

**STUDIES IