblances - aussi frappantes qu'elles puissent être parfois et jusque dans le détail - plus qu'elles ne peuvent donner.

Si nous analysons maintenant les raisons du succès considérable de Piaget en psychologie génétique, nous devous commencer par reconnaître deux périodes distinctes de gloire. Une première période va de 1925 à 1940 et caractérise un Piaget psychologue social dont le modèle américain est James Mark Baldwin. Une seconde période couvre les années 50-70 et est sans doute celle qui a laissé la plus grande impression sur ses collègues qui le réduisent le plus souvent à cette dernière période caractérisée par son formalisme logique, son intérêt quasi exclusif pour la vie cognitive et les isomorphismes partiels entre histoire des sciences et développement ontogénétique.

Si Piaget était resté le psychologue du développement sociocognitif, il aurait eu à résoudre un problème qu'apparemment il ne parvenait pas à résoudre et qui est celui des relations entre le social et l'individuel dans la transmission des normes et des valeurs. Pour Baldwin, comme pour Hall, cette transmission se fait exclusivement par imitation individuelle. C'était en France le point de vue de Tarde aussi. Mais face à lui s'élevait la puissante école d'Emile Durkheim qui expliquait cette transmission par le prégnance des institutions sociales sur l'individu. C'est assez dire qu'au moment où Piaget écrit, la question s'est compliquée. Il lui faut, soit se rallier à Tarde ou Durkheim, soit trouver une position intermédiaire entre ces deux extrêmes. Il croit, au moment de la rédaction de La Formation du Symbole, pouvoir le trouver dans les travaux de Paul Guillaume sur l'imitation chez l'animal et l'enfant. Ces travaux montraient, avec beaucoup de subtilité que tant l'animal que l'enfant n'imitent que ce qu'ils connaissent déjà. Mais une telle position poussée jusque dans ses dernières conséquences aurait conduit Piaget vers la Gestalt par rapport à laquelle il avait

antérieurement pris ses distances. C'était donc l'impasse théorique. Des considérations personnelles vers les années 40 comme la réorganisation d'une partie de sa vie affective, les tragiques événements de la seconde guerre mondiale qui vont l'isoler du reste de la communauté scientifique, ainsi que sa soif de respectabilité scientifique pour son domaine de recherche propre vont amener Piaget à revenir sur les questions de logique et de sciences exactes et à proposer son modèle des opérations logico-mathématiques de la pensée.

C'est alors que le succès de Bourbaki, de la logique formelle, des géométries nouvelles et, d'une manière générale, d'une certaine "avant-garde" intellectuelle et artistique vont donner aux travaux de Piaget un prestige nouveau et l'apparence d'une grande modernité. Ceci, une fois de plus, rapproche Piaget de Hall qui lui aussi connut deux époques successives de renommée. En effet, dans une première période allant de 1880 à 1892, Hall présenta une nouvelle psychologie qui se composait d'une psychologie historique inspirée de Wundt par laquelle il entendait une histoire naturelle de l'esprit (mind) incluant l'étude des animaux, des enfants, des adolescents, des peuples primitifs et des croyances populaires ainsi qu'une analyse de la philosophie, de la science et de la religion. Cette tentative parallèle, à bien des titres, de l'épistémologie génétique selon Piaget, fut la première époque de gloire de Hall. La seconde, allant de 1892 à 1924, se caractérise surtout par ce qu'on appelle le Child Study Movement qui est un movement de rénovation pédagogique dont Pierre Bovet et Edouard Claparède s'inspireront pour leur Institut Jean-Jacques Rousseau à Genève.

Claparède avait retenu du Movement de Hall surtout la dimension biologique faisant de la connaissance un besoin instinctif comme les autres. Mais dans une telle perspective, l'enfant ne peut se distinguer du primitif. Pour garder et expliquer la spécificité de l'enfant par rapport au primitif, Piaget, jeune chef de travaux à l'Institut Rousseau, est obligé de reconnaître que l'enfant est, quant à lui, soumis à la contrainte sociale. D'où sa première tentative en forme de psychologie sociale. Mais celle-ci va expliquer la croissante mentale de l'enfant par sa sortie de l'égocentrisme grâce à la coopération que deviendra, pour le second Piaget, co-opération avec tout l'accent sur le terme opération par lequel il donne les choses de la logique pour la logique des choses.

Fondamentalement, sa pensée n'a pas changé, comme Hall, Piaget veut donner une explication biologique de la connaissance et il le fait par un glissement subreptice de l'invariance fonctionnelle de l'organisme biologique à l'ordre social d'abord, à l'organisation logique, ensuite. Comme on le voit donc, le deuxième succès de Piaget est bien lié à la permanence dans son oeuvre du souci oublié par les grands pays puis redécouvert par eux d'un vieux problème du siècle passé, la relation entre la norme et le fait, que s'était posé le protestantisme libéral qui voyait dans la psychologie et les sciences humaines une avenue conduisant à sa solution.

Si bien que l'on peut affirmer, en conclusion, que l'absence de progrès linéaire dans les sciences humaines est due essentiellement à leur volonté de reprendre dès la seconde moitié du XIXe siècle, le rôle normatif joué précédemment par les sciences juridiques consistant à justifier les préjugés du temps, du lieu et de la société qui les produisent.

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SANDOR FERENCZI, UN DES PREMIERS PROFESSEURS DE LA PSYCHANALYSE

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Summary

Sándor Ferenczi (1873-1933) joined Sigmund Freud and the doctrines of psychoanalysis in 1908. His prolific literary activity in the subject is shown by the 80 essays published within ten years (1908-1918). As a result of Ferenczi's organizing activity, the Psychoanalytical Association of Hungary was founded in 1913. One of the outstanding achievements of the activity of Ferenczi and the Association was the organization of the 5th Internation Congress of Psychoanalysis in Budapest, in September 1918. As E. Jones pointed out, this was the first congress where government representatives took part as well, which interest was justified by the topic "was neuroses". Analysis as a therapeutic treatment began to assume official recognition. The scientific and cultural renewal following the fall of the Habsburg monarchy required changes in medical training, too. Sándor Ferenczi also added his remarks on the proposed curriculum, emphasizing that "the science of mental healing should not be absent from the knowledge of the starting practitioner". The importance of the topic was increased by the fact that Freud himself entered the debate and expressed his opinion. The teaching of psychoanalysis appeared almost as a social claim, which the Hungarian Soviet Republic wished to solve in April 1919 by the appointment of Sándor Ferenczi. Ferenczi's professorship lasted but a few months. As a collaborator of the leftist government he was not only removed from his university chair but also expelled from the Medical Association of Budapest.

C'était relativement tard que Sandor Ferenczi devint le suivant de Freud, mais il se rattacha avec autant plus d'énergie à la diffusion des principes et méthodes de la psychanalyse apportant le

renouvellement de la neurologie et de la psychiatrie. Ferenczi rencontra Freud pour la première fois, sur le propos de Jung, le 2 février 1908, ce qui marque le commencement d'une unique amitié scientifique.

Il ne se passait guère six semaines depuis le premier rencontre avec le fondateur de la psychanalyse et, le 28 mars 1908, Ferenczi tint déjà son exposé en tant qu'analyste zélé à la Société Royale des Médecins de Budapest, sous le titre Les névroses à la lumière de l'enseignement de Freud et la psychanalyse. (Ferenczi, 1908). Dans son exposé l'auteur fit connaître l'avis de Freud sur la classification correcte des névroses et sur le role de la sexualité dans l'étiologie des psychopathies fonctionnelles. Il explicita que dans les névroses actuelles la cause était quelque anomalie des fonctions sexuelles. Les causes pathologiques pouvaient être découvertes par la psychanalyse sous la forme des souvenirs oppressés, phantasmes et désirs refoulés.

L'auteur illustra par plusieurs exemples que la méthode de la psychanalyse pouvait réussir à guérir même des symptômes sévères des psychonévroses. Les analyses prouvaient que l'origine des psychonévroses était en connection avec des vécus sexuels d'enfance et des rêves s'y rattachant. L'exposé fut suivi d'un grand débat ou participèrent des grands personnages de la médecine traditionnelle comme Gyula Donath, Artur Sarbo et autres.

Après cette conférence Ferenczi se mit à un travail remarquable en tant qu'écrivain et conférencier, bien que outre la Société des Médecins. Un mois après il tint son exposé - toujours actuel - à Salzburg au Ier Congrès International de la Psychanalyse sous le titre "Psychanalyse et pédagogie". Ses 80 écrits publiés en 1908-1918 témoignent de son étonnante fécondité littéraire.

Ce n'était seulement dans ses publications que son activité se manifestait. Il tenait des conférences, il faisait des recherches, il organisait le mouvement psychanalytique. Il se chargeait d'un role important dans la préparation d'une organisation internationale des analystes en 1910. Il acheva comme organisateur le création de la Société Hongroise de Psychanalyse en mai 1913, dont il était le président pendant plusieurs décades (Linczényi, 1982).

Un produit spectaculaire de l'activité efficace de Ferenczi était l'organisation du Ve Congrès International de la Psychanalyse à Budapest les 28-29 Septembre 1918, dans la salle d'honneur de

l'Académie Hongroise des Sciences. "Ce fut le premier congrès où même des délégués de gouvernement aient pris part, ce temps-la c'étaient les gouvernements de l'Autriche, de l'Allemagne et de la Hongrie qui se firent représenter. Leur participation s'expliquait par le fait qu'on commencait à reconnaître le rôle des "névroses de guerre" dans le planning militaire (Jones, 1973).

Au congrès, d'ailleurs, il y avait 42 analystes, des médecins et chercheurs sympatisant y compris.

Le congrès avait un effet stimulatif surtout parmi des jeunes médecins et étudiants. Il contribuait à la fondation, de l'initiative des étudiants, de la "Société Psychanalytique de l'Ecole Supérieure" en janvier 1919. (Litvan, 1984).

La vie scientifique et culturelle renaissant après la guerre exigeait de plus en plus le changement des cadres anciens, dépassés, fossilisés en contenu tout comme en forme. La réforme de l'enseignement universitaire en était une manifestation. La réforme de l'enseignement médical fut élaborée par la commission du syndicat des médecins nouvellement organisé.

Sandor Ferenczi était un des premiers rapporteurs qui, outre ses nombreuses propositions, écrivit les suivants:

> "De toute façon, l'observation de la psychothérapie (psychanalyse) manque au programme présent de l'enseignement. L'étudient apprend aussi peu de la psychiatrie d'aujourd'hui que des expériences de la psychologie expérimentale conduites sur les machines. Or, c'est l'intérêt de l'humanité d'avoir des médecins qui possèdent les méthodes pour guérir non seulement les malades du corps mais aussi ceux de l'esprit ce qui veut dire en fin de compte les malades de corps aussi, comme nulle maladie n'existe où l'observation de l'état d'âme n'ait pas une importance primordiale. La science de guérir l'esprit ne doit pas manquer aux connaissances du médecin se portant vers la pratique." (Ferenczi, 1919).

Freud lui-même prit la parole dans le débat. Dans son écrit paru dans l'hebdomadaire médical Gyogyaszat sous le titre Faut-il enseigner la psychanalyse à l'université?, Freud (1919) était unanimement pour l'introduction d'une telle matière.

> "La question si la psychanalyse soit enseignée à l'université peut être approchée de deux côtés; du point de vue de l'analyste et de celuide l'université.
> 1. L'analyste ne serait donné que satisfaction si l'enseignement universitaire intégrait la psychanalyse au curriculum, mai lui, il peut manquer l'université sans aucun domage. La direction théorétique dont il ait besoin, il peut trouver dans la littérature et encore mieux aux séances des sociétés de psychanalyse ainsi qu'à travers des communications personnelles avec des membres

anciens pleins d'expériences. Il peut gagner des expériences pratiques par le traitement des cas outre son auto-analyse suivi sous la direction et inspection d'un psychanalyste distingué.
Cette organisation se créait juste à cause de l'exclusion de l'université. Si cette exclusion subsistait, cette organisation sans doute soutiendrait l'épreuve dorénavant aussi.
2. Du point de vue de l'université, la question se pose si elle veut au fond reconnaître l'importance de la psychanalyse dans la formation du médecin et de l'homme de science. Si elle veut, elle doit penser aussi à comment la mettre dans la cadre de l'enseignement."

A la fin de son essai Freud remarque que l'enseignement ne fait pas encore l'analyste, car tout comme le chirurgien, celui qui porte un intérêt pour la psychanalyse doit chercher à se perfectionner régulièrement.

Après ces antécédents on comprend qu'en avril 1919, le gouvernement de la République Hongroise des Conseils voulait résoudre l'enseignement de la psychanalyse étant en suspens par la nomination de Sándor Ferenczi à la faculté de médecine. Bien qu'il manque des documents datés de cette période, la décision du 12 mai 1919 de la séance de la Faculté de Médecine de l'Université de Budapest atteste que la chaire de psychanalyse fut donnée à Ferenczi.

L'activité du premier professeur hongrois de la psychanalyse fut courte. Les documents trouvés dans les archives de la Société Royale des Médecins de Budapest rendent compte de ses jours (Semmelweis Archives d'Histoire de la Médecine).

Les rapports entre Sándor Ferenczi et la Société Royale des Médecins de Budapest se rafraichissaient après la conférence de 1908 reçue avec critique, Ferenczi n'entreprit plus de performance publique dans cette société scientifique de grande tradition, il cherchait d'autres forums. Il ne rompa point avec la Société des Médecins mais restait l'auditeur muet des séances.

Le nom de Sándor Ferenczi rapparut dans les écrits de la Société des Médecins aux premier mois de 1920, quand le Bureau d'Administration, sous une pression extérieure, procédait contre ses membres ayant eu un rôle actif pendant la République Hongroise des Conseils. Il faut remarquer que la Société des Médecins ne montrait aucun empressement pour commencer l'enquête, comme elle en avait reçu la première sommation déjà en août 1919. Selon les procès-verbaux survécus, le Bureau d'Administration en prit connaissance et la mit dans les archives sans avoir fait aucune mesure.

Malheureusement les procès-verbaux survenant de cette période sont imcomplets, les suppléments étaient enlevés pour des raisons inconnues (p.e. des documents présentés au gouvernement Friedrich, au Bureau d'Administration, à la comité d'enquête, etc.), seules les copies au net des procès-verbaux restent à notre disposition. De l'activité de la comité d'enquête aussi, il ne restait que les décisions concernant des personnes et quelques suppléments y relatifs. Après la terminaison des enquêtes, le 24 avril 1920 le Bureau d'Administration se réunit à une séance extraordinaire afin de prendre la décision dans l'affaire des 22 exclusions proposées par la comité d'enquête. La comité d'enquête procédait dans les cas d'une cinquantaine d'hommes et proposait enfin 22 exclusions, en cas de six personnes elle proposait que la Société des Médecins exprime sa réprobation, les autres étaient déclarés justifiés. A la séance du Bureau d'Administration, le Professeur Vilmos Tauffer énonça qu'en ce qui concernait les exclusions, il ne pouvait suivre la majorité qu'avec certaines doutes étand donné que les status de la Société des Médecins ne proposait pour l'exclusion que ceux qui manquaient à ou allaient contre la profession médicale.

C'était au milieu des scènes tumultueuses que la réunion extraordinaire du 28 mai 1920 apporta sa décision et écarta 22 de ses membres.

Un discussion à part se formait pour l'affaire de Sándor Ferenczi étant donné qu'un groupe de soixante se désaccordait sur son éloignement et le proposa pour justification. Néanmoins, il faut remarquer que l'exclusion de la Société des Médecins n'entraîna aucune conséquence, n'influença point sa pratique médicale dans la suite.

Les documents ci-publiés, bien que la matière soit incomplète, contiennent en extrait l'enquête procédée contre Sándor Ferenczi. Tout d'abord il y manque la proposition signée par 30 personnes et présentée au Bureau d'Administration pour ordonner une enquête contre Sándor Ferenczi, puis la décision de ce corps dans laquelle le cas de l'éminent psychanalyste fut exposé à la commission d'enquête. Il ne subsiste pas, en copie non plus, la réquisition qui devait contenir les questions posées à Sándor Ferenczi par la commission d'enquête. Nous présentons ici la réponse y faite le 26 janvier 1920 certifiée par la signature de Sándor Ferenczi (document no. 1), ainsi que les extraits du procès-verbal fait à l'interrogatoire personnelle de

Sándor Ferenczi le 5 mars 1920 (document no. 2). Le dernier paragraphe de ce procès-verbal étant la décision de la commission d'enquête fut inscrire plus tard d'une autre main que les précédents. Aucune signature ne se trouve dans ce procès-verbal et aucune référence aux personnes présentes à l'audition. Mais il est notable que la lettre de Sándor Ferenczi contient les circonstances de sa nomination comme professeur de l'université ce que le procès-verbal de l'audition confirme aussi. Ce document se réfère également à quelques accusations que la plainte contre Ferenczi devait contenir (p.ex. qu'il demandât lui-même sa nomination).

La formule d'exclusion contient aussi une "modification": "La commission restreinte, après avoir écouté à plusieurs interventions et examiné l'affaire de tous côtés, modifie son opinion afin de proposer l'exclusion". Cette courte clause marque déjà que même la commission d'enquête était en désaccord sur l'exclusion de Ferenczi, la commission plénière ne voulait point l'éloigner de la Société, probablement il y en avait une décision qu'on changea le 5 mars 1920. C'était peut-être en raison de cela que plus tard à la séance extraordinaire - malheureusement les noms n'étaient pas retenus dans le procès-verbal - plusieurs personnes plaidèrent en sa faveur et parmi les 22 membres exclus il reçut le plus bas pourcentage négatif.

Notre étude et les documents ci-publiés cherchent à prouver comme il était pénible au début, même pour une autorité reconnue comme Ferenczi, de faire accepter les thèses de la psychanalyse. La psychanalyse, professant la libération de la personnalité et le débarras des contraintes et des inhibitions ne pouvaient se rendre favorable pour tous, donc elle était poussée à l'arrière-plan dans la thérapeutique aussi. Les mouvements de gauche et leur gouvernement ont bien appuyé la psychanalyse, mais cela ne suffisait pas à s'acquérir une place dans les sciences académiques.

Document no. 1

A Budapest, le 26 janvier 1920.

Cher Monsieur le Collègue!

En recevant aujourd'hui votre lettre du 22 courant, je vou prie de communiquer à M. le Professeur Haberern lui-même les suivants:
ad. 1. Le 25 avril 1919 moi et trois autres (les docteurs István Tóth, Lajos Török et Arnold Winternitz) fûmes nominés comme professeurs de l'université. J'appris ma nomination de la lettre suivante du Monsieur le Doyen Krompechner: "Au moment où j'ai l'honneur de vous aviser de votre nomination comme professeur de l'université, je prends avec joie l'occasion pour exprimer mes meilleurs voeux pour votre activité de professeur titulaire." Suivant l'instruction du Monsieur le Doyen, je pris la direction du département sanatorial B par voie du bureau économique.
ad. 2. Qu'à qui ou à quoi je dus ma nomination - je ne le rechercait point.
ad. 3. Au sanatorium Batizfalvy je fis nommé le propriétaire le docteur István Trajtler comme médecin assistant auprès de moi, j'étais contre la pression des constructions tellement désirées par des autorités donc elles n'avaient pas eu lieu de tout. Je cherchais à conduire le service des malades de façon qu'il passe toute influence politique.

Je suis avec un profond respect
Dr. Sandor Ferenczi
neurologue.

(Traduction du hongrois de la copie au net du manuscript original par Erika H. Bánóczy).

Document no. 2.

L'AFFAIRE DE SANDOR FERENCZI

Le neurologue Sándor Ferenczi pendant la dictature du prolétariat fut nommé comme professeur titulaire à la chaire de psychanalyse alors établie (en avril 1919) par le commissariat du peuple de l'éducation, et comme lieu d'activité le Sanatorium Batizfalvy fut lui assigné lequel avait été communisé par le commissariat du peuple tout comme les autre sanatoriums. Il expose l'histoire de sa nomination: il ne sait pas à qui il doit sa nomination. Il sollicita le privatdocentisme déjà en 1918, même le professeur Moravcsik le recommenda, mais ne pouvant pas gagner la majorité de votes il manqua le privatdocentisme. Un groupe d'étudiants qui connaissaient son activité allèrent le chercher et luis proposèrent qu'il demandât au Doyen de donner une pièce où Ferenczi les aurait enseignés, mais il répondit que c'était impossible qu'il demandât une chose pareille au Doyen. Quand est-ce que cela se passait? Ici il hésite dans sa réponse, une fois il dit qu'en 1912 ou 1913, l'autre fois qu'à la troisième ou quatrième année de la guerre. Par la suite les étudiants recueillaient quelques cents de signatures pour présenter la dite demande au Ministre des Cultes.

Un de ses connaissances lui dit qu'il avait vu cette sollicitation sur la table du M. Ruszten Vambéry, le recteur d'alors. Il croit probable que le commissaire du peuple trouvait son affaire parmi des actes insatisfaites enregistrée ensemble avec celle du professeur Tóth. D'ailleurs il jura à M. le Recteur ensemble avec le professeur Tóth. Il prit sa nomination avec des sentiments mixtes, puisqu'il avait l'impression que la dictature du prolétariat ne serait pas de longue durée, mais il vit se réaliser le comble de ses voeux en recevant sa nomination, comme il voulait toujours prendre part dans l'enseignement de la jeunesse. On lui pose la question pourquoi accepta-t-il la nomination, il y en avait plusieurs qui refusèrent des postes offerts. Il répondit que le poste ne fut pas offert à lui mais la nomination fut lui remise en mains et il n'osa pas la refuser en crainte des conséquences.

Il déclara qu'il ajournait les cours jusqu'à ce qu'il pouvait, et les commença (le 8 juillet 1919) en même temps que les autres "collègues".

La commission restreinte, après avoir écouté à plusieurs interventions et examiné l'affaire de tous côtés, modifie son opinion afin de proposer l'exclusion.

Appelée à la séance du 3 mars 1920.

(Traduction du hongrois de la copie au net du manuscript original par Erika H. Banoczy).

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ANALYSE COMPARATIVE DU PROCESSUS D'INSTITUTIONNALISATION DES SCIENCES SOCIALES DANS LES FACULTES DE DROIT ET DANS LES FACULTES DES LETTRES AUX XIXème ET XXème SIECLES.

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Summary

Applied to social sciences the concept of institutionalization defines the social process which conducts to recognition and legitimacy in society as well as its concrete consequences. Such an issue is decisive in France because the type of institutionalization observed in the Facultés commands:

1. the orientation and nature of the production of knowledge;
2. the relations between institutions and disciplines;
3. the emergence outside the University of other institutions destined to remedy the academic deficiencies;
4. the dispersion of the theoretical and methodological orientations of each discipline;
5. the failure in the treatment of the major problems which define the disciplines and which are central challenges for their failure: i.e. the problems of teaching, of research and of professionalization.

This paper analyses the institutionalization of political economy in the Facultés de Droit and of sociology in the Facultés des Lettres. The parallel is great in the situation of both disciplines. First it took a long time more than a century for political economy before the two disciplines secured their autonomy within the Facultés. Secondly the institutionaliztion remained limited.

Nevertheless we observe differences. Some of which are linked to the different professional orientations of the Facultés de Droit and of the Facultés des Lettres. Others proceed from the relations between production of knowledge and institutionalization. For political economy the emergence of the science took place before the institutionalization. In the case of sociology it was the contrary.

Par institutionnalisation, on désigne l'ensemble des actions entreprises en vue d'obtenir la reconnaissance et la légitimation des sciences sociales dans la société ainsi que leurs effets. En France

une telle question est fondamentale parce que le type d'institutionnalisation pratiqué au sein de ce qu'on appelle l'Université et qu'il vaut mieux appeler les Facultés éclaire.

1). l'orientation et la nature de la production intellectuelle et scientifique,

2). les relations entre institutions et disciplines au sein de la recherche en sciences sociales,

3). la création au dehors de l'Université d'autres institutions destinées à combler les lacunes de l'enseignement universitaire,

4). la dispersion des orientations théoriques et méthodologiques de chaque discipline envisagée,

5). le non traitement des problèmes majeurs, qui définissent les disciplines et qui sont autant d'enjeux décisifs pour leur avenir à savoir les problèmes de l'enseignement, de la recherche et de la formation professionnelle.

S'agissant des Facultés de Droit et des Facultés des Lettres et de leurs rapports, il paru utile de retenir une perspective d'histoire longue qui prenant en compte l'évolution des disciplines sur plus d'un siècle ferait apparaître les contraintes autant que les ruptures ainsi que le poids des héritages et des traditions nationales.

Au sein des Facultés des lettres, les sciences sociales ont ainsi connu une institutionnalisation que l'on peut qualifier de "molle" qui leur a assuré certes l'existence et la reconnaissance mais de manière très limitée. En effet les sciences sociales se penètreront pratiquement pas au coeur du système d'enseignement supérieur, c'est à dire dans les Grandes Ecoles. De même, dans le cadre des Facultés de Droit l'économie politique aura les plus grandes peines à acquérir le statut d'une discipline autonome.

Dans les lignes qui suivent il ne sera pas question de l'ensemble des sciences sociales mais de l'économie politique au sein des Facultés de Droit et de la sociologie dans les Facultés des Lettres (1).

La France n'a pas d'université au sens que ce terme a dans des pays de langue et de civilisation anglo-saxone où il définit une institution complexe combinant plusieurs fonctions sociales de formation, de recherche et de professionnalisation mais des universitaires regroupés dans le cadre des Facultés jalouses de leurs privilèges. Il n'en est que plus fondamental de saisir la place et le rôle

des diverses Facultés dans l'ensemble du système d'enseignement supérieur, en fait les relations qu'elles entretiennent entre elles et avec leur environnement - car c'est ainsi que l'on peut comprendre l'accueil et le traitement qu'elles ont réservés aux sciences sociales.

On commencera par définir rapidement les positions respectives des Facultés des lettres et de Droit dans l'espace universitaire avant de saisir le processus d'institutionnalisation proprement dit et ses effets. Par delà des différences indiscutables le parallélisme des situations est frappant: longtemps dominées par d'autres disciplines au sein des Facultés de Droit et de Lettres, économie politique et sociologie n'ont vu leurs tentatives d'émancipation couronnées de succès qu'après la seconde guerre mondiale.

Dans le cadre limité de cette communication, il ne saurait être question de traiter de la structure du système d'enseignement supérieur, qui a, comme on sait, fait l'objet récemment de plusieurs études (2). Qu'il suffise de rappeler ici que les Facultés forment un premier sous-ensemble distinct du sous-sensemble des grandes Ecoles chargées du recrutement et de la formation des élites économiques, sociales et politiques. Dans l'ensemble des Facultés, les Facultés de Médecine et les Facultés de Droit constituent un bloc à part en raison des finalités professionnelles des formations que sont les leurs depuis Napoléon.

I. L'organisation universitaire des sciences sociales.

Les Facultés de Droit

Le Consulat définit les "grades nécéssaires" pour éviter le charlatanisme chez les médecins et l'ignorance parmi les magistrats et les avocats. La loi du 22 ventôsean XII réorganisait l'enseignement des Ecoles de Droit transformées en Facultés par le décret de 1808: cinq professeurs comme dans les autres Facultés, une licence de trois ans, un doctorat de un an.

L'enseignement y était fondé sur le Droit romain dans ses rapports avec le Droit, le Code civil, la procédure.

Cet enseignement prépare essentiellement au barreau, à la magistrature et aux charges d'avoué. Ni l'histoire, ni la philosophie du Droit, ni l'étude des sciences et de droit comparé, rien de ce qui pourrait relativiser le Code civil, en faire une solution parmi d'autres ne font partie du programme.

Comme le montre L. Levan-Lemesle (3), on est devant un enseignement pratique et professionnel atténué et amélioré par une prise de conscience de l'histoire du Droit, et la lutte tenace contre les interventions de l'Etat en matière d'enseignement et de nomination.

Autre trait essentiel des Facultés de Droit: leur efficacité instrumentale. Avec un corps enseignant dont la taille est du même ordre de grandeur, on enregistre de neuf à quatre fois plus de licenciés qu'en lettres: 1136 licenciés entre 1870 et 1875, 1822 licenciés entre 1911 en 1913 contre 133 et 519 licenciés en lettres.

Le statut de la recherche désintéressée et des disciplines de pure érudition ou des sciences étrangères à des pratiques professionnelles est toujours resté précaire et entaché de suspicion. On n'en veut pour preuve que l'effort constant des disciplines pour s'affirmer "utiles".

Une dernière caractéristique concerne le recrutement des Facultés de Droit, très différent de celui des Facultés des Lettres par la place plus grande qui y occupent les classes dirigeantes tout au long du XIXème siècle, ce qui leur assure selon Victor Karady, une position stratégique dans l'enseignement supérieur.

"Fortes d'une clientèle autrement plus nombreuse et dotée d'un capital social, culturel et économique plus grand que le public auquel s'adressent les Facultés des lettres, les Facultés de Droit du fait qu'elles encadrent une fraction notable de la classe dirigeante sont par nature un enjeu essentiel dans les conflits entre fractions libérales et conservatrices traditionnalistes et modernistes des classes dominantes" (4).

Les Facultés des Lettres

Au XIXème siècle, les Facultés des Lettres se définissent fondamentalement par relation à l'enseignement secondaire puisque leur fonction sociale première est d'assurer la collation des grades universitaires et l'organisation de baccalauréat.

Les dispositions de Napoléon Ier en ce qui concerne les Facultés des Lettres prévoyaient qu'il y aurait près de chaque lycée, chef-lieu d'une académie une Faculté des lettres composée du professeur de belle lettres du lycée et de deux autres professeurs, le proviseur et le censeur pouvant leur être adjoints. Ces deux fonctionnaires devront être dans ce cas titulaires du grade de Docteur (décision du

3 octobre 1809). Le doyen de la Faculté devait être choisi parmi les trois premiers membres.

Les cours de Faculté sont la suite et le complément des études secondaires. Par le statut de 15 février 1810, chaque Faculté doit s'adjoindre un professeur de philosophe et un professeur d'histoire.

Sous le Second Empire, à la suite de plusieurs réorganisations (1854, 1856, 1859, 1863), les Facultés existantes ou réouvertes comptaient cinq chaines - philosophie, histoire, littérature ancienne, littérature française, littérature étrangère (5).

Les Facultés délivrent les grades de bachelier, de licencié à tout candidat bachelier ayant suivi pendant une année deux cours à la Faculté et ayant composé en français et en latin sur un sujet et dans un temps donné, de docteur enfin aux candidats ayant présenté et soutenu deux thèses (à partir de 1840 une en latin, l'autre en français).

Dans le cadre des Facultés tel qu'il est défini par Napoléon le but visé est la formation sur un modèle unique de médecins, de juristes, de bacheliers disponibles pour les caarières de l'enseignement, de l'administration, de commerce et de l'industrie.

Rien d'étonnant dans ces conditions si l'enseignement des sciences sociales se met en place en France plus tardivement que dans bien des pays d'Europe. Comme l'avait rappelé, dès 1900, Charles Gide, Professeur à la Faculté de Droit de Montpellier "quoique la France, entre tous les pays ait eu les prémices de la science économique avec les Physiocrates et de la sociologie avec Auguste Comte, elle a été à peu près la dernière à organiser l'enseignement des sciences sociales. On peut dire que dans notre pays il ne date que d'hier.

L'explication de cette anomalie doit être cherchée dans ce fait que nos Facultés n'étaient pas organisées en vue de distribuer un enseignement vraiment supérieur, embrassant l'universalité des connaissances humaines mais seulement en vue de préparer les étudiants à certains examens. Aussi est-ce avec raison qu'elles ne portaient point le titre d'Universités. Un enseignement ne pouvait trouver place dans leur sein qu'autant que préalablement une place lui était assignée dans le programme des examens. Or ceci ne dépendait pas des Facultés mais de l'administration supérieure. Un décret ou un arrêté étaient donc indispensables pour donner droit de cité à une science nouvelle.

Voilà pourquoi devant les premiers tiers de ce siècle, il n'y avait pas d'autres sciences sociales enseignées en France que les sciences juridiques (et encore à un point de vue exégétique bien plus que social) dans les Facultés de Droit et l'histoire avec un peu de morale dans les Facultés de lettres..."

Force est en tout cas de souligner l'opposition très nette entre cette conception de l'Université et celle de Wilhelm von Humloldt en Allemagne: culture désintéressée, épanouissement de la personnalité, scission entre Ecole qui communique des connaissances stables et l'Université, lieu de la science et de la recherche, c'est à dire du savoir instable, perpétuellement remis en question, enfin puissance de la Faculté de philosophie dotée à Berlin dès le début d'un grand nombre d'enseignants.

Ainsi entre les Facultés de Droit et les Facultés des lettres les clivages sont nets: les premières sont à finalité professionnelle tandis que les secondes sont plus orientées vers l'acquisition et la transmission de la culture savante. Cette opposition éclaire pour l'essentiel les options offertes aux sciences sociales qui n'y sont implantées et qu'ont été très largement commandées par les finalités professionnelles du Droit et par les références humanistes des Facultés de Lettres.

L'institutionnalisation de l'économie politique dans les Facultés de Droit et de la sociologie dans les Facultés des Lettres s'est accompagnée de toute une série de décalages chronologiques.

II L'institutionnalisation de léconomie politique dans les Facultés de Droit.

Tout d'abord, on doit rappeler que tant du point de vue de la reconnaissance universitaire que du développement scientifique l'institutionnalisation de l'économie politique est plus précoce que celle de la sociologie.

De plus, dans le cas de l'économie politique, c'est le développement scientifique qui précède le développement institutionnel, c'est une science déjà "faite" qui pénètre à l'Université alors que la situation sera inverse avec la sociologie à la fin du XIXème siècle.

Six dates majeures jalonnent le développement de l'enseignement de l'économie dans les Facultés de Droit:

1). une ordonnance du 24 mars 1819 avait divisé la Faculté de Droit de Paris en deux sections" vu disait l'ordonnance l'accroissement du nombre des étudiants" et pour "donner à l'enseignement du Droit les développements dont il est susceptible".
Pour des raisons obscures la chaire économique de la Faculté ne fut pas occupée. La même année était créée par Jean-Baptiste Say la première chaire d'économie, celle du Conservatoire des Arts et Métiers (7).
En 1822 une ordonnance supprimait les chaires nouvelles dont celle d'économie politique.

2). 1864: création effective de la première chaire d'économie politique.
Le décret du 17 Septembre 1864 créant effectivement la première chaire d'économie politique était accompagné du rapport de Victor Duruy dont le texte fut reproduit par le Journal des Economistes (octobre 1864). Duruy y déclare que "l'économie politique est à présent une science complète...Cette science qu'on enseigne partout autour de nous et à laquelle un pays voisin a dû récemment de pouvoir traverser une crise épouvantable pour ses classes ouvrières n'a pas assez de lieux d'enseignement.
Les jeunes gens qui se destinent aux professions dites libérales, ceux qui iront un jour au barreau, à la magistrature, à l'administration et aux fonctions électives comme ceux qu'attendent la grande industrie et le haut commerce ou ceux qui seront simplement propriétaires, tout le public enfin des Facultés des Lettres et des Ecoles de Droit restent privés de cet enseignement que le nombre d'élèves des Facultés des Sciences et des Ecoles de Médecine viendraient aussi chercher. Dans quelques années ces jeunes gens devenus hommes publics ou citoyens considérables par leurs talents, leur position ou leurs fonctions exerceront à des tires divers une sérieuse influence sur les affaires du pays. Selon la classification des sciences cette matière devrait être placée dans les Facultés des Lettres (comme en Allemagne). Mais celle-ci en France n'a pas d'étudiants. La Faculté de Droit de Paris elle compte plus de 2500 élèves (la moitié à elle seule des étudiants en droit de toute la France)".
La chaire fut confiée à Batbie, alors professeur suppléant de Droit administratif.

3). 1877. Transformations de l'économie politique en matière obligatoire d'examen.

Le décret du 26 Mars 1877 introduit une question d'économie politique dans l'examen de deuxième année de licence, ce qui eut pour conséquence l'organisation d'un cours d'économie politique dans toutes les Facultés de Droit. En 1889 l'économie politique fut transférée de deuxième en première année.

4). 1895. Réforme du Doctorat.

Par le décret du 30 avril 1895 était institué un nouveau régime du Doctorat. A côté du doctorat de droit privé, il était désormais possible de préparer un doctorat de droit public et de science économique.

5). 1905. Extension de l'enseignement économique de licence sur deux années.

Le décret du 1er août 1905 étendait sur deux années l'enseignement de l'économie en une de la licence.

6). Création du diplôme d'études supérieures d'économie politique.

La réforme de 1925 a consisté à supprimer les deux doctorats institués trente ans auparavant, et à leur substituer des certificats entre lesquels les étudiants peuvent choisir les deux qui ont leur préférence.

Cette brève présentation chronologique révèle tout d'abord que le développement de l'économie politique est allé de pair avec les poussées libérales (1815-1819-1840-1866-1877).

L'économie politique est en effet au XIXème siècle une matière dangereuse du point de vue politique et religieux dont le sort dépend étroitement de l'évolution des forces économiques et politiques.

Il apparaît en second lieu qu'en dépit de sa progressive insertion dans les Facultés de Droit, l'économie politique a longtemps gardé les caractères d'une discipline auxiliaire, étroitement subordonnée comme telle aux finalités professionnelles de ces établissements.

Si l'on en croit L. Le Van-Lemesle cette situation de discipline auxiliaire éclaire les deux stratégies mises en oeuvre pour surmonter ce handicap:

1). préciser la place et le rôle de l'économie politique dans le cadre d'un enseignement rénové du Droit.

Les Facultés de Droit devront contribuer à façonner le moral de la nation en agissant sur une élite nombreuse qui plus tard par son

instruction, sa fortune, les fonctions dont elle est investie peut exercer sur la population tout entière une autorité qui sera heureuse ou malheureuse suivant les idées dont elle aura été pénétrée elle-même.

2). mettre en place un nouveau cursus et un nouveau mode de recrutement.

Dans le cadre du mouvement de réforme de l'enseignement supérieur de la fin du XIXème siècle, l'un de ses principaux acteurs, Louis Liard, plaide pour l'intégration des Facultés de Droit dans le système qui se met alors en place: (8)

"Les Facultés de Droit sont comme les autres Facultés des établissements d'enseignement supérieur et non des écoles professionnelles. Rien de ce qui appartient à cette partie du savoir humain dont la culture leur a été dévolue ne doit leur demeurer étranger (...).

L'intérêt de la science n'est seul en jeu; il y va aussi d'un sérieux intérèt pour la société. L'enseignement supérieur est aujourd'hui considéré comme une des sources où se forment et d'où découlent les idées générales qui se répandant ensuite de couche en couche finissent par faire partie du moral de la nation. Autant que les autres Facultés, plus que les autres à certains égards, les Facultés de Droit ont une part dans ce travail (...).

Les études d'enseignement supérieur ne sont pas la préparation immédiate et directe à certaines carrières déterminées...Jusqu'ici l'enseignement du droit a été surtout dirigé vers le barreau et la magistrature. Or des 1400 licenciés mis chaque année en circulation par les Facultés de Droit, un tiers à peine s'attache définitivement au barreau ou entre dans la magistrature; beaucoup recherchent les carrières administratives et les carrières publiques. Il n'est pas indifférent qu'il y arrivent mieux préparés que par le passé; ce n'est pas indifférent ni pour eux ni surtout pour l'Etat".

En réalité, l'enseignement des Facultés de Droit s'enrichit alors de l'introduction d'enseignements nouveaux d'histoire du droit, de droit international, de philosophie de droit et l'économie politique. L'idée est que désormais les juristes doivent connaître les faits avant de dire le Droit. Grâce à la réforme du doctorat de 1895 les enseignants d'économie vont cesser d'être des autodidactes.

Toutefois si la professionnalisation n'était pas encore devenue réalité au milieu des années trente, elle était en marche et devait

se réaliser pleinement après la seconde guerre mondiale grâce notamment à la création de la licence puis du doctorat de sciences économiques.

Ces deux stratégies ne doivent pas faire oublier une revendication formulée dès la fin du XIXème siècle, visant à la création d'une Faculté des Sciences Sociales qui permettrait de surmonter les difficultés engendrées par la séparation des Facultés des Lettres et des Facultés de Droit. En 1937, Gaëtan Pirou précisait qu'on devrait étudier dans cette Faculté des Sciences Sociales les diverses disciplines relatives à l'homme en société: ethnologie, démographie, sociologie, morale, politique, religion. Une place y sera faite à la sociologie générale.

Les disciplines de Durkheim auront la parole pour exposer leurs thèses mais ceux de Pareto pourront également se faire entendre et également ceux de Tarde et de Le Play. Parmi les sciences particulières, l'économie politique occupera une place de choix (9).

III L'institutionnalisation de la sociologie dans les Facultés des Lettres.

Faut-il ajouter que cette revendication constamment reprise restera lettre morte jusqu'à nos jours?

Quant à la sociologie, son entrée à l'Université se fait dans la décennie 1880-1890.

C'est en effet en 1887 que fut créé le premier cours de science sociale à Bordeaux pour Durkheim. Cette décision ne se réduit pas à un caprice du Prince - mais s'inscrit dans une politique de réforme universitaire visant à faire des Facultés des centres de formation supérieure avec une activité de recherche.

Dans un premier temps on examinera les diverses composantes notamment épistémologique et institutionnelle du statut de la sociologie et du sociologue qui ressortent des textes rédigés par Durkheim à la suite de son séjour en Allemagne dont on sait l'importance dans la formation de ses idées. Le sociologue y est défini comme un savant qui élabore une science nouvelle à l'Université et comme un éducateur qui en assure ensuite la diffusion dans la population afin de renforcer la cohésion sociale du pays (10).

Après la seconde guerre mondiale, cette définition du rôle et des activités du sociologue a été reconduite lors des débats sur l'enseignement supérieur et en référence aux Etats-Unis. L'expansion

de la discipline particulièrement nette à la fin des années cinquante et au début des années soixante n'a pas chargé les données du problème. Le sociologue-Professeur-Fonctionnaire n'est toujours pas confronté à la moindre "demande sociale" aussi bien en amont qu'à l'aval.

Il en résulte - et ce sera le troisième point - que les problèmes majeurs auxquels est confronté le sociologue sont restés sans solution.

On sait que E. Durkheim s'était vu confier par Louis Liard une mission d'enquête sur les méthodes et le contenu de l'enseignement philosophique et sur l'état des sciences sociales en Allemagne (11).

Sans entrer dans le débat des spécialistes pour savoir quelle est l'importance réelle du voyage en Allemagne dans la formation de la sociologie durkheimienne, il est clair que Durkheim a fait Outre-Rhin deux "découvertes" majeures: la première c'est que l'Université allemande est une université vivante:

> Quand on analyse dans le détail une Université allemande il est facile d'y trouver beaucoup à critiquer, l'organisation générale est loin d'être parfaite, la pléthore des cours n'est que richesse apparente, l'enseignement pour la philosophie du moins est trop général. Les étudiants ont trop le souci de leurs examens. Mais si au lieu de se mettre de près pour l'observer avic minutie, on se recule un peu pour en prendre une vue d'ensemble, un fait frappe aussitôt: toute Université si petite quelle soit est quelque chose de vivant. Voilà qui impose silence à tous les raisonnements et à toutes les analyses car la vie est ce qu'il y a de plus rare au monde; il n'est rien de plus inimitable (12).

En second lieu, cette université produit des connaissances. Durkheim découvre ainsi une méthodologie historique et réaliste mise en oeuvre par la Volkswirtschaft ce qui lui permet de prendre ses distances par rapport aux économistes du groupe de Paris" (Leroy-Beaulieu, Levasseur, Molinièri et Guyot).

Durkheim s'initie également à la philosophie du Droit allemande (Jellinek-Jhering) et à sa conception du Droit entendu comme l'ensemble des conditions d'existence de la société assurées au moyen d'une contrainte extérieure par la force dont dispose l"Etat". Enfin, il se rend à Leipzig pour suivre les enseignants de Wundt dont l'Ethique publiée en 1887 le ravit.

Toutes ces rencontres conduisent Durkheim à la solution des problèmes qui le hantent depuis longtemps. La désunion nationale en France ne peut être surmontée que par l'invention et l'enseignement d'une nouvelle morale.

Il n'est donc pas interdit de penser avec B. Lacroix (13) que dès 1887 l'objet de la sociologie soit clairement circonscrit "la sociologie sera l'étude de la société et aussi des groupes qui la composent: elle cherchera le principe des transformations qui affectent celle-ci et ceux là dans le secteur du milieu qui les enveloppe et dont ils dépendent.

S'il n'est pas de société sans unité et cohésion, il en résulte que les principes unificateurs qui la rendent possible doivent faire l'objet d'une science nouvelle.

> Il n a dans toute société un certain nombre d'idées et de sentiments que les générations se passent les uns aux autres et qui assurent à la fois l'unité et la contrainte de la vie collective. Telles sont les légendes populaires, les croyances politiques, le language. Tous ces phénomènes sont d'ordre psychologique mais ils ne relèvent pas de la psychologie puisqu'ils dépassent suffisamment l'individu. Ils doivent être l'objet d'une science spéciale chargée de les décrire et d'en chercher les conditions: on pourrait l'appeler psychologie sociale. C'est la Völkerpsychologie des Allemands (14).

Cette science nouvelle doit être élaborée à l'Université pour être ensuite diffusée dans la société, afin "d'éclairer" l'opinion publique (15).

Rappelons aussi comment E. Durkheim définissait dans sa leçon d'ouverture du cours de science sociale les finalités et le programme de la sociologie naissante:

> Nous vivons dans un pays qui ne reconnaît d'autre maître que l'opinion. Pour que ce maître ne devienne pas au despote intelligent il est nécessaire de l'éclairer et comment sinon par la science. Sous l'influence de causes qu'il serait trop long d'analyser ici l'esprit de collectivité s'est affaibli chez nous (...). Il faut réagir et de toutes nos forces contre cette tendance disperée. Il faut que notre société reprenne conscience de son unité organique, que l'industrie sente cette masse sociale qui l'enveloppe et le pénètre, qu'il la sente toujours présente et agissante et que ce sentiment règle toujours sa conduite. Eh bien Messieurs, je crois que la sociologie est plus que toute autre science en état de restaurer ces idées. C'est elle qui fera comprendre à l'individu ce qu'est la société, comme elle le complète et combien il est peu de choses réduit à ses seules forces. Elle lui apprendra qu'il n'est pas un empire au sein d'un autre empire mais l'organe d'un organisme et lui montrera tout ce qu'il y a de beau à s'acquitter de son rôle d'organe. Elle lui fera sentir qu'il n'y a aucune diminution à être solidaire d'autrui et à en dépendre, à ne pas s'appartenir à soi-même. Sans doute ces idées ne deviennent vraiment efficaces que si elles se répandent dans les couches profondes de la population mais pour cela il faut

d'abord que nous les élaborions scientifiquement à l'Université.

Ce modèle universitaire défini par Durkheim se perpétue d'autant mieux après la seconde guerre mondiale que dans une première période qui va de 1945 à 1958 l'enseignement de la sociologie conserve en France le caractère très marginal qu'il avait avant avec ses quatre chaires d'enseignement universitaire (à Paris, Strasbourg et Bordeaux). Subordonnée à la philosophie, la sociologie existe sous la forme du Certificat de Morale et Sociologie crée en 1920 comme expression de l'ambition durkheimienne de fonder la morale sur la sociologie. En 1923, la sociologie avait été également introduite comme matière à option dans les classes de philosophie. Il faut toutefois ajouter qu'il existait un certificat de sociologie pouvant entrer dans le cadre d'une licence libre.

Les remarques formulées par R. Aron en 1937 sur l'enseignement de la sociologie (16) n'ont rien perdu de leur valeur pour la période d'une dizaine d'années qui suit la Libération: l'étudiant "lit ou étudie indirectement des classiques français de la sociologie (A. Comte, Tarde, Durkheim, Lévy-Bruhl, Simiand); il acquiert quelques notions d'histoire des doctrines (en particulier les doctrines sociales du XIXème siècle); en sociologie domestique et religieuse il apprend quelques unes des interprétations sociologiques les plus célèbres.Dans le meilleur des cas, il s'initie de plus aux problèmes de la sociologie politique et économique. En tout cas comme au lycée, it étudie des problèmes philosophiques et méthodologiques de la sociologie. Cette initiation philosophique aux questions sociales est le but et le résultat d'un enseignement sociologique isolé des autres sciences sociales".

Avec la création de la licence de soiologie (2 avril 1958) et celle du doctorat de troisième cycle (19 avril 1958), le changement de dénomination des Facultés des lettres qui deviennent "Facultés des lettres et Sciences Humaines" en juillet 1958 marque le début d'une nouvelle période pour ce qui est de l'enseignement de la sociologie. La solution de type universitaire qui s'est alors imposée est le point d'aboutissement d'une stratégie d'alliance esquissée lors du premier Colloque de Caen en 1956 entre sciences exactes et sciences sociales et humaines contre les disciplines dominantes - droit et médecine.

Dans son intervention consacrée aux sciences humaines, la Direction de l'enseignement supérieur, G. Berger ne ménageait pas ses

critiques à l'égard de la production américaine de sciences sociales alors rayonnante:

> Contrairement à ce que l'on pense parfois il n'y a pas non plus à l'étranger tellement de choses valables. Les Américains par exemple sont fréquemment déçus par le faible rendement de leurs nombreux travaux (17).

Comment ne pas rapprocher ce jugement des réserves ou des critiques que les universitaires français ne manquaient pas d'adresser à l'Université allemande à la fin du XIXème siècle alors même qu'ils s'y référaient constamment dans les débats sur l'enseignement supérieur?

G. Berger justifiait en ces termes la solution de type universitaire qu'il retenait pour les sciences sociales:

> L'Université a d'ailleurs une réputation d'objectivité et d'impartialité qui lui donne la possibilité d'effectuer des enquêtes là où seraient mal accueillis et parfois tout à fait impossibles de recherches entreprises par les organismes ayant des objectifs personnels (18).

A partir du début des années soixante, l'offre d'enseignement va créer une demande (il est désormais possible de "faire de la sociologie".

La création de la licence aboutit bientôt à une "fuite en avant", le doctorat de troisième cycle devenant le débouché "normal des études de sociologie.

Si les problèmes épistémologiques concernant l'objet et les fondements de la sociologie ont était maintes fois soulevés on n'a peut être pas suffisamment souligné le poids de la dispersion géographique et institutionnelle de la discipline sur les orientations intellectuels et idéologiques.

Si la sociologie est alors présente aussi bien à l'Institut National d'Etudes Démographiques, à l'Institut National de la Statistique et des Etudes Economiques, à la VIème section de l'Ecole Pratique des Hautes Etudes ou encore à l'Institut des Sciences Economiques Appliquées, faut-il s'étonner si comme le note R. Boudon (19) aucun paradigme ne paraît avoir réussi à s'imposer avec une évidence suffisamment convaincante pour fonder une tradition de recherche indépendante. A fortiori aucun n'a été en mesure de dominer la sociologie.

De son côté, F. Bourricaud faisait remarquer dès 1954 que "le caractère indéterminé de notre discipline ne tient pas seulement aux imperfections de méthode. Il tient à ce que l'activité humaine est

susceptible d'être étudiée à plus d'un point de vue et que ces points de vue ne sont pas rigoureusement comparables" (20).

En réalité la sociologie implantée au CNRS grâce au Centre d'Etudes Sociologiques et dans les Facultés des lettres et Sciences Humaines reste à la périphérie du système d'enseignement supérieur français puisqu'elle ne se pénètre pratiquement pas au coeur du système c'est à dire dans les Grandes Ecoles. Occupant une place réduite dans les Facultés, elles-mêmes coupées du CNRS, la sociologie reste à la lisière de la vie économique, sociale, politique du pays.

En effet, tout au moins jusqu'à la fin des années cinquante, l'Etat et les administratifs n'ont pas vraiment sollicité les sociologues en tant que tels, les techniciens de l'Institut National de la Statistique et des Etudes Economiques, les chercheurs de l'Institut National d'Etudes Démographiques sont devenus de fait des sociologues.

Toute une pratique sociale a alors tendu à devenir sociologique et toute une sociologie s'est efforcée de devenir pratique mais sans beaucoup de lien entre ces deux pôles.

Les études démographiques, les études économiques, les enquêtes d'opinion de l'Institut Français d'Opinion Publique manifestent à leur manière le rayonnement de la sociologie sur les autres disciplines de sciences sociales mais dans le même temps l'Université ne produit pas de sociologues et la pratique ne demande pas de sociologues. Il en résulte une séparation entre les universitaires d'un côté et les praticiens de l'autre, entre la recherche "empirique" et la sociologie "spéculative".

Dans ces conditions nombre de problèmes allaient devoir rester en suspens tant en ce qui concerne l'enseignement que la recherche ou encore la formation professionnelle. Le paradoxe que nous voudrions avancer ici pourrait s'énoncer de la manière suivante: la permanence des problèmes restés en suspens, n'est elle pas la contrepartie de l'institutionnalisation de la sociologie comme discipline académique?

IV. Les problèmes en suspens.

Dans le domaine de l'enseignement, l'absence de formation cohérente et satisfaisante est évidente. Avant la création de la licence de sociologie, les traits dominants de l'enseignement universitaire sont l'aspect rhétorique de la formation, l'absence d'initiation aux techniques et à la méthodologie de l'enquête, la séparation des

Facultés des lettres et des Facultés de Droit. La licence de sociologie en 1958 allait elle remédier à ces défauts? En fait elle se présente comme un replâtrage puisqu'elle se contente de rapprocher des certificats déjà existants, le certificat de sociologie générale et le certificat d'économie politique auxquels on adjoint le certificat de psychologie sociale et un certificat à option.

L'enseignement reste tout aussi rhétorique, spéculatif et magistral même si la mise en place des travaux dirigés s'inscrit dans la perspective d'une formation d'ordre méthodologique.

De même le doctorat de IIIème cycle est créé sans moyens pour le faire réussir et sans initiation à la pratique de la recherche. Il faudra attendre 1963 pour que les premiers éléments de formation méthodologique soient organisés dans le cadre de la VIème section de l'Ecole Pratique des Hautes Etudes, à l'intention et à l'usage des étudiants parisiens.

Pour ce qui est de la formation et de la pratique professionnelles, force est de constater que cette question n'a jamais été et n'est toujours pas réglée. La sociologie n'est pas une profession organisée ou réglée par une institution ou une haute autorité. Née avec la création de la licence et du troisième cycle la question des débouchés a d'abord été évacuée parce que les responsables académiques et administratifs pensaient que la croissance du système éducatif permettrait d'absorber les diplômés de l'enseignement supérieur.

La fuite en avant a ainsi été organisée par les "patrons" universitaires qui voyaient dans la poussée des effectifs étudiants un moyen de renforcer leur pouvoir et leur prestige. L'idée d'une professionnalisation possible hors de l'Université a été refusée à la fois par les Professeurs et par les "utilisateurs" qui quand ils n'étaient pas hostiles aux sciences sociales ont plutôt fait appel aux spécialistes des "relations humaines", c'est à dire aux psychologues et aux psychosociologues. Comment ne pas rappeler l'échec du projet de création d'un diplôme d'expert sociologue présenté par J.R. Tréanton en 1964 devant la Société Française de Sociologie?

Les premières lignes de l'appel lancé en 1981 "pour le développement d'une Association professionnelle des sociologues, pour le reconnaissance la promotion et la défense du métier de sociologue" n'ont donc rien d'étonnant.

"A la différence d'autres professions nées des sciences socioles, le métier de sociologue n'est ni reconnu, ni organisé en France (...)"
En réalité des raisons d'ordre culturel et idéologique éclairent jusqu'à un certain point l'aspect permanent de ces problèmes. Tout d'abord on sait que la sociologie n'a jamais été intégrée dans l'enseignement secondaire, la place essentielle étant réservée aux humanités classiques et aux mathématiques. Quant à la création d'un enseignement de sciences économiques et sociales, elle s'est faite à partir du milieu des années soixante et par le biais de l'histoire économique et sociale.

En second lieu on ne doit pas oublier que la formation de la plupart des dirigeants de la société et de l'économie par le Droit ou les mathématiques ne les préparait guère à solliciter les sociologues en tant que tels.

Il y a lieu de souligner également le poids de la séparation entre formation sociologique et formation politique. Les Durkheimiens avaient tenté de surmonter cette opposition en introduisant la sociologie dans l'enseignement primaire, mais ils s'étaient heurtés à de fortes oppositions notamment des milieux catholiques.

Par ailleurs, l'action des partis politiques repose souvent en France sur des fondements théoriques. C'est ainsi que le marxisme a tenu lieu de sociologie pour le parti communiste. Par ailleurs l'emprise du marxisme sur de larges secteurs à l'intelligentsia a constitué un obstacle très puissant à l'essor de la sociologie dans les années cinquante. Qu'il suffise de mentionner les témoignages et les analyses d'H.Lefevbre (21) ou d'E.Morin (22) sur cette question!

Au total, l'histoire récente de la sociologie en France révèle l'existence des constantes pour ne pas dire d'invariants culturels.

Tout d'abord, en dépit de son développement et des succès (23) la sociologie reste en marge ou à la périphérie des institutions notamment l'enseignement supérieur. Sans aller jusqu'à reprendre l'idée émise par A. Touraine d'un "refus" systématique et conscient des sciences sociales, lil est clair que les dirigeants de la société française n'ont pas été mécontents de voir la sociologie "cantonnée" principalement au CNRS, plus précisément au Centre d'Etudes Sociologiques!

Seconde constante qui peut être repérée: le discours sur l'utilité sociale des sciences sociales et surtout de la sociologie

définie en terme d'aide à la décision ou de prévision, voire de prospective. Ce discours s'inscrit dans un courant d'idées très ancien remontant au moins aux Lumières et qui se développe parallèlement aux processus de "rationalisation" et de la "laïcisation" de la société civile que l'on peut suivre dans nos sociétés depuis la fin du XVIIIème siècle.

Après la seconde guerre mondiale, ce discours prend de nouvelles formes. Il vise alors à unifier un champ d'action et de relation entre deux groupes d'acteurs, d'un côté les chercheurs en sciences sociales et de l'autre des gestionnaires et les utilisateurs de la recherche.

Or ce discours est idéologique parce qu'il masque aux yeux des acteurs qui le diffusent et d'une manière plus générale aux yeux de l'opinion les autres dimensions essentielles des sciences sociales qui sont de l'ordre des représentations sociales (24). Or au cours des trente dernières années des changements considérables ont affecté en liaison avec l'essor des sciences sociales nos perceptions et nos représentations du social et par la même les conditions et les modalités de l'action sociale s'il est vrai comme le rappelle P. Bourdieu qu'il n'y a pas d'action possible à quelque niveau que ce soit sans une certaine représentation sociale de la société. Un de ces changements n'est-il pas ce qu'on pourrait appeler la "sociologisation" progressive de la société qui renvoie tout à la fois à la destruction définitive des garants métasociaux de l'ordre social et à la prise de conscience progressive que la société est fille de ses actes?

Enfin les relations entre les sciences sociales et leurs interlocuteurs du monde économique ou politique ont été constamment placés sous le signe des malentendus, de l'ambiguité, voire du conflit, difficultés qui tiennent autant à des différences d'attentes et de language qu'au statut mal défini du sociologue dans la société française qui est tant à la fois fonctionnaire, expert et prohète.

En conclusion générale, deux points méritent d'être soulignés en raison de leur significtation générale.

Tout d'abord les différences de contextes, intellectuel et social, ou disons d'environnement et de recrutement éclairent jusqu'à un certain point les modalités d'institutionnalisation des sciences sociales dans les Facultés de Droit et dans les Facultés les Lettres. C'est aisi qu'on peut se demander si en dépît de ses déclarations

(25) le projet de Durkheim aurait pu voir le jour dans le cadre de Facultés à finalités professionnelles fortes. Il est permis d'en douter.

En deuxième lieu, l'insertion de l'économie politique dans les Facultés de Droit apparaît comme le comblement d'un vide, la science existant déjà avant son institutionnalisation à l'Université. Au contraire, pour ce qui est de la sociologie, le changement décisif s'est opéré d'abord et avant tout grâce à l'enseignement avant de se réaliser dans les écrits méthodologiques et dans les recherches. Ainsi l'introduction de la sociologie dans les Facultés des Lettres a très largement devancé l'organisation de la discipline, qui n'aurait pas été toutefois ce qu'elle a été sous "l'oeuvre fondatrice" de Durkheim.

Notes.

(1) Ce choix si arbitraire qu'il apparaisse se justifie par le caractère "exemplaire" de ces deux disciplines du point de vue du processus d'institutionnalisation.

(2) L'une des plus récentes est celle de Pierre Bourdieu, Homo academicus, Les Editions de Minuit, 1984, 302p.

(3) Lucette Levan-Lemesle, la Faculté de Droit de Paris et l'introduction de l'économie politique dans son enseignement 1864-1878, n° spécial de Historical Reflections sur l'histoire de l'enseignement: The making of Frenchman: current directions in the history of education in France
D.N. Baker et P.J. Harrigan editors.

(4) Victor Karady, Durkheim, les sciences sociales et l'Université: bilan du'un semi-échec, Revue française de sociologie, Vol. XVII, n° 2, avril-juin 1976.

(5) Seule l'Université de Paris eut neuf Professeurs: histoire ancienne, moderne, philosophie, histoire de la philosophie, littérature grecque, éloquence latine, poèsie latine, géographie, éloquence française, poésie française. Dix à partir de 1830 à la suite de la création de la chaire de littérature étrangère.

(6) Charles Gide, l'Enseignement des sciences sociales en France. Enseignement supérieur dans le Premier Congrès de l'enseignement des sciences sociales, Paris, Alcan, 1901, pp. 76 et 84.

(7) Appartenant à la société protestante libérale, comme Madame de Staël et Benjamin Constant, Jean-Baptiste Say est surtout connu outre son activité d'enseignant, pour avoir présenté de manière systématique la pensée de Turgot et d'Adam Smith. Il partagera les vues des "idéologues" pour qui l'économie politique est un moyen parmi d'autres de réaliser le bonheur d'une société fondée sur les droits de l'Homme; l'aisance obtenue grâce à la connaissance des lois économiques devant rendre les hommes plus "vertueux".

(8) Louis Liard, La réforme de la licence de Droit, Revue internationale de l'enseignement, 1889, p.18.

(9) Gaëtan Priou, L'enseignement économique en France, Cinquantenaire de la Revue d'Economie Politique, 1937.
(10) Emile Durkheim, Cours de science sociale, Leçon d'ouverture.
(11) Au terme de son séjour, Durkheim rédigea les deux articles suivants:
La philosophie dans les universités allemandes, Revue internationale de l'enseignement, 1887, p.313-338 et p. 423-440.
La science positive de la morale en Allemagne, Revue philosophique, Tome XXIV, 1887, p.33-58, p.115-142, p.275-284.
(12) in Durkheim (E), La philosophie dans les universités allemandes, art. cit.
(13) Lacroix (B), Durkheim et le politique, Presses de la Fondation Nationale des Sciences Politiques, Presses de l'Université de Montréal, 1981, 324 p.
(14) Conclusion de l'article consacré à la philosophie en Allemagne, Revue Internationale de l'enseignement, 1887.
(15) Cours de science sociale, leçon d'ouverture.
(16) Aron (R), La sociologie dans "Les sciences sociales en France, Enseignement et recherche" Préface de C. Bouglé, directeur de l'Ecole Normale Supérieure, Paris, Paul Hartmann, 1937.
(17) Les sciences humaines par Gaston Berger, Colloque de Caen, 1956, Comptes rendus parus dans les Cahiers de la République, 1957.
(18) Idem.
(19) Boudon (r), La crise de la sociologie: questions d'épistémologie sociologique, Genève, 1971.
(20) Bourricaud (F.), Indétermination de la sociologie, Critique, 1954, n°10, n° 973-983.
(21) Lefebvre (h), La somme et le reste, Paris, La Nef, 1959.
(22) Morin (E), Autocritique, Paris, Le Seuil, 1975.
(23) "La liste des enseignants universitaires des différents grades comporte aujourd'hui plus de 400 noms alors qu'en 1950 le nombre des chaires de sociologie se comptait sur les doigts d'une main. Au CNRS la section de sociologie-démographie dont les effectifs ont doublé en quatre ans, entre 1976 et 1980 rassemble 340 chercheurs en 1982 alors qu'elle en avait 18 en 1949". Ces lignes sont extraites de la toute récente enquête que la société française de sociologie a consacrée à l'enseignement de la sociologie.
(24) Par sa double composante - imaginaire et conceptuelle - la notion de représentation caractérise le type de savoir produit et diffusé par les sciences sociales. Etant des enjeux de lutte pour la définition même de la réalité sociale, les usages des representations sociales ne sauraient faire l'objet d'un quelconque contrôle social ou politique.
(25) Conclusion à la leçon introductive de Cours de science sociale à Bordeaux en 1888:

> "On a, vous le savez, contesté que ce cours fût à sa place dans une Faculté des lettres et plusieurs reprises la Faculté de Droit l'a revendiqué pour elle (...). Il me semble seulement que cet enseignement n'est pas déplacé dans une Faculté des lettres puisqu'il s'adresse à de nombreux étudiants de cette Faculté. Je reconnais d'ailleurs qu'il ne le serait pas davantage dans une école de droit; puisque de l'aveu de tout le monde il est utile aux étudiants de droit. Je ne revendiquerais pas pour nous la jouissance exclusive de la sociologie. Je souhaite au contraire que des cours analogues soient fondés ailleurs, car il ne peut y avoir que profits pour la science à ce que les mêmes problèmes soient étudiés à des points de vue

différents. Je demande seulement qu'on nous traite avec le même libéralisme".

L'ETUDE DES RELATIONS COLLECTIVES DU TRAVAIL ET LES SPECIALISTES DES SCIENCE SOCIALES: EXAMEN D'UNE CONTROVERSE

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summary
This article is concerned with the controversy between american industrial sociologists and labour economists over industrial relations that took place just after the end of World War II. The theoretical aspects of this argument, where the position of both academic specialties on the industrial expertise market is at stake, refer to incompatible conceptual schemes. The main features of these are reviewed, as well as the critics they endure. It is finanlly argued that this debate led to industrial sociologists neglecting the study of collective bargaining and reappraising the relations between industry and its social environment.

Aux Etats-Unis, les conditions de production de la deuxième guerre mondiale ont considérablement favorisé le développement des recherches sociales dans l'industrie. Les psychologues sociaux industriels et les sociologues industriels en ont profité pour justifier leur existence, en prouvant leur "utilité". Forts du statut d'experts qu'ils se donnent à ce moment, ces chercheurs offrent leurs services à l'administration et aux entreprises. Ils se lancent dans des campagnes de communication, notamment destinées à vendre des actions de guerre, ou dans l'étude des méthodes de propagande et des civilisations ennemies. Dans l'industrie, ils s'intéressent aux méthodes de sélection du personnel et participent aux recherches que le War Production Board commande sur l'absentéisme, ainsi qu'aux programmes de formations destinés aux superviseurs mis sur pieds par la War Manpower Commission.

L'engouement pour les "relations humaines" au cours de cette période n'est toutefois rien à côté du délire qui s'empare des gestionnaires immédiatement après la fin du conflit. Comme le souligne un article du New York Times paru à l'époque, les recherches menées pendant la guerre ont stimulé l'intérêt du monde des affaires et de l'industrie à un point tel que, pendant la période de

reconversion, ses représentants espèrent que les sciences sociales vont leur permettre de trouver des solutions aux problèmes de la gestion de la force de travail. Immédiatement après la guerre, la vague des "relations humaines" déferle sur l'Amérique, portée par les propos des hommes d'affaires ou des porte-parole des représentants d'organisations patronales. L'administration fédérale participe au mouvement, du fait de la multiplication des interventions gouvernementales dans les affaires de l'emploi, de la négociation collective et de l'économie en général. Dans la mesure où le gouvernement se porte garant des opportunités d'emploi offertes aux américains (Employment Act) et se donne la possibilité d'interférer davantage dans les relations industrielles (Taft-Hartley Act), il contribue également au développement des recherches sur ces questions. Bref, dès la fin de la seconde guerre mondiale, les fonds consacrés par les entreprises et les agences gouvernementales à la recherche sociale croissent très rapidement, pour atteindre des montants considérables. Au cours des mêmes années, les conflits sociaux sont nombreux. Dans ce contexte, les psychologues sociaux industriels, les sociologues industriels et les économistes du travail vont se trouver concurrents pour la conquête de ce marchée de l'expertise. Cette concurrence entre spécialistes des sciences sociales se caractérise aussi par des controverses théoriques, notamment dans le domaine de l'étude des relations collectives du travail. Ce texte porte sur les débats qui ont lieu, à ce propos, entre les économistes du travail et les sociologues industriels.

Les tensions qui vont se produire entre les économistes et les sociologues autour de la question des relations collectives du travail se profilent dès la première rencontre de l'Industrial Relations Research Association, qui a lieu en 1948. Cette association, créée à la fin de 1947, est une émanation de l'American Economic Association et est dominée par les économistes, même si tous ses membres sont loin de l'être.[1]

Dans son allocution d'ouverture, le président Witte met en évidence à la fois le développement récent du domaine[2] et l'importance de la tradition de l'économie dans ce champ. De ce fait, quand il s'agira, lors de la cinquième séance de ce premier congrès, d'examiner "Le rôle des différentes disciplines dans la recherche sur les relations industrielles", l'économie ne fera pas partie de ces

disciplines, puisqu'elle est centrale. La psychologie, la sociologie, la science politique et le droit seront par contre évoqués.

Witte estime que les sociologues doivent jouer un rôle dans le développement du champ, mais ne sont susceptibles que d'y apporter une contribution limitée. Selon lui, les sociologues devraient se charger d'étudier les organisations syndicales, et non les entreprises. Comme Witte, Milton Derber relativise les apports et les originalités des disciplines autres que l'économie dans le domaine des relations industrielles. En fait, sur cette question, les sociologues sont présentés comme des concurrents. Au début des années 1950, Kerr et Fisher le soulignent clairement: "Les économistes ont assisté à la croissance d'un concurrent nouveau et vigoureux, armé d'une conception différente de la nature de l'homme et d'une vision différente du bien-être humain."[3]

Les controverses entre les économistes et les sociologues industriels à propos des relations collectives du travail portent en fait sur la validité de l'application dans ce domaine du modèle utilisé par Mayo et ses collaborateurs depuis la fin des années 1930 pour rendre compte de l'entreprise, tel qu'il est alors défendu par William F. Whyte ou George C. Homans. A cette approche, les économistes vont en opposer une autre. Pour aborder cette question, nous partons du débat organisé par l'Industrial and Labor Relations Review en 1949[4], mais il est clair que les démarches qui s'opposent à cette occasion ne sont pas uniquement celles du sociologue Whyte et de l'économiste John Dunlop, les principaux protagonistes de cette rencontre. Ainsi, Gardner et Homans adoptent-ils des positions identiques à celles Whyte, et Yoder, Kerr ou Harbison des positions proches de celles de Dunlop.[5]

L'approche des sociologues industriels.

Whyte aborde l'étude des relations collectives par l'intermédiaire du concept de système social tel que l'a défini Lawrence J. Henderson et tel que l'ont appliqué les collaborateurs de Mayo.[6] Pour lui, l'entreprise comme le syndicat sont des systèmes sociaux, reliés et interdépendants, constitués par les membres de ces organisations et leurs relations.[7] Cela signifie donc que:

1. Un changement dans une partie d'un système social affecte toutes les autres parties. Et, dans le cas de la relation entre deux systèmes, qu'un changement dans un des systèmes est susceptible

d'affecter l'autre système. Comme les deux systèmes "se connectent en de nombreux points, un changement dans une partie du système tend non seulement à affecter les autres parties de ce système, mais aussi le système qui lui est connecté."
2. Ces changements risquent de remettre en question l'équilibre du système, c'est-à-dire de provoquer une crise de l'un ou de l'autre, ou des deux systèmes.[8]

Conformément à cette approche, les actions et les sentiments sont les points à expliquer: pourquoi les travailleurs accroissent-ils (ou réduisent-ils) leur productivité? Pourquoi restent-ils au travail, ou partent-ils en grève? Pourquoi expriment-ils des sentiments hostiles (ou favorables) à la directions?[9] Pour expliquer cela, dit Whyte, l'analyse des interactions est la meilleur méthode. L'explication porte donc aussi bien sur le "négatif" que sur le "positif", sur les décroissances comme sur les croissances; la question n'est plus seulement de savoir comment augmenter la productivité ou améliorer les relations entre les individus. Présenté comme un modèle sociologique, le modèle de Henderson est ici décrit sous un jour qui met ses ambitions heuristiques en évidence plutôt que comme un instrument de gestion des écarts à la norme.

Whyte emploie cette approche pour traiter des relations entre les travailleurs, les employeurs et les syndicats au niveau de l'entreprise. Et il ajoute que s'il se borne à examiner ce qui se passe dans de petites organisations, c'est pour des raisons pratiques et scientifiques. La raison scientifique étant qu'on ne pourrait examiner directement et de près que les relations qui constituent les petites organisations. Whyte précise que cette vision des choses n'est pas sans inconvénient, mais la considère néanmoins comme celle qu'il convient de privilégier. Il s'en explique de la manière suivante. "N'allons-nous par en déformer l'image si nous étudions l'usine comme une unité isolée? Plus encore, comme l'usine est sujette aux influences de l'environnement social de la communauté dans laquelle elle se trouve et aussi aux pressions de l'environnement économique, n'allons-nous pas en donner une image déformée si nous traitons l'usine isolément? La réponse, bien sûr, est 'oui' dans les deux cas. Nous devons avoir un moyen de traiter les influences de l'extérieur de l'usine. Mais en même temps, nous ne traitons pas des influences en général. Nous devons les étudier au point de contact; où elles pénètrent réellement l'usine."[10] On ne peut pas mieux

exprimer les traits du modèle du groupe de Harvard (formé par Mayo et ses collaborateurs).[11]

Comment Whyte propose-t-il de résoudre ce problème? En prenant les influences extérieures comme données (given). L'impact des contraintes (ou des degrés de liberté) provenant du fait que le système soical étudié s'intègre dans une organisation plus vaste est important mais est considéré comme une variable indépendante et non étudiée; ce qui est important, dit Whyte, c'est de voir comment les influences extérieures sont reflétées dans le comportement des gens étudiés. Pour ce qui est des environnements économiques et sociaux, il en va de même.

Les deux systèmes sociaux ne sont donc considérés par les tenants de cette approche qu'à l'endroit où leurs limites se superposent: l'entreprise. De plus, conformément aux enseignements du modèle de Henderson, Whyte considère celle-ci comme une entité relativement isolée, et ne prend en considération ses rapports avec son environnement qu'en tant qu'ils se manifestent dans le comportement des individus, c'est-à-dire dans les actions, les sentiments ou les interactions.

Dunlop se dit en désaccord avec l'approche de Whyte sur plusieurs points. Il précise d'ailleurs d'emblée que le modèle présenté par son collègue sociologue n'est, selon lui, pas à même de rendre compte des relations collectives. La principale critique que Dunlop fait à Whyte est la suivante: "Le système de communications dans une entreprise ou dans un syndicat est en soi très largement un produit d'autres caractéristiques de l'organisation et de son environnement." Ce système doit être expliqué et ne peut être pris comme donné. L'influence de l'environnement sur les sytèmes de communication, si elle est structurante et réfléchie dans les systèmes de communication internes, doit être rendue explicite "plutôt qu'être cachée dans les 'relations humaines' à l'oeuvre dans les organisations."[12]

En dépit de ces critiques, Dunlop n'estime pas que l'analyse de Whyte soit à rejeter complètement; elle doit intervenir dans toute explication du comportement industriel, mais de manière mineure. Elle est en effet à même, d'après lui, de rendre compte du comportement des travailleurs dans l'entreprise, mais non de l'ensemble des relations industrielles. Dunlop soutient, plus précisément, que le modèle de Whyte est incapable d'appréhender le processus de négociation collective, ainsi que l'organisation de la direction des

entreprises et l'organisation syndicale. Et telle est l'ambition de l'approche qu'il propose.

L'approche des économistes du travail.

Pour Dunlop, la meilleure manière d'expliquer la négociation collective est celle qui considère l'interaction et la rencontre des organisations patronales et syndicales dans un contexte économique, technologique et social défini de manière précise. Cette rencontre peut prendre des formes diverses. Les organisations en présence peuvent entretenir une relation de conflit ouvert, une relation de conflit latent ou encore une relation de coopération. Les organisations sont conçues comme des "systèmes sociaux qui existent indépendamment des individus qui composent le groupe qui les constitue."[13]

Quand il parle d'environnement, Dunlop se réfère à l'ensemble du contexte dans lequel les organisations se rencontrent. Les quatre aspects principaux de ce contexte, qui en compte beaucoup, sont, pour lui, les suivants.

1. Les conditions technologiques et physiques de la communauté considérée;
2. les conditions du marché du travail sur lequel l'entreprise se fournit en main-d'oeuvre, et les conditions du marché sur lequel les produits de l'entreprise sont vendus;
3. les "installations" de relations industrielles des parties en présence, y compris les relations entre le syndicat et les autres syndicats, et la direction et les autres directions, dans le domaine des politiques de relations de travail;
4. les idées, croyances et jugements de valeur dans la communauté et dans l'industrie au sein desquelles les parties négocient.[14]

Aux éléments du modèle (les deux organisations, leurs problèmes internes et l'environnement dans lequel les parties se rencontrent), Dunlop en ajoute deux qu'il présente comme suit:

- le passé de la relation de négociation, son histoire, en tant qu'elle influence la négociation, parfois de manière institutionnalisée;
- les axes de développement que suit la relation de négociation c'est-à-dire les thèmes autour desquels elle s'organise (salaires, conditions de travail, classification professionnelles...).

La position prise par Whyte à l'égard du modèle de Dunlop est assez sommaire. Il l'accuse de simplifier à l'extrême les problèmes

internes aux organisations. "Les systèmes sociaux, dit Whyte, sont trop compliqués pour se soumettre à cette approche."[15] Car celles-ci, précise-t-il, implique que l'on centre l'analyse sur des thèmes limités, comme les salaires ou l'ancienneté. Or, si le problème est d'expliquer "le degré d'harmonie ou de conflit entre le syndicat et la direction - et c'est ça qui est le problème populaire aujourd'hui -, alors l'approche thématique nous offre peu de choses."[16] Il estime en effet qu'un thème déterminé peut être traité de plusieurs manières dans le cadre de relations différentes mais également harmonieuses. Enfin, conclut notre sociologue, ces études sont particulièrement maladroites quand elles s'occupent "des facteurs dits 'psychologiques' qui favorisent ou entravent la coopération entre le syndicat et la direction de l'entreprise."[17]

Aspects théoriques de la controverse.

Les traditions propres à chacune des spécialités et la concurrence pour le marché de l'expertise sociale dans le domaine des relations collectives de travail entre les économistes du travail et les sociologues industriels les amène à opposer deux modèles dont, on le voit, les ambitions sont différentes. Dunlop cherche à analyser un type de relations, la négociation collective, où se rencontrent deux organisations. Cette relation n'a pas de forme postulée. Whyte, par contre, s'intéresse à la manière d'établir de "bonnes" relations collectives, c'est-à-dire à expliquer ce qui fait que la coopération apparaît ou ce qui l'empêche d'apparaître.

Parmi les participants au débat, c'est assurément Fisher qui, avec sa verve coutumière, décrit le mieux l'opposition entre Whyte et Dunlop: "Whyte et Dunlop divergent avant tout sur une question fondamentale (...) de perspective philosophique et scientifique. M. Whyte, en faisant valoir que le comportement humain est le sujet important, adopte la position incontestable, quoique quelque peu redondante, selon laquelle la manière d'étudier le comportement humain est d'étudier le comportement humain. Cela doit être fait dans les environnements sociaux dans lesquels les êtres humains peuvent être observés, c'est-à-dire l'usine (<u>plant</u>)."[18] Fisher critique aussi la conception que se fait Whyte de l'entreprise, c'est-à-dire de la petite société qu'il étudie. "La compagnie, prétend M. Whyte, est un système social et le syndicat un autre. Ensemble, ils forment deux systèmes sociaux reliés et interdépendants. Mais comment M. Whyte

peut-il être sûr que ces organisations ne sont pas un seul système, plutôt que deux; ou, encore plus probablement, comme le soutient M. Dunlop, simplement une partie relativement petite d'un système social beaucoup plus grand? Les professions de foi dans la vision de M. Whyte sont que (1) que les phénomènes qu'il étudie sont systématiques, et (2) que, comme ils sont systématiques, ils forment eux-mêmes des systèmes. En ce qui me concerne, je trouverais difficile de définir un système en des termes tels que les variables indépendantes principales qui donnent aux phénomènes leur caractère sytématique se trouvent en dehors du système décrit."[19]

Fisher relève ce qui caractérise non seulement l'approche de Whyte mais toutes les études qui s'inscrivent dans la tradition des recherches industrielles américaines depuis qu'on y utilise le concept de "système social": le système est situé dans un environnement mais les effets de cet environnement sont pris comme des contraintes qui imposent la mise en oeuvre des mécanismes de contrôle. Fisher met alors en question la définition que Whyte donne du système social: il rappelle que le système se définit par l'interdépendance et par les effets provoqués par le changement. Se pose alors le problème des limites: "De toutes manières, je suspecte qu'une partie du système (un sous-système?) pourrait satisfaire à l'épreuve des liaisons multiples, et n'être pourtant pas un système du tout, si par système on entend un tout cohérent qui contient en lui les principes nécessaires pour expliquer son comportement."[20]

Homans est le seul à défendre l'approche de Whyte. Et il le fait non pas en luttant pour que les fondements de sa démarche soient reconnus, mais en opposant la modestie du modèle de Whyte aux ambitions de celui de Dunlop. Alors que Brown et Bakke, qui participent aussi à cette discussion, soutiennent que les modèles sont complémentaires, Homans affirme qu'ils sont identiques. Après avoir cité les composantes du modèle de Dunlop, Homans se dit d'accord avec son approche: "Je lui souhaite la bienvenue; je l'acclame." Mais il ajoute: "Je ne vois rien dans l'approche de M. Whyte qui entre en conflit avec (ce modèle)."[21] Pour Homans, la question est de savoir si la recherche portera sur tous les éléments ou seulement sur certains d'entre eux. "M. Dunlop propose visiblement d'étudier tous les éléments. M. Whyte dit qu'il va se limiter à en étudier un: deux ou plusieurs organisations (systèmes sociaux), et, plus particulièrement, une seule usine et un seul local syndical." Et ses raisons

pour le faire "sont pratiques et pas théoriques. Nous sommes tous limités par le temps, l'argent, et l'aide dont nous pouvons disposer."[22] De ce fait, selon Homans, Dunlop aura une vision générale des phénomènes qu'il étudie, alors que Whyte parviendra à une connaissance approfondie des éléments qu'il examine.

Du point de vue théorique, l'opposition entre les économistes et les sociologues peut être résumée comme suit. Les économistes groupés autour de Dunlop, Kerr et Fisher traitent des relations collectives de travail de manière globale: les organisations (ou systèmes sociaux) qui se rencontrent se trouvent dans un environnement qui détermine leurs relations. Les sociologues industriels traitent des faits locaux qui se produisent dans un nombre limité d'entreprises et ne proposent aucune vision de leur contexte. La rencontre des deux modèles évoqués ici va se terminer au profit des économistes, en partie grâce aux relais que ceux-ci trouvent auprès d'autres sociologues qui vont réserver un accueil très favorable à leurs travaux et contribuer à développer un type d'analyse des relations collectives de travail proche du leur.[23] Ces sociologues sont ceux qui critiquent (sur certains points) les travaux de Mayo et ses collaborateurs à la fin des années 1940. Toutefois, ces critiques finiront par aboutir à une approche des relations industrielles et des relations entre l'entreprise et son environnement qui sera une version agrandie, au sens photographique du terme, de l'approche que Henderson, Mayo et leurs collaborateurs ont mise en oeuvre à l'échelon de l'entreprise. Cette dernière sera alors considérée comme un système social appartenant à un système social plus vaste, qui l'englobe: la Nation. Et c'est aux pouvoirs publics qu'il appartient de favoriser l'intégration de l'industrie dans l'espace social national, de manière à ce que l'"ordre" soit préservé. Les sociologues se situeront alors du point de vue de la société globale, tout en traitant celle-ci comme une entité qui peut être appréhendée à l'aide du même cadre conceptuel que l'organisation industrielle. Les sociologues industriels américains deviennent à ce moment à la fois des penseurs de la gestion de l'organisation et des penseurs de la gestion de l'Etat.[24] Au cours des années qui suivent la controverse relatée ici, l'évolution de la sociologie industrielle, telle qu'elle sera développée par les sociologues qui voient l'approche des économistes d'un bon oeil, montrera donc que Homans n'a pas non plus tout à fait tort quand il

prétend que le cadre conceptuel sur lequel reposent les deux approches sont identiques.

Notes.

1. cf. Derber (1949, pp. 3-5). Parmi ses premiers membres et promoteurs, nous trouvons Vincent Bladen (Toronto), Frederick Harbison (Chicago), Richard A. Lester (Princeton), Sumner Slichter (Harvard)< William F. Whyte (Chicago), Dale Yodor (Minnesota). Ses officers pour 1948 sont entre autres E.E. Witte (Wisconsin), Charles W. Mills (Columbia) et Clark Kerr (Berkeley).
2. "It is almost litterally true that some new book dealing with industrial relations has been published in every week of the past year." (Witte 1949, p. 17).
3. Kerr, Fisher (1957, p. 281). Cet article date en fait de 1952.
4. Dunlop, Whyte, Bakke, Brown, Fisher, Homans, Kerr, Richardson Jr. (1950).
5. cf. Whyte, Gardner (1946); Whyte (1949); Whyte (1951); Dublin, Harbison (1947); Kerr, Randall (1948).
6. cf. Desmarez (1983).
7. Whyte in Dunlop et al. (1950, p. 393).
8. ibid., p. 394.
9. Whyte in ibid., p. 395. Whyte et ses collaborateurs empruntent ces concepts (interaction, sentiment et action (ou activité)) à Homans, sans encore à ce stade le signaler explicitement, ce qu'ils feront dans Whyte et al. (1955, pp. 191-193). Nous avons montré ailleurs (Desmarez (1986a, chapitre 7)) que le schéma conceptuel de Homans, que son auteur présente comme novateur, n'est en fait qu'une version rajeunie du modèle utilisé par le groupe de Harvard.
10. Whyte in Dunlop et al. (1950, pp. 399-400).
11. cf. Desmarez (1984a, chapitre 2) et surtout Desmarez (1986b).
12. Dunlop in Dunlop et al. (1950, p. 390).
13. ibid. p. 386.
14. ibid. pp. 386-387.
15. Whyte in ibid., p. 396.
16. ibid., p. 397.
17. ibid., p. 397.
18. Fisher in ibid., p. 407.
19. ibid., p. 407.
20. ibid., p. 408.
21. Homans in ibid., p. 409.
22. ibid., 410.
23. voir par exemple les comptes rendus suivants: Van Sickle (1947); Sorensen (1949); Moore (1948a); Moore (1948b); Miller (1949).
24. cf. Desmarez (1986a, chapitres 8 et 9).

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LA PROFESSIONNALISATION DU SAVOIR PSYCHOLOGIQUE AUX ETATS-UNIS (1887-1929)

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Summary

The chosen period is characterized, on the level of the general social context, by an industrial and technological growth without precedent. With regard to Psychology, it corresponds to the production of a legitimate academic knowledge, to the conquest of intellectual "markets" and "markets" for application.

The proces of professionalization, seized through its tensions, its conflictual and dynamic aspects, represents an heuristical frame of analysis of this setting.

The two fundamental hypotheses, both tested through a bibliographical analysis an through biographical data about key-persons, set forth, on one hand, that there has been no opposition between the settlement of a psychological academic nowledge and the different applications of Psychology, and, on the other hand, that the applications had a leading role in the development of knowledge.

Il est toujours difficile de délimiter une période particulière dans une histoire foisonnante. Celle que j'ai choisie répond à plusieurs critères pertinents pour ma démonstration. Disons tout d'abord qu'elle correspond à une période d'un dynamisme extraordinaire d'ouverture et de création de marchés tant intellectuels qu'économiques.

La première date, 1887, marque l'entrée de la Psychologie dans une ère qui se veut résolument scientifique, lorsque James Mc Keen Cattell inaugure la chaire de psychologie statistique de l'université de Pennsylvanie, la première chaire officiellement nommée chaire de psychologie. Ainsi commence à prendre corps le souhait énoncé vingt ans plus tôt par W. James: "it seems to me that perhaps the time has come to psychology to begin to be a science" (cité dans C. Mueller, 1979). 1887, c'est aussi l'année de la création de l'American Journal of Psychology par John Stanley Hall, à l'université Johns Hopkins. La

même année, la psychologie est créée comme discipline indépendante et se donne un organe d'expression. Cette marche vers la reconnaissance académique apparaît alors de la fondation, cinq ans plus tard, de l'American Psychological Association par Cattell et Hall.

La seconde date, 1929, marque un autre tournant. Cette année se tient à Yale le neuvième Congrès International de Psychologie, présidé par James Mac Keen Cattell. C'est le premier congrès international qui se tient aux Etats-Unis. Il symbolise la reconnaissance de la Psychologie américaine après que celle-ci ait traversé des hauts et des bas et connu des périodes de doute et d'incertitude. Inauguré par le célèbre mot de son secrétaire, Claparède, "enfin, enfin en Amérique", ce congrès fut marqué par la déconfiture des théories lamarckiennes et spenceriennes, alors que Mc Dougall présentait les résultats falsifiés de l'un de ses assistants, et par l'émergence de la Gestalttheorie, malgré le fait que la plupart des chercheurs américains aient présenté des communications "réflétant la "science normale": un fonctionnalisme mitigé, appliqué et comportemental" (M. Sokal, 1984, p. 297).

Simultanément, en dehors des frontières de la Psychologie, 1887-1929 délimitent le développement et la chute de l'ere progressive, cette période de créative technologique sans précédent et en constante accélération, de mise en place galopante d'une industrie, d'une production d'une consommation et d'une habitat de masse, d'ouverture des marchés du travail et de la consommation encore accrue par l'effort de guerre. Cette période inclut aussi un tournant important dans la nature et la densité de l'immigration alors qu'un million de personnes environ débarquent chaque année aus Etats-Unis. L'intrusion est ressentie avec d'autant plus d'acuité que ces nouveaux arrivants viennent de l'Europe du Sud et de l'Est, et non plus de l'Europe du Nord. l'impression d'être submergé par la lie de l'humanité, soutenue par une propagande eugéniste, va aboutir aux lois de restriction de l'immigration de 1924. La date où je m'arrêterai, 1929, c'est, bien entendu, l'année du "jeudi noir", du krach de la bourse de New York et du debut de la grande récession économique, qui donne un brutal coup d'arrêt à l'enthousiasme producteur et consommateur. C'est donc le début d'une fermeture drastique des marchés et, pour les psychologues, la fermeture du marché de l'emploi.

Ce rappel, plus que bref, des conditions socio-économiques de cette période s'impose néanmoins dans la mesure où, à la suite de beaucoup d'atres (A. Buss, 1979; K. Danziger, 1979, F. Samelson, 1977-79; M. Sokal, 1984; W. van Hoorn et T. Verhave, 1977 ... et moi-même, 1985), il s'agit pour moi de resituer l'emergence d'un champ de savoir et de pratique psychologiques et sa dynamique dans un contexte social global et de tenter d'apporter une contribution dans le domaine encore naissant d'une "sociologie du savoir psychologique" (A. Buss, 1975).

Ayant délimité ma période d'étude, il me rese à délimiter mon champ d'étude. Il s'agit de faire porter mon investigation sur la création de la profession de psycholoque (Flexner, 1945; Peterson, 1976); profession tout à la fois de production, de délimitation, de contrôle et de reproduction d'un savoir et profession offrant des services divers: encadrement des déficients, des enfants, des travailleurs, prise en charge des troubles du comportement etc...: profession organisée en sociétés fondées sur la définition d'une ou de plusieurs sortes de compétence; profession mettant en place des organes de diffusion internes ou externes, en veillant à ce que l'impression de compétence reste étrangère aux profanes, a fortiori aux "charlatans" que l'on s'efforce de débusquer.

Basée sur un savoir, la compétence professionnelle se manifeste par la maîtrise de technologies spécifiques. Pour Peterson (1976), la propriété la plus significative d'une profession consiste dans la disponibilité d'une "technologie communicable fondée sur une discipline intellectuelle raisonnablement cohérente" (p. 572).

Les analyses des professions, des critères de définition des professions constituent à elles seules un vaste champ de la sociologie. Je voudrais néanmoins préciser que mon objectif est de prendre des distances par rapport à une perspective fonctionnaliste (Carr-Saunders et Wilson, 1933; Parsons, 1949). Ainsi que le note W.J. Goode (1960), "no occupation becomes a profession without a struggle, just as no specialty develop inside a profession without a struggle". Je ferai donc porter mon investigation sur la professionnalisation, c'est-à-dire le mouvement, largement conflictuel, par lequel un groupe social crée et structure une réalité sociale, un champ cognitif sur lesquels il fonde sa pratique, intellectuelle ou technique. Bien évidemment, cette analyse en termes de conflicts et de conquête de statuts et de marchés rejoint celle de M.S. Larson (1977). Elle

doit également beaucoup à E. Freidson (1984), eu égard au renversement qu'il opère par rapport au modèle fonctionnaliste.

L'objet de ma démonstration est, tout d'abord, en me centrant sur les rapports entre savoirs et technologie dans le mouvement de professionnalisation de la psychologie, de mettre en évidence le primat de l'élaboration technologique sur l'élaboration scientifique (1), l'existence d'une technologie socialement efficace et reconnue légitimant la qualité de la Psychologie comme science véritable, proche des sciences naturelles et de la biologie; proximité qu'il est nécessaire d'affirmer avec redondance et récurrence et qui se manifeste dans les liens effectifs, institutionnels, entre Psychologie et sciences "dures". En retour, le sentiment de produire une véritable science légitime toute technologie et la recherche de créneaux pour ses applications. Ensuite, afin de comprendre l'émergence d'une discipline et sa professionnalisation, à travers un procès de mise en place de "technologies disciplinaires" (2), au sens où l'entend Michel Foucault (1975), il s'agit de démontrer comment la Psychologie américaine du tournant du siècle a été un des vecteurs privilégiés de la mise en place de techniques de contrôle par la quadrillage individualisant des comportements humains et leur visibilisation. Ceci me permettra de souligner comment "les sciences humaines font partie d'un ensemble plus vaste de pratiques organisées et structurantes qu'elles contribuent dans une large mesure à élargir" (Dreyfus et Rabinov, 1984, p. 152).

Vaste objectif qu'il ne saurait étre question d'atteindre pleinement dans le cadre de ce court texte. Aussi faut-il encore resserrer l'univers de ma démonstration en analysant un segment particulier de la profession de psychologue. A la suite de Bucher et Strauss (1961), je veux mettre l'accent sur l'importance de différents niveaux spécialités dans une profession qui interagissent de façon dynamique, soit de concert, soit en opposition, eu égard à la définition de leur méthodologie, de leur mission ou de leur méthodologie. Le segment sur lequel je centrerai mon attention concerne un groupe particulier de psychologues qui se sont employés activement à la fois à définir le savoir académique, à tenir les rènes des sociétés et des organes de diffusion de ce savoir, à conquérir des positions universitaires, à "vendre" la Psychologie aux entreprises privées, aux grandes administrations étatiques et/ou à populariser la

Psychologie auprès du public profane à travers les grands organes de presse.

Dans ce segment, je m'intéresserai plus particulièrement au devenir conjoint de cinq personnalités exemplaires, de cinq entrepreneurs de la Psychologie: Walter Dill Scott, James Mac Keen Cattell, John Broadus Watson, Robert Mearns Yerkes et Lewis Madison Terman; personnalités de premier plan puisque tous ont été, à un moment ou à un autre, au cours de la période qui m'intéresse, présidents de l'American Psychological Association.

Je voudrais souligner que mon objectif n'est pas d'effectuer les biographies croisées de ces cinq personnalités. Notons seulement qu'elles émergent de façon particulièrement saillante dans la mise en place d'institutions académiques et extra-académiques caractérisant le processus de professionalisation du savoir psychologique. Elles interviendraient à titre de 'types-idéaux' caractérisant un fonctionnement dont je fais l'hypothèse qu'il était présent dans toute la profession.

A cet égard, je voudrais souligner ici l'opposition entre mon analyse et celle de D. Napoli (1984). En effet, Napoli se centre sur une définition dela profession de psychologue seulement comme profession de service, ne considérant qua la psychologie appliquée. Je veux au contraire mettre en évidence le lien congénital entre psychologie scientifique et psychologie appliquée, laissant Napoli en découdre avec la contradiction qui traverse son investigation et qu'il exprime de loin en loin. "Until recently, applied psychologists could claim to be the only occupationnal group with academic origins" (p. 8). "If applied psychology was becoming established as a technical field, it remained in its childhood as a profession. The overwhelming proportion of its practitionners were academicians first and foremost" (p. 28) De fait, comme le note M.S. Larson, une profession est toujours définie par ses élites.

1. Le versant académique

L'étude du versant académique de la profession permet de mettre en évidence la construction de la crédibilité de la Psychologie pour les autres sciences, en particulier pour les sciences biologiques qui connaissent une grande renommée à la suite des travaux de Darwin.

1.1. La conquête des bastions universitaires

Cette conquête s'effectue au sein d'universités qui se créent et de savoirs qui se constituent des champs autonomes. Le premier PhD des Etats-Unis a été attribué à l'Université de Yale en 1861. G. Stanley Hall, qui passa son PhD à l'université d'Harvard en 1873 est considéré comme ayant reçu le premier doctorat de Psychologie des Etats-Unis. Ce doctorat était le dix-huitième attribué à Harvard dans tous les domaines d'étude. Quant au processus de création de chaires et de laboratoires de Psychologie, il s'accélère après 1887 pour dépasser la vingtaine en 1892 (T. Camfield, 1973).
Si l'on en prend pour preuve le nombre des Universités ayant un psychologue pour président et jouissant d'un grand prestige, - entre autres, Northwestern, Yale, Cornell, North Carolina, Michigan... - on peut affirmer, qua la fin des années dix, la psychologie avait atteint un statut académique élévé et comptait parmi les références obligées des autres sciences.

1.2 La proximité de la Psychologie et des sciences dures

Omniprésente dans la conviction et le discours des Psychologues, elle est particulièrement illustrée par l'action de James Mc Keen Cattell. Elève de Wundt et de Galton, premier détenteur d'une chaire de Psychologie, Cattell ne s'est pas particulièrement illustré par ses succès scientifiques: sa tentative de mettre sur pied une Psychologie anthropométrique fut un échec (M.M. Sokal, 1983). Mais il a particulierement bien réussi dans son effort de rapprochement de la psychologie et des sciences dures. Premier président élu de l'American Association for the Advancement of Science (AAAS), qui fut la première société scientifique américaine, il dirigea les éditions des revues Science Scientific Moonthly, American Naturalist et les six premières éditions de l'American Men of Science. Notons également qu'il fit preuve d'une remarquable longévité à la tête de la fameuse revue Science.

Quatrième président de l'American Psychological Association (APA), qui tenait ses réunions annuelles en même temps et dans les mêmes lieux que l'AAAS, Cattell ouvrit son discours en affirmant que la Psychologie devait être considérée sur un pied d'égalité par rapport aux "autres sciences naturelles", et il le termina en prédisant que la psychologie allait contribuer, plus que toute autre science, à éclairer la Philosophie égarée. Bien plus, la psychologie devait légitimement occuper une place de tout premier plan dans les

sciences naturelles car "le contrôle scientifique de la conduite pourrait devenir bien plus important que l'utilisation de l'electricité et de l'acier" (J. Mc Keen Cattell, 1896).

1.3. La Psychologie, une science des faits et de la mesure

Pour devenir une véritable discipline scientifique, la Psychologie doit se différencier de la philosophie morale et remplacer sa pierre angulaire théorique - la conscience - et méthodologique - l'introspection - par la manipulation objective des nombres. Galton et Wundt inaugurent le règne de la mesure des performances individuelles et la synthèse de leur orientation est faite par leur élève commun: J. McKeen Cattell. Mais c'est la mesure de l'intelligence et son extraordinaire expansion qui vont véritablement permettre d'affirmer le caractère objectif de la Psychologie. Ainsi que le souligne Terman (1921) en reprenant à son compte des affirmations antérieures concordantes de Boring, Goddard, Thorndike, Warren, Woodworth: il n'y a pas de différence entre l'utilisation du test mental et l'expérimentation en laboratoire. Le test mental est véritablement de la canned science, de la science en boîte de conserve!

1.4. Une théorie structurante, le behaviorisme

C'est néanmoins le behaviorisme qui va faire entrer pleinement la Psychologie dans une ère d'élaboration théorique. Que le behaviorisme n'ait pas bouleversé d'emblée une communauté scientifique déjà bien diversifiée en créant un sentiment général de "rupture épistémologique", comme l'affirme la légende, est attesté par F. Samelson (1981, 1985). Cependant le behaviorisme s'est imposé comme théorie princeps du contrôle de l'activité humaine. Ici encore, la concordance de deux dates ne manque pas de nous frapper: 1911, publication des Principles of scientific management de F. Taylor, qui présente le travailleur comme un "gorille dressé"; 1913, publication du premier manifeste behavioriste de Watson. Il est hors de question de dresser ici un parallèle détaillé entre les deux oeuvres, dont les convergences ne manquent pas (cf. V. Curi, 1978), soulignons seulement leurs principaux traits communs: le contrôle de l'activité humaine fondé sur la malléabilité du comportement, la décomposition et l'analyse la plus fine possible des mouvements et des réactions, la séparation entre ceux qui modèlent, observent, dirigent, pensent et ceux qui exécutent, voire l'inutilité de l'activité spéculative pour

l'exécutant et son impossibilité à appréhender le dispositif global dans lequel il est inséré.

2. La conquête des marchés

2.1 La publicité

La première demande adressée à la psychologie émanait de la publicité (D.P. Kuna, 1979). A cet effet, en 1896, à l'Université de Minesota, Harlow Gale mit au point des méthodes d'investigation expérimentale. Walter Dill Scott, formé en Europe chez Wundt et professeur, puis président de 1920 à 1939 à la Northwestern University, débuta sa carière en étudiant les modalités d'application de la psychologie à la publicité, publiant dès 1901 The Psychology of advertising. Grace à la psychologie, affirmait-il, il devenait possible d'adjoindre à l'art de la publicité une science véritable. Puis, sortant des méandres de la persuasion clandestine, la publicité est de plus en plus perçue comme technique de normalisation et intégrée dans l'effort d'américanisation (S. Ewen, 1983).

2.2. La sélection du personnel

La sélection du personnel devint un problème aigu lorsque, dans les années dix, la rotation de la main d'oeuvre, conséquence d'une situation de sur-travail encore accrue par le développement de l'industrie de guerre, devint vraiment insupportable aux employeurs. Les psychologues vont alors se consacrer au recrutement et à la gestion du personnel. Dès 1915, Walter Dill Scott fut l'un des précurseurs de l'application de la psychologie à la gestion du personnel, des vendeurs en particulier. En liaison avec la division de Psychologie appliquée du Carnegie Institute of Technology, il fonde avec Walter Van Dyke Bingham le Bureau of Salesmanship Research, qui travaille avec de nombreuses et importantes firmes, entre autres: la Ford Motor Company, Goodrich Tire and Rubber, Westinghouse Electric, Heinz Foods, Presidential Insurance. Carnegie Steel. Dès l'entrée en guerre des Etats-Unis, en 1917, Scott et Bingham proposent leurs services au War Department et organisent un Comité de Classification du Personnel qui, en un an, jusqu'à l'armistice, évulua les capacités de trois millions de personnes!

Ce bel effort avait attiré l'attention et la demande d'entreprises. En 1919, pour répondre à cette demande, Scott fonda la Scott Company, s'adjoignant les services d'Angell, Bingham, Thorndike,

Watson, Yerkes et d'autres, et collaborant régulièrment avec une quarantaine de firmes.

Dans le même temps, Cattell s'employait à mettre sur pied la Psychological Corporation, "une compagnie de holding pour psychologues", visant à faire de l'exercice de la psychologie une profession de services ouverte sur le public et mise à l'abri des "charlatans". Il obtint la collaboration de psychologues tels que Hollingworth, Scott, Strong, Thorndike, Titchener, Mc Dougall, Woodworth, Yerkes, Terman et Watson. Bien que la Psychological Corporation ait connu des hauts et des bas dans sa gestion financière et un démarrage difficile (M. Sokal, 1981), elle contribua grandement à la diffusion de la psychologie et est aujourd'hui tout à fait florissante.

De son côté, enfin, Yerkes fonda en 1921 la Personel Research Federation, faisant appel à la collaboration de son ami Terman.

2.3. L'effort de guerre et le marché de l'intelligence

Il est difficile de dissocier la recherche et la promotion de la classification du personnel de la conception et de l'utilisation à grande échelle des tests d'intelligence. L'histoire de la mesure de l'intelligence commence à être bien connue (cf S.J. Gould, 1983; F. Samelson, 1977, 1979; N. Pastore, 1978; G. Lemaine et B. Matalon, 1985). Dès 1910, les études de H.H. Goddard, qui importe et adapte aux Etats-Unis les tests de Binet, démontrent l'infériorité intellectuelle des immigrés et l'existence de "races inférieures". Dès l'entrée en guerre des Etats-Unis, Yerkes, alors président de l'APA, considéra qu'il était de son devoir d'évaluer la façon dont la psychologie pouvait contribuer à l'effort de guerre. Il réunit en session extraordinaire les membres de l'APA qui proposèrent leur expertise au War Department. C'est ainsi que débuta l'évaluation de l'intelligence de 1 700 000 recrues par des méthodes bien approximatives et que, selon ses practiciens, la psychologie remplit son devoir patriotique en contribuant grandement à la victoire.

Laissons Terman (1924) dresser le bilan positif de cette grande aventure scientifique. "It is the method of tests that has brought Psychology down from the clouds and made it useful to men; that has transformed the "science of trivialities" into the "science of human engineering". The psychologist of the pre-test era was, to the average layman, just a harmless crank, but now that Psychology has tested and classified neearly two million soldiers; has been appealed

to in the grading of several million school children; is used everywhere in our institutions for the feeble-minded, delinquent, criminal and insane; has become the beacon light of the eugenics movement; is appealed to by congressmen in the reshaping of national policy on immigration; is furnishing high-powered explosives for the social reformers of one wing, while serving at the same time as the target drawing the hottest fire from the other wing- no psychologist of to-day can complain that his science is not taken seriously enough. And is not most of this change, you will say, due to the mental test?"

Laissons aussi conclure Samelson (1977) qui se demande si le grand mouvement de mesure de l'intelligence doit être considérè comme "un épisode de l'histoire réussie d'une science empirique ou comme l'histoire d'horreur d'une idéologie scientifique" (p. 180).

En effet, la popularisation de cette investigation dans la grande presse renforça les thèmes de l'ubiquité du "moron", de son danger pour la démocratie, donc de la dégénérescence de la Nation américaine envahie d'immigrant débiles. Les psychologues de l'intelligence militent dans l'Immigration Restriction League. Quant à Yerkes et Terman, ils fondent l'Eugenics Record Office. Une anecdote donne le ton du débat et la virulence des arguments: la fait que le journal New Republic abrite dans ses colonnes la fameuse controverse entre Terman et W. Lippman, ce dernier soulignant l'absurdité de l'affirmation selon laquelle l'intelligence moyenne américaine était inférieure à la norme, le fit surnommer Jew Republic...

Quant aux eugénistes, ils obtinrent gain de cause avec la promulgation en 1924 des lois de restriction de l'immigration. La psychologie unie avait bien fonctionné dans le sens du vent...

2.4. Retour à la publicité

En 1920, Watson se fait renvoyer de l'Université Johns Hopkins pour une histoire de coeur sur laquelle l'Amérique puritaine ne plaisantait pas. Il entre dans la J. Walter Thompson Company où il commence à régner sans partage. Il affirme détenir les meilleures méthodes pour influencer autrui, prédire et contrôler les comportements (cf. K.W. Buckley, 1982).

L'Intérêt de Watson pour la Psychologie appliquée doit être lié à son désir de dotor la psychologie d'objectifs professionnels indépendants. De leur côté, les publicitaires avaint besoin de

connaître les principes universels expliquant la motivation à la consommation. Watson apportait une caution scientifique incontestable qu'il monnaya bien chèrement, en partie par attachement à un rêve scientifique progressiste et au désir d'un ordre social stable et prédictible, et en partie par mercantilisme.

En guise de conclusion

A travers ce survol -trop- rapide de la professionnalisation du savoir psychologique, la psychologie du début du siècle apparaît à la fois comme chaque pseudo-scientifique des idéologies dans lesquelles elle s'insère et qu'elle contribue à renforcer et comme légitimation des pratiques sociales des groupes dans lesquels elle est élaborée. De plus, il faut, bien entendu, resituer l'émergence de la profession de psychologue dans le cadre de la mise en place d'une industrialisation de masse et d'une nouvelle division du travail où, à la prolétarisation du travail productif, il faut opposer la diversification et multiplication des activités de service exercées par les classes supérieures qui échangent une fonction de contrôle social contre de privilèges. "As capitalism matured, the ideology of profession could be incorporated into generalized forms of social control, extended now to the growing class of nonmanual and "unproductive" workers. (...) Compartmentalization and differenciation within the division of labor create (mainly bureaucratic) positions of relative privilege, which their incombents strive to "dignify" and monopolize by claiming expertise and by professionalizing... (M.S. Larson, 1977, p. 238).

Il faut resituer l'émergence de la psychologie dans la mise en place d'un ensemble de pratiques qui visent à subdiviser le contrôle en un ensemble de pratiques ténues, minutieuses et omniprésentes, fonctionnant tout à la fois à l'extérieur et à l'intérieur des individus et assurant leur hiérarchisation sur la base de classements incontestables, mais inconnus d'eux.

C'est ce que la Psychologie opère à travers la mise en place de mesures fouillant des moindres recoins des individus, puis d'une théorie instituant le contrôle et l'optimisation du comportement individuel comme principe de base.

Le lien qui s'établit d'emblée discours scientifique et pratiques technologiques, illustré par l'action pluraliste des mêmes hommes, démontre à l'évidende que la psychologie prouve dès le début qu'elle a quelque chose à vendre, et que ses acheteurs payent en

monnaie sonnante et trébuchante un discours et des pratiques efficaces dans leur contexte socio-économique. Ainsi la psychologie devient-elle l'affaire de "professionnels" et non de philosophes ou de rêveurs.

Notes

(1) On notera la convergence de notre démonstration et de celle de K. Danziger lors de ce même colloque. On notera également la proximité de nos hypothèses avec cette affirmation de W. Van Hoorn (1983):

> "In rough outline, I propose to argue that from the beginning of this century on, mechanistic ways of thinking have penetrated into the heart of the science of psychology. From the first world war on, applied psychology, usually called psychotechnics, has pushed out theoretical and general psychology at an ever increasing speed" (p. 105).

(2) Dans Surveiller et Punir, M. Foucault traite de l'examen comme "technique disciplinaire". Les tests psychologiques peuvent être envisagés comme des sortes d'examen, avec cette contrainte particulière et supplémentaire que les individus ne disposent d'aucun savoir de référence qui leur permet de prédire leur évaluation.

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THREE PSYCHOLOGISTS AND THEIR FILMS: WOLFGANG KÖHLER, KORT LEWIN AND FRITZ HEIDER

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Summary

The scientific film has a long tradition in psychology and has served as a tool (1) to record behavior, (2) to demonstrate behavior, e.g. for teaching purposes, or (3) to be used as a stimulus, e.g. in studies on social learning. Despite this tradition and despite the fact that early behavioral scientists like Hugo Münsterberg, John B. Watson, or Emil Kräpelin produced valuable films, it seems surprising that very little is known about the history of films in psychology. Many films are apparently lost, or damaged. In other cases where the films still exist we know nothing about when or why they were made.

This paper gives some background information on films of three Gestalt psychologists who shaped resp. emerged from Gestalt psychology:

(1) Wolgang Köhler's Intelligenzprüfungen an Mensenaffen dated 1914-17,

(2) the recently rediscovered films of Kurt Lewin showing the behavior of children in conflict situations, produced 1923/24,

(3) the cartoon produced by Fritz Heider and Marianne Simmel for the 1944 study on apparent movement.

Excerpts from the films were shown at the Fourth Meeting of Cheiron Europe, they are compared and evaluated.

Wolgang Köhler's Intelligenzprüfung an Menschenaffen

(Intelligence tests for primates)

In 1970 Herman Kalkofen was rummaging through the nitrofilm archives of the Institute for the Scientific Film (IWF) in Göttingen when he found a film entitled Ape film from Teneriffe. Together with the film he found an anonymous imcomplete typescript with Comments on the ape

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1 I gratefully acknowledge valuable suggestions of Hermann Kalkofen, Lothar Sprung and Hans Rappard.

film from Teneriffe. As was soon discovered, this "ape film" was an early copy of Wolfgang Köhler's famous film of the investigations into primate intelligence. Stills from the film were included in his book (1917, 1921, 1963; translated into English 1925, into French 1928). Mrs Lili Köhler was also able to confirm that the anonymous manuscript had been written by her husband. So the film was reconditioned and edited, Kalkofen (1975) provided a set of accompanying notes, and the film was released for general viewing. As it turned out, however, this was not the only copy of the film still in existence: Köhler himself had shown a private copy in November 1966 shortly before his death in June 1967 (Preface to Köhler 1971).

The Anthropoid Research Station run by the Prussian Academy of Sciences on Teneriffe existed from 1912 to 1920. Its purpose was the investigation of animal and brain psychology (Rothmann & Teuber 1915). These investigations could not be carried out in Berlin because "the costliness of the chimpanzee material needed for this kind of research presents almost unconquerable difficulties". A climatic suitable location was found in Oratawa on Teneriffe; the necessary funding was provided by the Albert Samson Foundation. Much of the enthusiasm for founding this station must have come from Carl Stumpf (1848-1936), at that time Director of the Psychological Institute of the University of Berlin (cf. Sprung and Sprung 1985, and personal information from L. Sprung 1985). Curiously, however, there is no mention of Carl Stumpf in Köhlers early publications (1917, 1922 a, 1922 b) even though he must have played a considerable part in establishing the station and was also instrumental in ensuring that Köhler was appointed for a year as its director since January 1st, 1914, shortly before the outbreak of the First World War (cf. Köhler's account of this in the 1973 ed., p. 14).

Köhler's period of office lasted not only until 1915; he had to stay on Teneriffe as an internee until 1920. Boring and his followers have viewed the extension of Köhler's involuntary stay in Teneriffe as being to "the ultimate advantage of psychology" (Boring 1957, p. 596). However so far as Köhler's chimpanzee studies are concerned this is not true, because Köhler had finished "almost all my observations" in "the first six months of 1914" (Köhler 1973, p. 5). He tells us: "in the first few months I had overworked the apes rather" (p. 102). As can be confirmed from the illustrations and Köhler's

report to the Academy it can be shown that Köhler began filming on March 7, 1914.

Looking back on Köhler's work we can see that both his experiments and his film conform with the Zeitgeist - to use one of Boring's favourite expressions. But what does that mean? For the animal psychology of the twentieth century they were pioneer experiments. Virtually to introductory text on psychology fails to present Köhler's attractive experiments. There can be no doubt that Köhler's ideas in 1914 were already strongly determined by Gestalt psychology which he had helped to found. His doctoral thesis had been supervised by Stumpf (1909) and from 1910 to 1913 Köhler had been Max Wertheimer's assistant together with Karl Koffka. In later years (1942, p. 97) Köhler described this period as "years of cheerful revolt in German psychology". Köhler's recognition that the problem-solving behaviour of the chimpanzees was insightful behaviour is a result of this Gestalt psychology approach.

It is interesting to ponder where Köhler got the idea for such original experiments with apes. Unfortunately this question is not discussed either in Köhler's own publications or in secondary works. Köhler's publications give the impression that both the conception of the experiments into ape intelligence and all the individual investigations were his own idea: no other instigators are mentioned. But it seems rather improbable that Köhler, who was not a zoologist, should have left Wertheimer in order to take over the director-ship of an anthropoid station and within a very short time to conduct a series of very imaginative experiments on chimpanzees.

We can set up three hypotheses to account for this sequence of events:

Hypothesis 1: Wolgang Köhler had had zoological ambitions for a longtime, and the studies had been prepared well in advance. I consider this hypothesis to be false. We can find no indication for early zoological ambitions in Köhlers works. It is also indicative that Köhler did not continue his experiments once he was back in Berlin although this would have been possible. Nor did he take part in the lively discussions about anthropoid intelligence conducted in the twenties and thirties.

Hypothesis 2 Köhler's experiments can be viewed as the continuation of earlier experiments although there is now no record of these. This hypothesis seems very probable. Investigations into the

specific abilities of various animals were carried out in large numbers at the turn of the century (see overview in Rosenthalt 1977). Köhler could have followed up these studies, and indeed he does mention three of them, though only en passant, in his own study (1917, 1973): Hobhouse (1901), Sokolowski (1908) and Thorndike (1911).

Like Köhler, Hobhouse and Thorndike had performed detour problem studies. (These experiments were to be important later for Lewin's field theory.) In addition Hobhouse had performed rope experiments (cf. Köhler 1973, footnote on p. 22) and some of the other experiments later conducted by Köhler. Solokowski had observed anthropoids in Hagenbeck's Zoo in Hamburg-Stellingen and claimed that their behaviours gave evidence for particular intelligence (Sokolowski 1908).

These three references show that Köhler was able to link his own work to research which was very up-to-date at the time. And it also explains the reason why he chose to make films. Solokowski's observations (which Köhler, incidentally, was able to confirm) had been doubted by other researchers. So Köhler decided that eye witness records were not sufficient proof. In the 1917 edition of his chimpanzee book he states "A cinemographic film, which was made so that no-one could accuse me of exaggeration, cannot be developed, and is probably spoiled" (p. 65). So we are probably right to assume that Köhler viewed his films as proof for his claims.

Hypothesis 3: During Stumpf's directorship studies aimed at investigating animal intelligence were an important area of research activity in the Psychological Institute in Berlin.

At first glance this third hypothesis seems rather improbable. Stumpf (1848-1936) was Professor of Psychology at a number of universities and founded the Psychological Institutes in Munich and Berlin. Today he is best known as the teacher of Edmund Husserl and the founder of acoustic psychology. His student Erich von Hornbostel (1877-1935) built up the Staatliche Phonogramm-Archiv (State Phonogramme Archive) at the Psychological Institute in Berlin. (Lothar Sprung, Humboldt-University, informs me, that in the aftermath of the Second World War, the sound recordings from this archive ended up in West Berlin while the catalogue remains in East Berlin. Like Köhler and Lewin,

Hornbostel emigrated to the USA in 1933). So there seems to be no evidence that Stumpf's Berlin Institute was particularly engaged in animal psychology. But there is one important study which is known to have been performed at Stumpf's suggestion, namely that of Stumpf's students, Oskar Pfungst, on Herr von Osten's horse, "Clever Hans" (1907). Hans was a famous horse who was able to solve arithmetical problems, spell out names, and do many other things. Together with other prominent people such as the circus director Paul Busch, the zoo director Ludwig Heck and Dr. Oskar Heinroth, Stumpf was a member of the Committee which was given by Stumpf the task of investigating Hans. They managed to exonerate Hans' owner, Herr von Osten, of any suspicion of complicity or fraud, but otherwise they were completely baffled.Subsequently, however, Oskar Pfungst, who is described by Wolfgang Metzger as a rather "eccentric" colleaque (1970), was able to solve the riddle in close collaboration with Stumpf. When Köhler submitted his doctoral thesis to Stumpf in 1909, he must have heard about this, particularly since Pfungst later had conducted other animal studies (cf. footnote 5 in Rosenthal 1977, pp. 29-307).

If we summarize the results of our three hypotheses we can say that Wolggang Köhler had no specific training in the study of animal psychology but that he did have access to a number of recent studies in the area. His work fits into the variegated spectrum of studies conducted under the leadership of the versatile and often underrated Carl Stumpf (cf. Sprung & Sprung 1985).

Kurt Lewin's films of children in conflict situations

In the early seventies Grace Heider from the University of Kansas in Lawrence made an extraordinary discovery. In the Department of Psychology she found about 100 rolls of film belonging to Kurt Lewin, who neve worked at that University. Around half of the films had been made when Lewin lived in Berlin.

Kurt Lewin made films for many years and often they had three different purposes simultaneously. They were primary material for looking into affects, conflicts, and life space; they were illustrative material for congresses and lectures; and they also had a private function, because the people filmed were almost always

children: his own, or those of his relatives or friends (cf. Lück, 1985).

Lewin's films had a wide variety of topics. Grace and Fritz Heider translated the still legible labels on the rolls and made great efforts to ensure the preservation of the films. Some of the nitrate films had been destroyed by fungus, and they were also explosive. Unfortunately no institution or private individual was prepared to donate the necessary $4,000, for restauration and so the films had to be destroyed. One of them contained records of the development of a little girl form the age of five weeks to six months.

However, some of the scenes from the Berlin period have been preserved, probably because Lewin himself had them copied onto safety film and provided them with English captions. At the instigation of Volker Möntmann and with the permission of Miriam Lewin, Lewin's daughter, this material was used for a teaching film produced by the FernUniversität in 1983/4.

One example of a film which was probably originally intended for private purposes shows a two-year-old girl going up and down some low garden steps for the first time. This is very probably Karin Köhler, Wolfgang Köhler's daughter. In a number of his publications, particularly those concerned with developmental psychology, Lewin refers to his own films, for instance in his 1926 essay Filmaufnahmen über Trieb- und Affektäusserungen psychopathischer Kinder (verglichen mit Normalen und Schwachsinnigen) (KLW, Bd. 6, p. 41 ff). Lewin stresses the advantages of films in psychological research and writes that he himself uses a 35mm camera belonging to the Psychological Institute which enables sequences up to 6 minutes in length to be shot. In note 6 to this essay he says: "In addition to the big camera I also use a small Kinamo with a spring drive which is very useful in filming unexpected events happening in unexpected places and which also makes it possible to follow the subjects if they move about." According to Volker Möntmann this "Kinamo" was Lewin's private 16mm camera with which he had started to film. The existing Lewin films were probably all made with this small camera.

The 16mm camera made by the firm of Ica in Dresden first came onto the market in 1923 and cost 190 gold marks (cf. Kuball 1980, p. 68). It was the first German hand-held camera for amateurs After winding up the spring (up to then an unusual device) 6 metres of

normal film, in other words a few seconds only, could be exposed in one shot. The roll of film was 15 metres long. As no tripod was used the film scenes were short and unsteady, but the advantage of the Kinamo lay in the greater mobility of the cameraman.

The film material used at the time was flammable and extremely expensive. One minute of 35mm film cost around 20 marks; this was the equivalent of around three days' pay for an industrial worker (Kuball 1980, p. 60). Even if 16mm film was less expensive, we can see from these figures how important the films were to Lewin.

In the same essay (footnote 7) Lewin writes about the film technique: "If it wasn't possible to use a hedge or something of the sort to remove the camera from the subject's field of vision then we made use of a tent which hid everything but the lens which poked out through a slit". We can assume that Lewin's own children, Agnes, Fritz, Miriam and Daniel, were so accustomed to the whirring camera that such precautions were not necessary with them.

So far as we know Lewin made most of the films himself. This is the reason that Lewin does not appear in the scientific films; it is only in the private film that we glimpse him now and then.

Lewin's activities as a film maker received much praise both in Germany and the USA. In 1927 William Stern included a Bild- und Textbeitrag (picture and text contribution) from Lewin in his encyclopaedic 4th edition of Psychologie der frühen Kindheit (Psychology of early childhood). In his foreword Stern writes:

> Another regrettable gap was fortunately filled by investigations performed by Privatdozent Dr. Kurt Lewin (Berlin). Up to now our book had lacked illustrations of expressive movement in children. Dr. Lewin, who has recently been making scientific films of children's behaviour, was so kind as to put together a series of pictures of small children which we have included in four new plates. Dr. Lewin also wrote the accompanying text (see Appendix). We take this opportunity of expressing our thanks for this welcome enrichment of our text.
>
> Hamburg, 7th Dec. 1926 William Stern

The film scenes which Lewin selected and described have only been partially preserved. One of these films shows two-year-old Frank who wants a plum which his mother has just ostentatiously peeled and stoned. She is holding the plum to high above his head that at first Frank cannot reach it. For Frank, conflict arises out of the positive valance of the plum and its unavailability. It is easy to recognise the conflict in the child's restless movements. If conflicts become

too strong we avoid them. Appropriately Lewin calls such behaviour "leaving the field". In another Lewin film we find an excellent example for this. Five-year-old Agnes, Lewin's daughter from his first marriage, wants her friend to return her doll, but the friend doesn't want to. "First of all there is a brief scuffle over the doll, but then Agnes runs away, drooping in despair. The picture shows Agnes 'drooping'. Indeed, after running a few steps in this posture, Agnes then throws herself down on the grass" (Lewin 1927, p. 506). While this is not on the film we do see how Agnes accidentally knocks over her younger brother. "The picture shows the typical regretful, rather frightened and apologetic gesture: the hand is clapped to the open mouth" (Lewin 1927, p. 506).

Today the vast bulk of Lewin's films must be regarded as lost. No-one knows what has happened to Das Kind und die Welt. Eine Tonfilmstudie über acht Jahre Entwicklung, Aufbau und Lebensraum des Kindes (The child and its world: a study of eight years in the development and life of a child). This sound film was made in Berlin; it was 2000 metres long and had been selected from 100,000 metres of filmed material. A long review by Scheerer and Katzenstein (1933) stresses the uniqueness of this film. The children were filmed without their knowledge, and the film shows how the purely quantitative and formal phenomenon of 'growth' acquires a qualitative dimension. It is not merely that the child can 'do' more in each successive phase; it perceives its environment as having a different meaning. The camera shows us the world of the home from down below, from the child's viewpoint and prerspective... From its position on the floor the crawling child develops its own sphere of interests and activities at precisely those levels of which the normal adult is unaware. ...we see the tempation furnished by the hanging corner of the tablecloth... which finally becomes so irresistible that with great effort we pull ourselves up onto our toes and finally manage to grab the dangling corner. Inevitably, of course, together with the tablecloth we also pull all plates and dishes onto the floor" (p. 118-120).

These short extracts of the film review show that already towards the end of his Berlin period Lewin was able to present basic aspects of his field theory in a convincing visual form.

Lewin's films do not reveal any startlingly new aspects of his theories, but they are of considerable importance for historical

research into Lewin's work. Through his films Lewin also became known in the USA. In 1929 he was invited to the 9th International Congress of Psychology in Yale where his films helped him to make a lasting impression.

> "The Soviet psychologist Alexander Luria was so impressed by Lewin's films that he arranged a meeting with Sergeij Eisenstein, and started to plan a psychological laboratory of his own in Moscow which would cooperate closely with the National Film Academy" (Graumann in KLW, Bd. 1, 1981, p. 9).

Bluma Zeigarnik, a Russian student from Lewin's Berlin period also stressed in a recent interview that Lewin's films had been very important for his research and in the reception of his work (1984).

The Heider-Simmel cartoon as the start of attribution research

In the majority of text books on social psychology Fritz Heider's name appears twice: once as the founder of attribution theory and once as the founder of the balance and dissonance theories (see below). Both theories have been very important in research into social psychology in the last thirty years. However, in a recent "handbook of great psychologists" (Bonin 1983) which contains potted biographies of more than 400 researchers, one looks for Heider's name in vain. This lack of resonance is partly the result of Heider's personality and philosophy of life: although he is a prolific writer he published relatively little and what he did publish was exacting and ahead of its time.

Heider's recent autobiography (1983, German 1984) provides fascinating insights into his work, in particular to his "experimental study of apparent behaviour", published in 1944 in the American Journal of Psychology together with his student, Marianne Simmel.

The investigators begin with the remark that the psychology of perception has so far paid little attention to the ways in which other people are perceived. Their investigation is different from previous ones because they use not faces without situations but situations and actions without faces. They are interested not in the correctness of the responses but rather in the dependency of the responses on the stimulus configuration.

In the experiment the subjects are askes to interpret a film lasting two and a half minutes. This film shows three geometric figures (a large triangle, a small triangle and a circle or disk) which move around at differing speeds.

The only other figure in the field was a rectangle, a section of which could be opened and closed as a door is. The film was produced by a trick-film method.

The movements for the large triangle (T), the small triangle (t) and the circle (c) are specified as follows:

1. T moves toward the house, opens door, moves into the house and closes door.
2. t and c appear and move around near the door.
3. T moves out of the house toward t
4. T and t fight, T wins: during the fight, c moves into the house
5. T moves into the house and shuts door
6. T chases c within the house; t moves along the outside of the house toward the door
7. t opens the door and c moves out of the house and t an c close the door
8. T seems to try to get out of the house but does not succeed in opening the door: t and c move in circles around outside of the house and touch each other several times
9. T opens the door and comes out of the house
10. T chases t and c twice around the house
11. T and c leave the field
12. T hits the walls of the house several times: the walls break.

In his autobiography Heider describes how the film was made (p. 136 ff.):

> To make the film, we used an old moving-picture camera that could be driven manually. We had cardboard figures for our three characters and the room, and we placed them on a sheet of glass, on which the camera was focused from below. I figured out in advance exactly how far I had to move a figure to give the impression of slow or rapid movement, and we set out to work. I would place a figure; Marianne would then make the exposure; I would move the figure to the next position; and so on. It took us about six hours, working in this exposure-by-exposure fashion, to make a film that gives a perception of lively movement and takes about three minutes to project. I still remember how pleased I was at my first showing of the film. And is has been impressive the way almost everybody who has watched it has perceived the picture in terms of human action and human feelings.
>
> The film was a by-product of some work that Grace and I had done with children at the Clarke School. We were using a formboard test in which the child's task was to fit geometrical forms into the holes that had been cut in a wooden board of perhaps ten by fifteen inches. We had observed the children as they moved the forms about, sometimes trying one in a wrong hole, correcting errors, and trying another. We soon realized that just the movements of the hands holding the forms gave us a great deal of information about a child's puzzle-solving thought processes. We made moving pictures of some of these sessions so that we could study sequences of motions several times over. Then it occurred to me that the movements of the forms alone might be used to portray interpersonal situations.

Altogether Heider and Simmel performed three sub-experiments:

Experiment I: free description of the film by the subjects (N = 34)

Experiment II: structured questionnaire after showing the film (N = 36)

Experiment III: short structured questionnaire after showing the film backwards (N = 44).

In experiment I the film was shown twice. Then the subjects were instructed: to "write down what happened in the picture" (Heider and Simmel 1944, p. 245).

So, there was no indication that the symbols were intended to be people, or that they could be people.

In the second experiment (the main experiment) the subjects were instructed to interpret the movements of the figures as the actions of people. A sealed envelope contained a short questionnaire which the subjects were asked to complete after seeing the film. The 10 questions were mostly related to the character and to the intensions of the "persons".

In the third experiment the same film was shown backwards. The subjects were asked to respond to four questions.

Heider and Simmel state that in the first experiment only one of the 34 subjects described the action of the film using almost exclusively geometric concepts.

All the rest interpreted the movements as the actions of living beings, in the majority of cases as those of people, in two cases as those of birds. 19 people wrote a coherent story.

At the beginning of 1985 Wolgang Manz repeated the Heider-Simmel-study with all three experiments in the University of Düsseldorf using small groups of students as his subjects. He used a 16mm copy of the cartoon film.

The results obtained in Düsseldorf, just like those of Heider and Simmel, showed for amost all subjects that the geometric figures which change their locations in the film as time passes are not only <u>not</u> perceived as geometric figures; they are seen as acting protagonists whose movements have causes and who aim at specific goals. In the perception of the action, motives and intentions are ascribed to the people involved. The basic principle of social perception can be seen in these attributions of motives and goals, and this is something which probably happens uncounsciously. Interesingly the article by Heider and Simmel records the first occurrence of the concept of

attribution. And attribution takes place whether the figures are said to be persons or whether they retain their abstract form. The pattern of movement alone exerts an irresistible compulsion towards causal interpretation whether the figures are personified or not.

To sum up, the Heider-Simmel film was an important step in the development of attribution theory.

Comparison and assessment

The three films/film series shown come from different phases of psychology and cinematography. At the time when Köhler was making his silent films, films were the attraction at fairs and markets; when Heider filmed, both colour and sound had been added and Hollywood movies were well into their resounding success stories. But Heider's films are all simple and differ not very much in quality from Köhler's.

All three films are extraordinarily alive. They show behaviour in situ. All have proved extremely succesful as teaching aids. However, the researchers were following different aims. Köhler wanted evidence and his films were made after the investigations had been performed. Lewin wanted to catch dynamic behaviour in conflict situations in order to study it and theoretize about is. He then used the films for teaching purposes. Heider's film, on the other hand, was a stimulus for this questionnaires.

It is not surprising that the films of all three researchers generally are seen as relatively peripheral to their research, but it is equally unsurprising that in alle three cases the films were a major factor in ensuring the spread of their ideas, enhancing the development of new theories, and fostering the reputation of the researchers in the scientific community.

Finally, it should come as no surprise to us that these films were long forgotten, that some have been destroyed, and that some (hopefully) still await discovery. Psychologists and psychological historians have neglected films and their importance in the past. This should be a lesson to us for the future.

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Fig. 1: From the Köhler experiments: Chimpanzee using a stick as a pole to jump up to a banana.

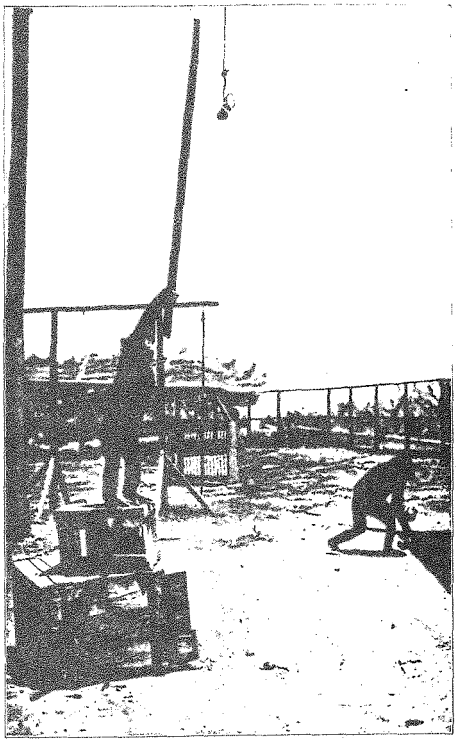

Fig. 2: Using a stick as a club to beat down a banana.

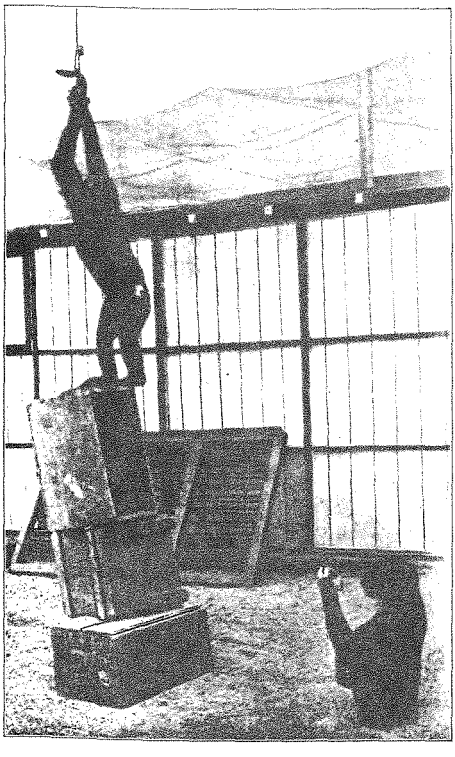

Fig. 3: Erecting a three-story structure to reach a banana.

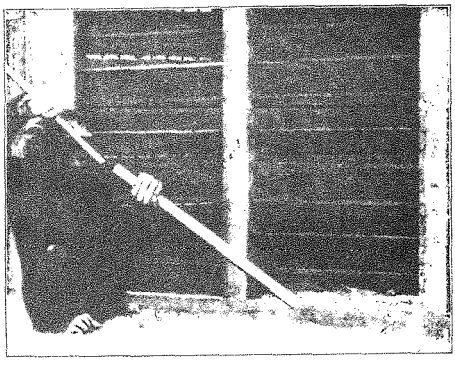

Fig. 4: Sultan making a double stick.

Fig. 5: From the Kurt Lewin films:
Hannah tries to sit down on a rock.

Fig. 6: Hans sits down on a rock by moving backwards.
Hans manages to coordinate the direction of action with the direction of attention.

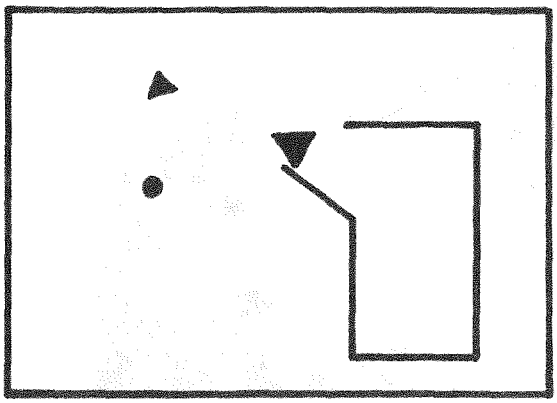

Fig. 7: Exposure-objects displayed in various positions from the Heider and Simmel film.

PSYCHOANALYSE UND FILM IN DEN ZWANZIGER JAHREN (BERLIN, WIEN). DIE IDEE IM BILD UND DAS BILD DER IDEE: VORSTELLBARKEIT UND ANSCHAUUNG.

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Summary
Studying the origins of the first psychoanalytic film Secrets of a Soul (1926) enables one to get a glimpse of the institutional and professional processes of the psychoanalytic movement. The comparison of two film projects, Secrets of a Soul in Berlin, and unknown scripts by Siegfried Bernfeld in Vienna which have been discussed for the first time in the present paper, demonstrates unreflected disagreements. It is argued that these disagreements were to influence the development of psychoanalysis.

Die erfolgreiche Behandlung der Kriegsneurosen im ersten Weltkrieg gab den Psychoanalytikern die entscheidenden Impulse zur Ausdehnung ihres Wirkungsfeldes. Auf dem Hintergrund des sozialen Elends und des politischen Chaos einer sich hektisch neu formierenden gesellschaftlichen Ordnung wurden die Anhänger Sigmund Freuds im Sinne einer "Massenanwendung" der Psychoanalyse aktiv. Dies forcierte ihren institutionellen und professionellen Entwicklungsprozess. Zentrale Bedeutung kam dabei dem Aufbau psychoanalytischer Ambulatorien als Behandlungs-, Ausbildungs- und Forschungsstätten zu.
In den zwanziger Jahren bot im deutschsprachigen Raum Berlin die besten Möglichkeiten zur Entfaltung der Psychoanalyse. Hier konnte bereits 1920 die psychoanalytische Poliklinik eröffnet werden, hier entwickelte sich der institutionelle und professionelle Prozess am ungehemmtesten. In Berlin konnte die Psychoanalyse auch von nichtärztlichen Analytikern zu Heilzwecken ausgeübt werden, es bestand Kurierfreiheit. Im Gegensatz dazu konnten in Wien die mit den Psychoanalytikern konkurrierenden psychiatrisch-neurologischen Standesvertreter und Universitätsmediziner (zum Beispiel der Ordinarius der psychiatrisch-neurologischen Universitätsklinik, Prof. Julius von Wagner-Jauregg) die restriktiven Paragraphen des

Kurpfuschereigesetzes zur Verhinderung und Verzögerung des Beginns der Arbeit des psychoanalytischen Ambulatoriums und zur Anzettelung von Gerichtsklagen gegen nichtärztliche Psychoanalytiker ausnutzen (zum Beispiel gegen Theodor Reik). (1) Berlin war in den "Golden Twenties" das Zentrum der psychoanalytischen Bewegung.

Psychoanalyse, Film und Öffentlichkeit

Das öffentliche Interesse am jungen Medium Film wie an der Psychoanalyse war enorm. Der Präsident der Internationalen Psychoanalytischen Vereinigung, Karl Abraham, schrieb am 17. Oktober 1925 in einem (unveröffentlichten) Rundbrief: "Aus Deutschland ist zu melden, dass die Diskussion der Psa. in Zeitungen und Zeitschriften nicht mehr zur Ruhe kommt. Überall findet man sie erwähnt. (...) Natürlich fehlt es auch nicht an Angriffen. Aber ohne Zweifel war das Interesse noch nie so stark wie jetzt." (s. Anhang B).

Psychoanalyse und Film galten als Ausdruck der "modernen Zeiten" und wandten sich an das Massenpublikum. Zwischen Film und Psychoanalyse entwickelten sich konkrete Verbindungen. Die Filmproduzenten wussten um die Popularität und Verwertbarkeit des sujets "Psychoanalyse". Bereits 1922 trat in dem Film von Fritz Lang "Dr. Mabuse, der Spieler" als Psychoanalytiker auf. (2) Nach der spektakulären Ablehnung des Angebotes von Samuel Goldwyn durch Sigmund Freud, versuchte nun die UFA des Geschäft mit der Psychoanalyse.

Es war im Sommer 1925 als Hans Neumann, Filmproduzent der grössten deutschen Filmgesellschaft UFA (Universal Film Aktien Gesellschaft), an die beiden Berliner Psychoanalytiker Karl Abraham und Hanns Sachs mit dem Angebot herantrat, an einem "psychoanalytischen Film" mitzuarbeiten. Verlockend und faszinierend war, was mit dem neuen Medium erreichbar schien, ungeachtet der Tatsache, dass die Psychoanalytiker wenig vom Film und die Filmleute wenig von der Psychoanalyse verstanden haben dürften. Karl Abraham erklärte in einem Brief an Sigmund Freud: "Dass Derartiges gerade mir nicht 'liegt', bedarf kaum der Erwähnung" (Freud und Abraham, 1980: 1925, 356). Als Rechtfertigung ihrer Mitarbeit an dem Filmprojekt, die einen Konflikt innerhalb der Kollegenschaft und eine Auseinandersetzung mit Sigmund Freud auslösten, schrieb Hanns Sachs in einer Begleitbroschüre zum Film: "Ein Fachmann vom Range Hans Neumanns hatte aus sich heraus den Vorsatz gefasst, etwas vom Wesen der Psychoanalyse filmisch zu erfassen und durch dieses modernste Mittel künstlerischer Gestaltung dem

grossen Publikum aller Länder nahezubringen. Es wäre uns ungeheuerlich erschienen, ein solches Unternehmen einfach zu ignorieren, wir zogen es vor, uns an seine Seite zu stellen..." (Sachs, 1926, S. 31). Für die beiden Berliner Psychoanalytiker waren vor allem Macht, Prestigegründe und die Angst, andere "wilde Analytiker", könnte den "materiellen Gewinn" (3) haben, ausschlaggebend für ihre Mitarbeit an dem Projekt der UFA.

Im Herbst 1925 schritt man in Berlin zur Verwirklichung des Vorhabens. Karl Abraham und Hanns Sachs überarbeiteten zunächst das von Hans Neumann und Colin Ross (einem bekannten Reiseschriftsteller) verfasste Drehbuch und standen bei den Dreharbeiten beratend zur Seite. Sachs schrieb ein leicht verständliches Begleitheft mit dem Titel Psychoanalyse. Rätsel des Unbewussten.

Die Bezeichnung "psychoanalytischer Film" und das Auftreten eines behandelnden Psychoanalytikers im Film täuschten darüber hinweg, daß es den beiden Mitarbeitern nur in bescheidenem Masse möglich war, psychoanalytische Erkenntnisse im Film unterzubringen und auszudrücken und die dem neuen technischen Medium innewohnende Gestaltungsmöglichkeiten für die Darstellung psychoanalytischen Denkens zu nutzen. Für die Psychoanalyse blieb ein Reklame- und Werbeeffekt, der sie dem "grossen Publikum" näherbrachte. Den potentiellen Patienten im Publikum wurde der Weg auf die Couch gewiesen. Die psychotherapeutische Kompetenz der Psychoanalyse wurde gegen die zunehmende Konkurrenz (5) in diesem Bereich unterstrichen.

Sigmund Freud lehnte ein derartig dem Zeitgeist entgegenkommendes Unternehmen strikte ab. "Die Verfilmung lässt sich so wenig vermeiden wie, scheint es, der Bubikopf. Aber ich lasse mir selbst keinen schneiden und will auch mit keinem Film in persönliche Verbindung gebracht werden." (Freud, 1976, S. 224) schrieb am 14. August 1925 Freud an Sandor Ferenczi.

Jahre zuvor hatte Samuel Goldwyn, der bekannte Filmproduzent aus Hollywood, Sigmund Freud für die Mitarbeit an einer Filmproduktion 100.000 Dollar angeboten. Es ging dabei um die Darstellung der berühmtesten Liebesgeschichten aller Zeiten, angefangen mit Antonius und Kleopatra. Freud lehnte ab. Seine Haltung blieb auch gegenüber dem deutschen Projekt ablehnend, "es scheine ihm nicht möglich, seine abstrakten Theorien durch das plastische Medium des Films darzustellen." (Jones, 1962, S. 141f). Seine Kontroverse mit Karl Abraham war nur die Spitze des Eisbergs innerhalb der heftigen Reibereien und

Konkurrenzen zwischen Wiener und Berliner Psychoanalytiker-Kollegen im Filmgeschäft. Die streitbaren Wiener, Josef Storfer und Siegfried Bernfeld, die ihrerseits ein Filmprojekt zu realisieren versuchten, unterlagen dabei ihren Berliner Rivalen. (6) Finanzielle Spekulationen gehörten zum Gerangel. Willi Hoffer, ein Freund und Kollege Siegfried Bernfelds, machte sich in einem Brief an ihn über diese Affäre lustig: "Hallo - alter Junge was ist los mit Dir, hallo was ist los mit Dir, was tust Du alter Junge? Hallo - der Abraham und Sachs sollen Dir die ganze business wegnehmen? Hallo - die ganzen 3 Milliarden wollen sie Dir rauben. Das darf - hallo das darf nicht sein. Du muss ihnen - hallo - schreiben, hallo - schreiben musst Du ihnen. Sie müssen Dich teilnehmen lassen an dem - hallo - an dem Geschäft." (7)

Aus der Teilnahme wurde nichts. Neben Bernfelds Entwurf zu einer filmischen Darstellung der Freudschen Psychoanalyse im Rahmen eines abenfüllenden Spielfilms blieb auch seine "Idee eines Films mit psychoanalytischen Perspektiven". Drei Welten in einem Zimmer. Kindervariationen auf ein erwachsenes Thema im Nachlass erhalten. Er zeigt die emotionalen Verhältnisse und Verhäkelungen einer "Normalfamilie des bürgerlichen Mittelstandes" aus dem unterschiedlichen Erleben und Phantasieren der beteiligten drei Kinder. Erinnerungen an Themen und Szenen aus Filmen von Louis Bunuel werden beim Lesen des Manuskripts wach.

Psychoanalyse als Behandlungsverfahren versus Psychoanalyse als Forschungsmethode.

Zunächst zum Film Geheimnisse einer Seele, der in Berlin entstand. Das Bild, das mit der Spielhandlung von der Psychoanalyse vermittelt wird, macht vor allem ihr therapeutisches Potential offensichtlich. Im Filmskript und im Begleittext dominiert "das Seelenheil": "Heilung", "seelischer Reinigungsprozess", "Beherrschung der gefährlichen und hässlichten Triebe des Urmenschen", "Selbstkontrolle", "Einheit der Persönlichkeit" bestimmen die Behandlungsziele, die die Gesundung und Anpassung des seelisch Kranken gewährleisten sollen. Die Analyse der Eifersucht und Kinderlosigkeit des Patienten verbleibt im Rahmen einer einfachen kausal-genetisch rekonstruierten Kindheitsszene. Nicht berücksichtigt wird die Dynamik der Übertragung. Die analytische Arbeit stoppt gerade dort, wo die Psycho-Analyse eigentlich erst beginnen sollte: bei den grundlegenden sexuellen Konflikten, der

ödipalen Problematik, die zwar im Traum über Sexualsymboliken angedeutet, aber auch nicht im Filmbegleittext die Bedeutung z.B. der infantilen Sexualität in der Psychoanalyse unterstreicht. Sexualität wird, von unbedeutenden Ausnahmen abgesehen, auf ihre Fortpflanzungsfunktion reduziert. Von allem was Lust bereitet, also Sexuellem im Freudschen Sinne, fehlen die Spuren im Film. Kein Wunder, wo doch die staatliche Filmzensurbehörde keinerlei sexuelle und erotische Darstellungen durchliess und selbst das Wort "aufgedeckt" in einem Zwischentext im Film verbot, das sie offensichtlich in sexueller Bedeutung verstand. (8) Dennoch waren die Geheimnisse einer Seele für Jugendliche nicht zugänglich ("Jugendverbot"). Eine derartig zurechtgestutzte Psychoanalyse passte sich dem Publikumsgeschmack und der billigen Filmstory an und war somit kein Hindernis für den grossen Erfolg des Films, der sicher den künstlerischen, technischen und regiemässigen Leistungen entsprach.
Für die Realisierung des Projektes wurde als Regisseur der Österreicher Georg Wilhelm Pabst (1887-1967) engagiert, der zuvor mit seiner Verfilmung von Hugo Bettauers Roman Die freudlose Gasse mit Greta Garbo als Hauptdarstellerin seinen ersten Erfolg feierte. Werner Krauss spielte in den Geheimnissen einer Seele die Hauptrolle des phobischen, zwanghaften Chemikers; Ruth Weyher die Ehefrau. Die Bauten stammten von Ernö Metzner. (Siehe Anhang D). Die Uraufführung im Berliner Gloriapalast fand am 24. März 1926 statt.

Das Bild der Psychoanalyse als "Dienstmagd der Psychiatrie" (wie Sigmund Freud ihre Stellung in den USA beschrieb) bestätigte sich auch indirekt über die Reaktionen auf den Film, nachdem er in den USA auf einem psychoanalytischen Kongress aufgeführt wurde. Der Moskauer Schauspieler Pawel Pawlow, der im Film den Psychoanalytiker darstellte, wirkte anscheinend so überzeugend, dass beim Regisseur G.W. Pabst Briefe aus Amerika eintrafen, in denen der Name des Arztes verlangt wurde, um ihn nach den USA zu berufen.
Die Geheimnisse einer Seele sind ein Beispiel für die Trivialisierung und Popularisierung der Psychoanalyse und vielleicht prototypisch für ihre spätere Rezeption im amerikanischen Trivialkino. Das Projekt UFA-Abraham/Sachs zeigt, wie die von den Psychoanalytikern hingenommenen und nicht reflektierten äusseren Bedingungen (Filmindustrie, staatliche Zensur, Zeitgeist, eigene Konkurrenzsituation etc.) inhaltliche Konzessionen zur Folge hatten, die die Substanz der

Psychoanalyse kosteten. Ein Schicksal, das sich für die deutsche Psychoanalyse im ganzen in ihrem Anpassungsprozess an den Nationalsozialismus wiederholte.

Anders als im Berliner Filmprojekt versuchte Siegfried Bernfeld mit seinem zur selben Zeit entstandenen Entwurf zu einer filmischen Darstellung der Freudschen Psychoanalyse im Rahmen eines abendfüllenden Spielfilms die Auseinandersetzung mit dem neuen Medium. Dieser Versuch ist allerdings nur als Grundlage zu einem Drehbuch von ihm ausgeführt worden. Als lästiger Konkurrent im Filmgeschäft wurde Siegfried Bernfeld ausgeschaltet. (9) Bei der story Bernfelds standen die Träume eines jungen Mannes im Mittelpunkt, bei dem sich akute Liebes- und Arbeitsschwierigkeiten eingestellt hatten. Er beginnt sich für seine Träume zu interessieren und wird in seinem Erkenntnisdrang vom Analytiker unterstützt. Er wird aber nicht zum Patienten des Psychoanalytikers. Psychoanalyse wurde hier als Methode der Erkenntnis vermittelt und nicht als Psychotherapie. Bernfeld bemühte sich um eine authentische und dem Erkenntnisstand adäquate Darstellung des psychoanalytischen Denkens. Der Berliner Film-Analytiker agierte als Spezialist zur Aufdeckung und Beherrschung der "störenden" Triebäusserungen (10), der Bernfeldsche Analytiker steht als Anwalt verborgener Triebkräfte und unterdrückter Triebwünsche zur Verfügung. Im Entwurf von Bernfeld wurden die um den ödipalen Konflikt gruppierten Äusserungsformen der kindlichten Sexualität augenfällig gemacht, hier treiben die Kinder ihre "Doktorspiele", während es bei Abraham und Sachs bei unverfänglichen "Vater- und Mutter-Spielen" bleibt. Bernfeld versuchte das Freudsche Schema des psychischen Apparats von 1923 (das Strukturmodell von Es, Ich und Über-Ich) (11) in einer Raumkonstruktion sichtbar zu machen (12), während bei den Geheimnissen einer Seele das alte topische Modell (Unbewusstes, Vorbewusstes, Bewusstes) einen vagen theoretischen Hintergrund abgab. (13) Parallelen in den beiden Filmgeschichten finden sich zur Rolle und Funktion der Frauen, die ohne eigene Handlungsmöglichkeiten bis zum happy-end auskommen müssen.

ANMERKUNGEN

(1) Als ein Beispiel die Stellungnahme des Wiener Ärztekammerpräsidenten Dr. Thenen: "... In Wien gibt es einige geradezu privilegierte Kurpfuscher, denen man nicht beikommen kann. Drei dieser Herren, die ich als offizielle Kurpfuscher bezeichnen möchte, erfreuen sich offensichtlich so hoher Protektion, dass sogar die Sanitätsbehörden erfolglos gegen sie eingeschritten sind (...). Trotzdem geschieht dieses Vergehen gegen unsere Standesehre immer wieder. Jetzt muss aber damit endgültig Schluss gemacht werden. Wir führen den Kampf gegen die Kurpfuscherei in aller Stille schon seit Jahren. Wir haben wiederholt auch öffentlich Stellung in dieser Frage genommen. Unsere nächste Stellungnahme wird die massgebenden Organe aber wahrscheinlich doch zu einem energischen Einschreiten veranlassen. Was den Fall des Psychoanalytikers Dr. Reik betrifft, so stehen wir auf dem Standpunkt. dass er nicht das Recht hat, Behandlungen durchzuführen. Er hat seine Patienten recht lange genug gehabt und ein ganz hübsches Honorar damit verdient - aber auch damit muss es jetzt endgültig vorbei sein."
In: Neues Wiener Journal. 14. Juli 1926.

(2) In der Wiener Psychoanalytischen Vereinigung gab es dazu eine bemerkenswerte Anfrage:
"Federn macht auf die böswillige Darstellung der Psychoanalyse im Film Mabuse der Spieler aufmerksam, regt an etwas gegen solche Darstellungen zu unternehmen. Freud lehnt entschieden ab."
Protokoll der Sitzung der WPV vom 1. November 1922. Siegfried - Bernfeld-Archiv. Library of Congress. Washington D.C.

(3) Vgl. Freud, Abraham, Briefe 1907-1926. Frankfurt am Main, 1980. 356.

(4) Der Analytiker im Film wurde wie ein "streetworker" aktiv; er verfolgte den Neurotiker und "stellte" ihn vor seiner Haustüre. Der Zwischentext zu dieser Filmszene lautet: "Sie haben diesen Schlüssel im Café liegen lassen! Sicherlich haben Sie einen Grund Ihr Haus ungern zu betreten!" - "Woher wissen Sie das??" - "Es gehört zu meinem Beruf."
Weder der Film noch das Begleitheft gibt einen Hinweis auf die psychoanalytishe Poliklinik in Berlin, wo Gratisbehandlungen durchgeführt wurden. In "Bettauers Wochenschrift" (1924-1927), einer weitverbreiteten und sehr populären Wiener Zeitschrift, die um Sexualaufklärung bemüht war und einen umfangreichen Leserbriefkasten beinhaltete, finden sich in jeder Ausgabe in Antworten auf hilfesuchende Leser Hinweise auf das Wiener psychoanalytische Ambulatorium.

(5) Zum Beispiel fand 1925 der 2. Internationale Kongress für Individualpsychologie in Berlin statt; 1926 der erste allgemeine Kongress für ärztliche Psychotherapie in Baden-Baden. Vgl. Lockot, 1985, p. 55.

(6) Anna Freud schrieb in einem Brief an Siegfried Bernfeld am 10. September 1925: "Zu der Filmangelegenheit sage ich nichts mehr, denn es ist schon zu viel darüber gesprochen und geschrieben worden. Ich persönlich habe ja, wie Sie wissen, überhaupt keine Vorliebe für analytische Films, aber es hat mir ehrlich leid getan, dass für manche der Beteiligten so viel Aerger aus der Sache gekommen ist." Siegfried Bernfeld Archiv. Library of Congress. Washington D.C.

Ernest Jones erinnert sich in seiner Freud-Biograpie: Hanns Sachs beschwerte sich über den damaligen Verlags-direktor Josef

Storfer, "dass er einen von ihm selbst stammenden Zeitungsartikel, in dem er dem Streifen allen Wert abspreche, überall weitergebe. Dann verfasste Siegfried Bernfeld ein eigens Drehbuch und bot es mit Storfer verschiedenen anderen Gesellschaften an. ... Dies führte zu einer bewegten Kontroverse, in deren Verlauf sich Abraham über die Vertrauenswürdigheit der beiden Wiener nicht gerade die beste Meinung bildete." (Jones, 1962, p. 142).

Bernfelds Arbeit trug den Titel Entwurf zu einer filmischen Darstellung der Freudschen Psychoanalyse im Rahmen eines abendfüllenden Spielfilms. Ein Auszug ist im Anhang (C) wiedergegeben.

Nach einer persönlichen Mitteilung von Ernst Federn (Triest, 1985) war Siegfried Bernfeld bis an sein Lebensende über die Film-Affäre verärgert.

(7) Siegfried Bernfeld Archiv. Library of Congress. Washington D.C.

(8) Vgl. Zensurkarte der Film-Prüfstelle Berlin, Prüf. Nr. 12226 vom 25. Januar 1926. "Aufgedeckt" musste durch "bewusst gemacht" ersetzt werden.

(9) Noch während der Spielzeit des Films schrieb Siegfried Bernfeld unter dem Titel Geheimnis der Seele. Psychoanalyse als Forschungsmethode einen Artikel in der Vossischen Zeitung, indem er einen Abriss der Psychoanalyse wiedergab, den man als Gegendarstellung zum Film werten muss. Möglicherweise wurde als Reaktion auf das kritisierte Filmprojekt und die eigenmächtige Mitarbeit der beiden Berliner Analytiker das "Propaganda-Komitee" im Berliner Institut gegründet, das sich einer "seriösen" Öffentlichkeitsarbeit widmen sollte. Vgl. Harnik, 1930.

(10) Vgl. Anmerkung 4.

(11) Sigmund Freud hatte schon bei der graphischen Darstellung des Strukturmodells grosse Bedenken geäussert. "Urteilen Sie nicht zu hart über einen ersten Versuch, das so schwer erfassbare Psychische anschaulich zu machen".
(Freud, 1933, p. 516).

(12) Vgl. Anhang (C)
Bernfeld hatte sich dazu von den avantgardistischen Theatertechnikern Anregungen verschafft. Im September 1924 fand in Wien die Internationale Ausstellung neuer Theatertechnik statt, für die der Wiener Avantgardist Friedrich Kiesler die radikalsten Theaterbeiträge aus ganz Europa zusammentrug und als Erbauer der Raumbühne Aufsehen erregte. (Vgl. Bogner und Lesak, 1985).

(13) Mit diesem Modell blieben ja wirklich einige Fragen und Probleme, die sich aus der klinischen Praxis stellten, ungelöst, "rätselhaft" bzw. "geheimnisvoll".

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A

A N H A N G

Diese Presseaussendung Josef Storfers war die Grundlage für einen Zeitungsbericht, auf den Ernest Jones bei seiner Darstellung der Kontroverse verweist. (s. Anmerkung 6)

Internationaler Psychoanalytischer Verlag

Wien VII. Andreasgasse 3

Sehr geehrte Schriftleitung!

Nachfolgende Notiz dürfte für Ihre Leser gewiss von Interesse sein. Wir stelles es Ihnen anheim, sie zu veröffentlichen, bezw. von den dort gemachten Mitteilungen beliebigen Gebrauch zu machen.

Hochachtungsvoll

In der letzten Zeit sind aus den Kreisen der Filmindustrie verschiedene Projekte "psychoanalytischer Filme" aufgetaucht. Bekanntlich hat einer der grössten amerikanischen Filmindustriellen Prof. Freud selbst zur Autorisation solch eines Filmes vergeblich zu gewinnen versucht. Da das Projekt eines solchen Filmes neuerlich wieder von verschiedenen Seiten, auch in Deutschland aufgegriffen werden soll, hat der Internationale Psychoanalytische Verlag in Wien, der die zentrale Publikationsstelle der ganzen psychoanalytischen Bewegung, auch der Verleger der Schriften von Prof.Freud ist, sich entschlossen, selbst für die Herstellung eines psychoanalytischen Filmes zu sorgen, um dadurch die Gefahr einer irreführenden Darstellung, bezw. einer anstössigen oder unsinnigen Verballhornung auszuschalten. Der Internationale Psychoanalytische Verlag hat durch einen bekannten Psychoanalytiker das Manuskript einer authentischen Darstellung der Freud-schen Lehre im Rahmen eines abendfüllenden Spielfilmes herstellen lassen und wird für die rascheste Verfilmung Sorge tragen. Der Umstand, dass der Film nicht nur neben der Firma der betreffenden Filmgesellschaft auch die des Internationalen Psychoanalytischen Verlages tragen wird, sondern dass dieser Verlag die Herstellung des Filmes selbst kontrollieren wird, darf zur Hoffnung berechtigen, dass das volle Mass authentischer Darstellung erreicht sein wird.

Quelle: S. Bernfeld-Archiv. Library of Congress. Washington D.C.

B

Rundbrief von Karl Abraham an die Mitglieder des Komités.
(Siehe Fortsetzung nächste Seite.)
Quelle: Karl Abraham-Archiv. Library of Congress. Washington D.C.

NTERNATIONALE PSYCHOANALYTISCHE VEREINIGUNG
NTERNATIONAL PSYCHO-ANALYTICAL ASSOCIATION

Präsident
Dr. KARL ABRAHAM 17.10.25
Berlin, Grunewald, Bismarckallee 1

Sekretär
Dr. MAX EITINGON
Berlin W. 10, Raudstraße 9

Liebe Freunde,

Dieser Brief geht mit einiger Verspätung ab. Als herkömmlicher Ver-
sser der Rundbriefe kann ich erst heute wieder in Funktion treten und
utze nun gern die Gelegenheit, auf diese Weise meine Wiederherstellung
beweisen. Natürlich fühle ich noch die Nachwirkungen der Krankheit, wie
nicht anders sein kann, wenn man 5 Wochen gelegen hat, aber die Symptome
r Bronchopneumonie sind, im Gegensatz zur Rekonvalescenz im Juni, dieses
l ganz beseitigt, und ebenso macht die Gallen-Affektion mir keine Beschwer
r. Jetzt heisst es vorsichtig abwarten, ob die Infektion endgiltig über-
den ist. Ev. wird in etwa 14 Tagen noch eine Autovaccin-Behandlung ein-
eitet werden, um Rückfälle zu verhüten. Genug von mir! Nur zwei Tatsach will
noch erwähnen. Während der ganzen Zeit hat van Emden, der seine Ferien in
lin verbrachte, sich in einer einzigartigen Weise um mich bemüht, die ich
nicht vergessen werde. Und zweitens war von grösster Bedeutung für mich da
reifen von Fliess in die Behandlung. Ich habe einen Begriff von seinen
z ungewöhnlichen Eigenschaften als Arzt bekommen. Er wiegt wirklich die
l ordentlichen Professoren der inneren Medizin auf, die wir in Berlin haben, und
n man noch einige ausserordentliche als Zuwage giebt, so macht das auch no
nt viel. Nebenbei hat der ganze Verlauf meiner Krankheit seine Perioden-
re in frappanter Weise bestätigt.

Max befindet sich auf Reisen, z.Zt. in Florenz. Hanns ist während der letzten Zeit mit der Arbeit am Film sehr belastet gewesen, zumal ich ihn ja nicht unterstützen konnte. Allem Anschein nach gelingt das Werk gut, und in etwa 3 Wochen werden die photographischen Aufnahmen beendet sein. Da soeben schon Ernest's Brief angekommen ist, so kann ich seine Frage schon beantworten. Von dem Bernfeld-Film hat man nichts mehr gehört. Es hat wohl niemand Freude daran, die Angelegenheit nochmals aufzurollen, aber kurz erwähnt mag doch sein, dass die ganze Sache sich als ein Bluff herausgestellt hat. Das Angebot einer Wiener Firma war fingiert, um uns zu bluffen. Jener Firma gegenüber behaupteten St. & B., sie hätten ein Angebot von der Ufa und Cäsmen in Homburg mit amerikanischen Interessenten zusammen, die ebenfalls hohes Gebot machten. Und Aehnliches mehr. Aber nun ist ja äusserlich die Ruhe wiederhergestellt, und ich habe gestern in andrer Angelegenheit einem Brief an Storfer geschrieben, als wenn nie etwas geschehen wäre. Ich hoffe, dass nun alle weiteren Störungen des Friedens vermieden werden.

Aus Deutschland ist zu melden, dass die Diskussion der Psa. in Zeitungen und Zeitschriften nicht mehr zur Ruhe kommt. Ueberall findet man sie erwähnt. Ein Lokalblatt in Charlottenburg druckte kürzlich aus dem psa. Almanach Herrn Prof.'s Artikel über die Widerstände ab. Ein Jahrbuch „Minerva", das über alle wissenschaftlichen Gesellschaften & Institute berichtet, verlangte kürzlich genaue Angaben über unsre Vereinigung. Natürlich fehlt es auch nicht an Angriffen. Aber ohne Zweifel war das Interesse noch nie so stark wie jetzt.

Landauer in Frankfurt, der bisher zur Wiener Gruppe gehörte, will demnächst zur unsrigen übertreten. (Ist inzwischen erfolgt.)

~~Alle Einzelheiten über die hies. Vereinigung usw. vertagen wir auf~~ das nächste Mal. Mit herzlichen Grüssen

Abraham

P.S. Mit Rücksicht auf obigen Brief dürfte eine weitere Diskussion überflüssig sein. Ich bin der A. sehr dankbar, wenn er jetzt etwas Mitteilenswertes dabei sein kann. Kurz. Kurz. in Eigenheit

Sach.

C

Auszug aus dem "Entwurf zu einer filmischen Darstellung der FREUDSCHEN PSYCHOANALYSE im Rahmen eines abendfüllenden Spielfilms" von Siegfried Bernfeld. S. 24-27.

Quelle: S.Bernfeld-Archiv. Library of Congress. Washington D.C.

Das Mädchen in ihrem Zimmer. Qui pro quo vor ihrem Schlafengehen, indem sie an Hand der Dokumente ihre Beziehungen zum Träumer sich und dem Zuschauer klar macht, dass er sie offenbar liebt und sie solcher Liebe würdig wäre. Ihr und dem Zuschauer bleibt völlig rätselhaft, warum er nichts dergleichen tue.

Im Wohnzimmer des Träumers. Es ist tiefe Nacht. Der Träumer sitzt vor Freuds Buch, das beinahe zu Ende gelesen ist. Er sinnt übermüdet über den Sätzen, die von Zensur und Unbewusstem handeln. Das Lesezeichen, eine Visitkarte, fällt auf die gelesene Seite. Der Träumer betrachtet sie gedankenlos, es formieren sich auf ihr die Buchstaben: Professor Dante Alighieri, Unterweltscicerone. Der Träumer sieht erstaunt auf. Vor ihm steht Dante mit einem Lorbeerkranz, ganz wie die Büste auf seinem Tisch. Massloses Erstaunen des Träumers. Dante sagt mit liebenswürdigstem Lächeln: Ich bin nur verdichtet, kommen Sie! Er nimmt seinen Kopf ab, wie man freundschaftlich winkend seinen Hut zieht und ist nun der lachende Psychoanalytiker. Er zieht den Kopf wieder an. Der Träumer steht auf und sie gehen in das Unbewusste.

Der Gesammtschauplatz., auf dem der Rest des Aktes nun spielt, der aber meistens nur in Ausschnitten sichtbar ist, ist etwa der folgende: Nach Art der Raumbühne drei Schauplätze übereinander. Der oberste undeutlich durch halbkreisförmiges Gewölbe gedeckt, hat rechts statt eines Fensters einen halbkugelförmigen gläsernen Ausbau (ein Auge), der durch eine Jalousie, wie Augenlid und Wimper schliessbar ist. In diesem Glasvorbau ist mit dem Objektiv nach aussen, ein kinematographischer Aufnahmeapparat in automatischer Bewegung. Das Filmband geht durch ein offenes, in den Boden eingelassenes Entwicklerbad und von dort ohne Ende in den Kinoprojektionsapparat, dessen Objetiv nach innen oben gerichtet ist. An der Decke erscheinen die Bilder der Aussenwelt

Ganz links ist der Plafond durchbrochen durch eine ohr- und rauchfangförmrige Gramophonempfängermuschel. Der Gramophonapparat spricht durch ein Megaphon in den Innenraum. In der mitte ein hohes Katheder, auf dem eine reichhaltige Apparatur von Telephonen und Glocken angebracht ist. Hinter dem Katheder sitz mächtig, streng, furchtbar und aufmerksam der Vater des Träumers. Auf halber Höhe des Katheders eine Estrade, Durch Treppen mit dem Fussboden des Obergeschosses verbunden. Auf der Estrade lagern auf mannigfaltigen Liege- und Sitzgelegenheiten die erlaubten Wünsche. Jeder hat eine Radioantenne auf dem Kopf , mit der er mit dem Vater Ueberich verbunden ist. Das Mittelgeschoss, durch Lift mit dem Obergeschoss verbunden, ist eine ungeheure Registratur von Gramophonplatten und Filmrollen. Der Lift trägt die Aufschrift: Zum Gedächtnis. Das Untergeschoss ist durch eine Falltür und Leitern direkt mit dem Obergeschoss verknüpft. Vor der Falltür im Obergeschoss eine Anzahl von Polizisten und Aufschrifttafeln: Unbefugten ist der Eintritt strengstens verboten. In der Höhe des Mittelgeschosses an den Leitern eine Sperre mit Polizisten und der Aufschrift: Aufgang ohne Austrittsvisum strengstens verboten. Das Untergeschoss ein Mittelding zwischen Gefängniskeller und Tartaros. Zu tiefst unten die infantilen verbotenen Wünsche an Ketten geschmiedet. In den höheren Regionen als gewöhnliche Gefangene die sonstigen unerlaubten Wünsche.

Auf diesem Schauplatz macht Dante dem Träumer allgemeine Erklärungen entsprechend den Funktionen der Freudschen psychischen Apparate. Er zeigt , was geschieht, wenn dei Seele einschläft: das Ueberich kommandiert: Schlafen! Die Rollbalken werden herabgelassen, das Gramophonohr von aussen mit einem Watepfropfen verstopft. Die Polizisten lagern sich, das Ueberich ~~xxxx~~ nickt am

Katheder ein und die Wünsche schlafen. Ein und der andre der unerlaubten Wünsche steht vom Bett auf, tanzt im Raum herum, zupft die Schlafenden, verführt gelegentlich einen von ihnen zum Mittanzen. Das Ueberich weist sie immer wieder aufs neue zurecht, wie Kinder, daß nicht schlafengehen wollen. Mit bloss vorübergehendem Erfolg. Besonders zwei oder drei von den Wünschen, z. B. die Liebe zur Kunst, gekleidet wie die Tanagrafigur, ist nicht zur Ruhe zu bringen, da sie sich aber vor dem Ueberich versteckt, lässt man sie gewähren. Aus dem Gedächtnis holt sie Filmrollen, setzt den Projektionsapparat in Bewegung und freut sich der Bilder. Es ist aber nur ein kleines Stückchen der Projektionsfläche beleuchtet, das Ueberich blinzelt hin und lässt es geschehen. Indessen ist es im Tartaros schrecklich lebendig geworden, manche der Angeketteten haben sich frei gemacht, verstecken sich hinter den leichten Gefangenen und drängen gegen die Passstelle. Alle voran die Liebe zum Mädchen, hinter der sich ein kleines Kind - ein Mädchen - versteckt. Die Liebe zum Mädchen hat die Gestalt des Mädchens und entwischt den Polizisten, daß schlaftrunken sind, das Visum nicht lesen können und dem Mädchen ins Gesicht leuchtend es nicht erkennen. Sie mit andern stürmt nach der Falltür. Nach kurzem Kampf mit den ermüdeten Polizisten öffnet sich eine Spalte der Falltür und die unerlaubten Wünsche lauern hinter ihr. Wie die Tanagrafigur an der Spalte vorübertanzt, ergreift das Mädchen die Gelegenheit, versteckt sich hinter ihr und gelangt ins Obergeschoss. Polizisten und Ueberich halb erwacht, forschen misstrauisch, entdecken aber nicht den Betrug. So schlafen sie wieder ein; das Mädchen dirigiert die Figur und leitet die Auswahl der Filmrollen. Wie das Kind, das sich hinter ihr versteckt hat (des Träumers Schwester) mutig geworden, das Dirigieren übernimmt, wird sie bei einer unvorsichtigen

Wendung dem Ueberich sichtbar. Es erwacht steil und kommandiert: Alarm, Angst, Erwachen! Der Rolladen springt auf, der Wattepfropf wird herausgestossen, es wird licht, die Wünsche erwachen und rennen mit Angst durcheinander, die Polizisten beziehen ihre Posten, die unerlaubten Wünsche werden in ihr Gefängnis gejagt und die Ruhestörerin arretiert. Das Kind wird ihr entrissen und in den Tartaros gesperrt, das Mädchen soll in ihr Gefängnis gejagt werden, der Träumer aber rettet sie. Er erwirkt für sie ein Visum des Ueberichs und geleitet sie an die Estrade der erlaubten Wünsche. Der Träumer erwacht vor dem aufgeschlagenen Buch, lächelt sehnsüchtig und froh und schickt sich an schlafenzugehen.

D

Ankündigung des Films in: Sachs, Hanns: Psychoanalyse. Rätsel des Unbewußten. Berlin: Lichtbild-Bühne, o.J. (1926). S. 2

Neumann-Film der Ufa

Geheimnisse einer Seele

Ein psychoanalytisches Kammerspiel

Manuskript: Colin Roß, Hans Neumann

Psychoanalytische Mitarbeiter:
Dr. Karl Abraham †, Dr. Hanns Sachs

Regie: G. W. Pabst

Musik: Siegfried Schultz
Dekorationsentwürfe: Ernö Metzner
Aufnahmeleitung: Richard Ortlieb
An der Kamera: Guido Seeber, Curt Oertel, Robert Lach

PERSONEN:

Der Mann . . . . . Werner Krauß
Die Frau . . . . . Ruth Weyher
Die Mutter . . . . . Jlka Grüning
Der Vetter . . . . . Jack Trevor
Der Arzt . . . . . Pawel Pawlow
Die Assistentin . . . . . Hertha von Walther
Die Dienerin . . . . . Renate Brausewetter

Ort der Handlung: Berlin / Zeit: Gegenwart

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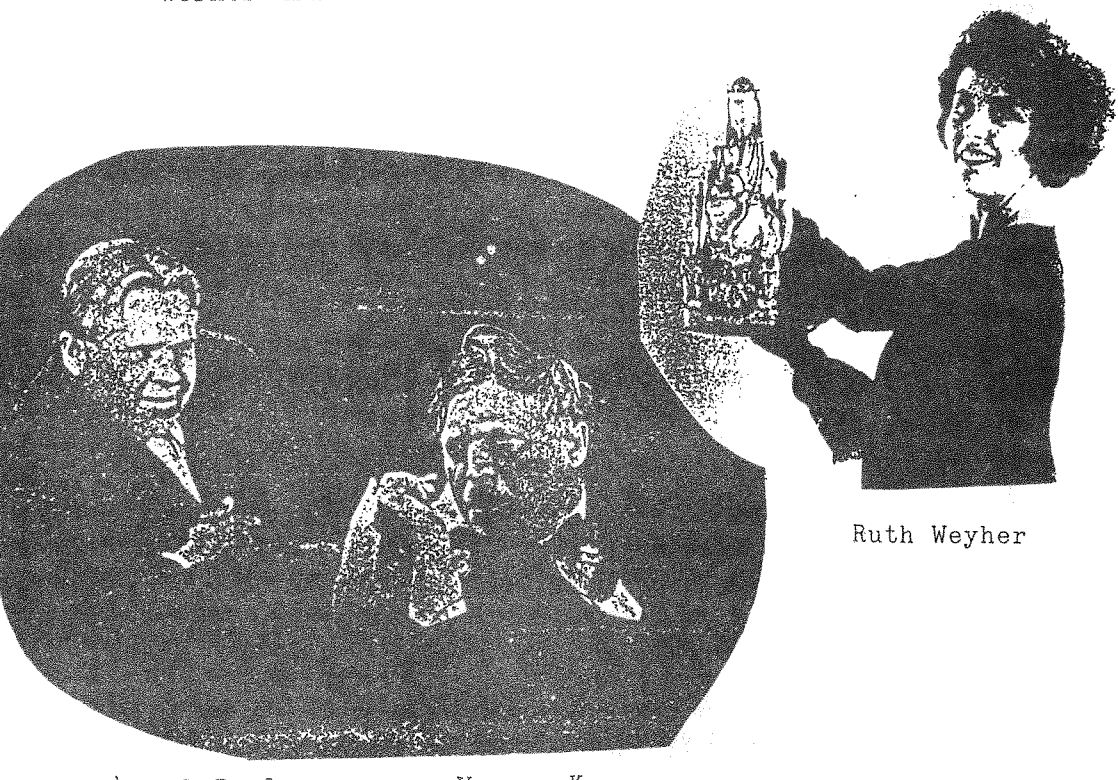
Werner Krauss



Ruth Weyher

Pawel Pawlow Werner Krauss

LES CONSÉQUENCES ÉPISTÉMOLOGIQUES D'UNE POLÉMIQUE: PSYCHOLOGIE ET TÉLÉPATHIE AUX ETATS-UNIS A LA FIN DES ANNÉES TRENTE

François Dachet
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Summary

Why does psychology argue so seriously against para-psychology? Is it to persuade those who share the belief in thought transference of their error? Or isn't it rather to convince itself of the impossibility of such a phenomenon?

Examining the first polemic directed against the Extra-Sensory-Perception (E.S.P.) experiments of J.B. Rhine in the United Stated during the thirties, we propose the following answer: parapsychological belief is the converse, the hidden face of the empirical epistemology which has become dominant in psychology since the development of behaviorism. This hypothesis is examined by taking the arguments used by psychologists to discount parapsychology and contrasting them to the background of the theoretical and political questions arising during the same period.

La publication des ouvrages de J.B. Rhine (1) et l'audience assurée à l'expérience de perception extra-sensorielle organisée par la fondation Zenith du 26 Septembre 1937 au 2 Janvier 1938, semblent avoir décidé certaines revues psychologiques d'orientation strictement scientifique à ouvrir leurs colonnes à une polémique sur ce thème. Chaque dimanche soir entre ces deux dates, la Radio Zenith Corporation organisait un "test télépathique" au cours d'un programma radiophonique "consisting of dramatised examples of the retardation of progress through intolerance and the presentation of personnal telepathic experiences". (Goodfellow, 1938, p. 601). D'un paquet composé uniquement de deux sortes de cartes (des blanches et des noires, par example), cinq cartes étaient successivement tirées au hasard. Le résultat de ces tirages n'était connu que de dix étudiants de la North-Western University, réunis à part dans les studios, et supposés être les émetteurs télépathiques. Après chaque tirage,

signalé sur les ondes par une sonnerie, les auditeurs étaient invités à inscrire ce qu'ils pensaient être la carte tirée, puis à envoyer leurs réponses par courrier. Selon les semaines le nombre de réponses a varié de 1605 à 20367.

Ce rappel n'est pas dévolu à l'anecdote. Un seuil minimum de diffusion dans le grand public semble bien être nécessaire pour que la psychologie s'occupe ouvertement de questions ordinairement laissées à leur ombre. Ce motif est d'ailleurs très souvent rappelé par les auteurs (2) des articles. Ceux-ci rappellent les principes qui fondent une approche scientifique dans la discipline, tant en ce qui concerne les objets que les méthodes. A partir de cette délimitation de la bonne façon d'aborder les données du problème, ils mettent en garde le lecteur contre les confusions possibles. Ce lecteur est bien sûr un lecteur fictif, puisque les lecteurs réels des revues spécialisées sont en principe prévenus. Et les psychologues ne semblent pas douter de ce que seule la naïveté de cet interlocuteur bien particulier -image qu'ils se donnent du public- soit à la source des confusions entretenues entre psychologie et parapsychologie.

C'est ce que ce travail se propose de questionner. Car l'étonnant n'est pas d'abord que la parapsychologie soit confondue avec la psychologie; mais que cette dernière se sente assez concernée par cette assimilation pour s'en défendre au prix d'une argumentation serrée. Car dans ce domaine, toute tentative de réfutation n'est-elle pas avant tout preuve de croyance?

Les arguments de la psychologie contre l'existence des phénomènes parapsychologiques

Tout débat suppose l'acceptation par les protagonistes d'un espace commun d'échange des arguments. En l'occurence, cet espace est celui que fournissent les notions de chance ou de hasard. Les défenseurs de la parapsychologie arguent de l'improbabilité des résultats qu'ils obtiennent pour en inférer l'existence du processus par lequel ils expliquent ces résultats: la transmission de pensée. Les psychologues ne peuvent faire autrement que de répondre sur le même terrain puisque c'est le leur, celui sur lequel ils justifient leurs propres hypothèses: la plausibilité, tant de l'existence que des caractéristiques d'un processus supposé, est fonction de l'écart entre la fréquence observée des phénomènes qu'on lui assigne comme conséquences, et la probabilité d'observer ces mêmes phénomènes dans une

situation aléatoire. En d'autres termes, la construction théorique de l'objet soumis à l'expérience étant indépendante des moyens de la preuve, la parapsychologie peut très légitimement soumettre à ces moyens un nouvel objet, celui à propos duquel elle cherche à faire partager ses croyances. Outre ses conséquences qui sont examinées plus loin, ce clivage rend compte du fait que l'argumentation de la psychologie se scinde en deux domaines tendantiellement contradictoires. Le premier met en cause la rectitude de l'usage des probabilités (moyen de la preuve). Le second tente d'expliquer les résultats par des processus différents de la transmission de pensée (objet de la preuve).

a/ En ce qui concerne l'évaluation des probabilités, plusieurs catégories d'objections ont été opposées à l'interprétation que propose Rhine des résultats de ses expériences. En premier lieu, l'inférence serait non fondée du fait que les probabilités choisies comme base de comparaison l'ont été en fonction d'un raisonnement erroné sur la nature même des probabilités (Kellog, 1936-37; Herr, 1938, Willoughby 1935-36; Becknell, 1938 & 1940). En second lieu, l'inférence serait non fondée parce que le mode de recueil des données n'autoriserait pas, ou rendrait ambigües, les comparaisons effectuées (Kellog, 1936-37; Willoughby, 1935-36; Wolfe, 1936-37). Mais l'argument le plus interessant pour notre propos est celui avancé par E.A. Becknell dans un article visant à montrer que le sort de la question dépend de la "façon de voir" (3) de chacun. Pour un évènement donné parmi d'autres possibles, la probabilité objective (possibility) se modifie en fonction des tirages effectivement réalisés. Quant à la probabilité subjective (probability) elle évolue en fonction de la connaissance que les sujets prennent des résultats. Mais un même savoir sur les tirages passés peut amener deux sujects différents à faire des hypothèses différentes sur les tirages à venir: "The person does not behave like a machine. Wherever we have a human individual we have also, presumably, an ideology. Hence, in a set-up such as Rhine's, probability becomes incomputable, since the subject may be responding to a series which exists only in his imagination, as well as to the actual empirical series". (Becknell, 1940, p. 607).

Discuter les incidences de cette remarque ne relève pas de notre propos. Mais il est remarquable que l'auteur ne fasse, en l'occurence, que mettre le doigt sur une question dont la pertinence

s'étend sans conteste à l'ensemble de la psychologie expérimentale. L'expérimenatateur suppose nécessairement qu'il connait la façon dont les sujets prennent connaissance de la situation dans laquelle il les place: soit qu'il "sait ce qu'ils pensent". La validité de ses généralisations est à ce prix. Que ce problème soit évoqué à propos de la transmission de pensée ne doit donc pas étonner: la transmission de pensée résoud, bien sûr à sa façon, une difficulté théorique et pratique que la psychologie se refuse le plus souvent à considérer, mais qui n'en continue pas moins à lui faire problème. (4)

b/ Le second volet argumentatif consiste à accepter les résultats au moins par hypothèse, afin de montrer qu'ils s'expliquent par d'autres raisons que la transmission de pensée. Cette démarche met en oeuvre implicitement une échelle de plausibilité ou de crédibilité relevant de l'évidence ou du "bon sens", et sur laquelle la télépathie occupe à priori une position très suspecte: "That such a mechanism is a priori extremely improbable need hardly be said". (Willoughby 1935-36, p. 115). Le rapport existant entre probabilité et ontologie qui a été souligné précédemment est donc repris dans ce second domaine d'argumentation, mais inversé. L'improbabilité de la transmission de pensée doit inciter à chercher une explication des résultats par des alternatives plus probables, "c'est-à-dire" plus vraisemblables. En effet, parmi les pouvoirs et propriétés que l'on prête aux individus, certains sont d'existence plus crédible que d'autres: "We do, it is true, commonly refer to individuals as gifted in intellect, mechanical skill, musicianship, athletic prowess, etc...; but these are common human traits, varying widely in amount of course, but characteristic in some degree of humanity at large. In speaking of degrees of any power, we take its existence for granted; and surely we are in no position to assume any such power as telepathy of clairvoyance until a considerable body of evidence has been gathered..." (Kellog, 1936-37, p. 193). A partir de la certitude assurée par ce raisonnement, les auteurs se sont efforcés d'interpréter les résultats par des moyens plus économiques que la transmission de pensée, et qui se regroupent tous sous les rubriques des variables secondaires ou des artéfacts expérimentaux: existence de patterns de réponse dominants, effets de la situation expérimentale (Goodfellow, 1938), inductions déterminées par le vocabulaire utilisé dans la consigne, effets de suggestion (Fernberger, 1938, Goodfellow, 1938, entre autres) (5).

Empirisme et superstition

Ainsi, en répondant au défi que lui lance la parapsychologie, la psychologie exhibe les ressorts, et par là les impasses, de ses propres critères de vérité. Le recul historique ne met pas seulement en relief ce qui se modifie, mais aussi ce qui demeure. Il est à remarquer que les deux volets argumentatifs qui viennent d'être dégagés, sont ceux que répète depuis bientôt cinquante ans la psychologie, à l'occasion des retours périodiques de la télépathie aux avant-postes de l'actualité; cette répétition témoigne bien sûr de ce que le désir de convaincre des uns et des autres demeure insatisfait. Peut-être aussi d'un lien, pour ne pas dire d'une connivence, qui s'établit au couvert de ce désir de convaincre -ou au contraire de dessiller- les crédules, c'est-à-dire dans tous les cas les autres, évidemment.

a/ Il n'est qu'à lire les articles précédemment cités pour comprendre que si la psychologie ne croit pas à la transmission de penséee, elle sait de quoi il s'agit. En témoigne l'absence dans ces articles de toute approche théorique des mécanismes supposés à la transmission de pensée, et de toute tentative de réfutations des arguments que les parapsychologues avancent dans ce domaine (p.ex.: Holroyd, 1936-37). Or il est clair que cette absence est un indice du caractère d'évidence que revêt pour la psychologie le terme de transmission de pensée: il n'est pas nécessaire de se mettre d'accord sur les mots, tout le monde sait de quoi il s'agit. Ainsi la psychologie est, relativement à la parapsychologie, ce que les parents sont à leurs enfants: le père Noel, ils n'y croient pas, mais il savent ce que c'est (6). Ceci n'a d'ailleurs rien d'étonnant: comme tout parent a un jour cru au père Noel, tout enfant a un jour cru que les adultes devinaient ses pensées.

Pourtant, la question n'est pas de savoir si finalement tel ou tel psychologue croit ou non à la transmission de pensée, malgré ses dénégations éventuelles (7). En effet sous cet angle, il est possible de dire que tout le monde croit à la transmission de pensée ... sans y croire! Par contre, il est interessant de se demander pourquoi, et sous quelles formes, la psychologie de l'immédiat avant guerre était, aux Etats-Unis, sensible à cette croyance. Il faut pour cela distinguer deux plans.

b/ Le premier est celui de la conjoncture idéologique propre à l'après 1929, et de ses incidences sur la psychologie. A cette

époque, la crise économique met en défaut les idéaux qui régulaient jusqu'alors la cohésion sociale. Face à l'exacerbation des contrastes économiques et à la résurgence consécutive d'antagonismes ethniques et linguistiques, le "creuset" Américain ne remplit plus momentanément sa fonction. On assiste alors dans les sciences humaines à l'éclosion et au développement des moyens de connaitre et d'analyser "l'opinion publique"-techniques de sondages et mesure des attitudes-, et à l'essor de recherches centrées sur la compréhension des articulations entre l'individu et le groupe: rumeurs, leadership. influence ... Pour résumer l'effet de sensibilisation au thème de la transmission de pensées que cette conjoncture produit en psychologie, on peut avancer ceci: les critères traditionnels d'appréciation des clivages idéologiques et politiques se révélant insuffisants, l'incertitude qui en résulte pousse au premier plan des thémes de recherche la question suivante: "Que pensent les gens?" (8).

c/ Le second plan est celui de la structure épistémologique de la psychologie. L'échange d'arguments entre psychologues et parapsychologues s'effectue sur le socle d'une croyance fondamentale partagée par les deux parties: qu'il est possible "d'établir les faits" de façon indubitable, c'est-à-dire de telle sorte qu'ils emportent à eux seuls la conviction. Cette croyance s'enracine dans la philosophie empiriste de la connaissance. Qu'il s'agisse bien d'une croyance est suffisament indiqué de ce que, tant concernant la transmission de pensées que mainte polémique en psychologie expérimentale, personne ne convainc personne sans que le débat cesse pour autant. Sa reprise périodique vient même souligner à quel point chaque "version" d'une croyance ainsi articulée est indispensable à l'autre.

On remarquera d'ailleurs que ces deux versions cohabitaient sans trop de difficultés dans les travaux d'un W. James, et que leur antagonisme date de l'établissement du behaviorisme. On peut donc penser que le terrain sur lequel psychologie et parapsychologie se rencontrent a été produit par l'opération même par laquelle Watson les séparait. Or contrairement à ce que l'on pourrait croire, la séparation n'a pas d'abord été produite relativement à des questions de méthode ou de rapport aux faits. En tant que président de la Society for Psychical Research, W. James n'était pas moins soucieux de l'établissement et de la vérification des données que les psychologues qui lui succèderont. Les travaux parapsychologiques se sont trouvées écartés du courant principal de la psychologie en fonction

de l'a-priori watsonnien à l'encontre des mystifications philosophiques et religieuses, et ce en même temps qu'elles, en particulier concerant l'âme et la conscience. Le behaviorisme exclut le recours à des notions teintées selon lui d'obscurantisme, en délimitant ce cabinet de Barbe-bleue qu'est la boite noire. Ce refus, cet a-théorisme, laissait ouverte la possibilité pour toute tentative de théorisation, quel que soit son statut, de venir occuper la place ainsi laissée vacante. L'E.S.P. profite incontestablement de cette situation; et elle est favorisée dans sa tentative de réinvestissement de la psychologie par l'usage alors récent des probabilités: étant donné le vide théorique délimité par le behaviorisme, la dissociation de l'objet et des moyens de la preuve dans la procédure statistique d'exhibition des résultats fonctionne comme la matrice d'inscription et de validation possibles de la transmission de pensées. Il suffit pour s'en convaincre de parcourir l'exposé des mécanismes physico-physiologiques par lesquels Holroyd (1936-37) tente d'expliquer le fonctionnement supposé de l'E.S.P. Les références mises en jeu ne différent en rien, quant à leur niveau et au nombre d'hypothèses qu'elles impliquent, des modèles qu'à la même époque lespsychologues s'efforcent d'élaborer pour rétablir une continuité entre les deux extrèmes du schéma stimulus-réponse.

En établissant les "faits" de la situation comportementale en observable unique, Watson se fiait pour leur repérage au sens commun de la langue vernaculaire supposée couvrir de façon exhaustive le champ de la réalité. La mise en échec de cette position, avant même la fin du Titchenérisme (9), va déboucher sur la tentative opérationniste: chasser de la langue psychologique tout ce qui, ne pouvant être défini opérationnellement, est porteur d'ambiguités et donc de désaccords entre les psychologues. Mais dans cette démarche, l'opérationnisme se heurte en retour au problème posé par la détermination des "faits de base", des constituants derniers de l'observation, ceux à partir desquels pourrait disparaitre l'ambiguité fondamentale des langues naturelles, et donc se réduire l'incertitude, et s'établir le consensus entre les observateurs.

On trouve ainsi à la même époque, au coeur des préoccupations épistémologiques de la psychologie, la question dont la structure même justifie les prétentions des travaux parapsychologiques: comment l'accord sur les données de l'expérience peut-il être obtenu entre deux observateurs si, tant l'appréhension sensorielle que la langue

permettant d'en rendre compte, s'avèrent insuffisantes à cette tâche? A cette question la psychologie répond par la production d'une fiction tératologique qui n'a rien à envier aux processus électromagnétiques fictifs que les tenants de la parapsychologie attribuent à l'activité cervicale:

> You can not compare Peter's sensations with Paul's. You can compare Peter's with Peter's and know that for him blue is not red and conversely, and the same for Paul. But that Peter's blue is not Paul's red must remain unknowable until the operation of cross-connecting the nervous systems of two persons is possible. Only then could an integrated Peterpaul compare his Peter vision with his Paul vision ... (Boring, 1957, p. 654) (10).

Les leçons à tirer d'une polémique

Il serait hatif de conclure de ce raisonnement qu'empirisme et croyance à la transmission de pensées sont une seule et même chose, Une telle interpétation n'est pas possible puisque, justement, la psychologie passe son temps à contourner et à combattre cette croyance. Mais celle-ci est à coup sûr la contrepartie de l'épistémologie empiriste qui domine la psychologie. C'est au lieu théorique où la psychologie se refuse à aborder une question qui n'est pas formulable en des termes déja scientifiquement reconnus, que la croyance à la transmission de pensées trouve à s'incrire. Si la polémique que mène à son encontre la psychologie met en avant la question du status de la preuve statistique et des modalités de son usage, c'est faute pour elle de pouvoir exprimer crument son incrédulité dans les colonnes des revues d'obédience scientifique: elle ne peut ni ne veut croire à la perception extra sensorielle. Mais "ne pas croire à" est une forme de croyance; et toute la démarche de la psychologie indique bien qu'il s'agit pour elle de "se défendre" de la croyance qu'elle combat, dans la mesure où cette croyance s'inscrit tendantiellement comme une solution aux contradictions épistémologiques qui résultent du point d'informulable, voire d'impensable, où le mène son empirisme. Et elle doit d'autant plus s'en défendre, qu'à cette période la conjoncture idéologique porte au premier plan de ses préoccupations et de ses thèmes de recherche, des questions (comme l'influence p.ex.) qui impliquent de traiter en tant que tels des phénomènes de croyance.

A vouloir faire l'économie d'une étude effective de l'échange langagier et de la complexité de ses incidences subjectives, et ceci pour ne pas sortir d'un cadre conceptuel et méthodologique posé a priori comme scientifique, non seulement la psychologie se prive des

moyens d'une critique effective de la parapsychologie, mais elle en nourrit en son sein même la tentation. C'est ce qui permet de comprendre que la polémique que nous venons d'analyser puisse se répéter à chaque retour en vogue du thème de la transmission de pensées dans le public, et ce dans des termes invariables. Car sous couvert de dessiller les crédules, c'est en fait à elle-même que s'adresse la psychologie, c'est à son propre usage qu'elle ressasse les raisons qui la retiennent au bord de la croyance parapsychologique. En témoigne, du reste, que c'est dans ses propres revues que la psychologie a pour l'essentiel fait connaitre ses réflexions sur la question.

Dans ces conditions, le "public" n'est jamais que l'interlocuteur que "se" donne la discipline, le sujet autre qu'elle même qu'elle assigne à la croyance dont elle se défend. Cette projection sur l'autre imaginaire, qui est un mécanisme constitutif de toute croyance, a pour effet de voiler que ce que la psychologie oppose à la crédulité, à savoir la Science, ne fonctionne pas pour elle seulement ou d'abord comme ratio, mais justement comme croyance. C'est ce qui explique qu'elle préfère les chemins balisés, pour ne pas dire rituels, de "la" méthode (chemins supposés à l'avance pouvoir ne mener qu'à la vérité), à l'ensemble du réel qui excède cette méthode. Ce réel laissé pour compte s'offre alors comme le support priviligié des superstitutions collectives.

Notes.

(1) J.B. Rhine, Extra sensory perception (1935) & New frontiers of the mind (1937).

(2) "Through the media of extensive radio advertisements and high pressure book review, the systematic program of parapsychological propaganda is apparently directed ..." (Heinlein & Heinlein, 1938, p. 137).
"The fact that professor Rhine's latest book on extra-sensory perception, "New frontiers of the mind", was widely heralded at the selection of the Book of the Month Club, indicates clearly enough the popular enthusiasm". (Becknell, 1938, p. 373).

(3) En Anglais "ideology", mais la traduction par le Français "idéologie" serait un contre-sens.

(4) Sur cette question, voir: Dachet, 1975.

(5) On trouvera une étude détaillée de la controverse sur l'usage des statistiques par la parapsychologie durant cette période dans Mauskopf & McVaugh (1979).

(6) Sur cette question: Mannoni, 1969, p. 9-33.

(7) Ainsi S.W. Fernberger, qui éprouve la nécessité dans une contre-expérience de "contrôler" la transmission de pensée: "... the experimenter carefully refrained from thinking of either black

or white. To insure this during the five second interval between the two signals, he set himself such tasks as naming the Presidents of the United States in order". (Fernberger, 1938, p. 605).
Pour entrer un instant dans la logique de ce raisonnement, que faut-il penser du fait que cette liste ne comporte à coup sûr aucun noir ...?!

(8) A titre d'indice, on remarquera que l'étude de la Radio Zenith Corporation est contemporaine de l'usage systématique de la radiophonie comme moyen de propagande, usage inauguré par le président T. Rossevelt au cours de ses célèbres "causeries au coin du feu".

(9) Sur cette question: Boring, 1957, p. 645.

(10) Souligné par nous.

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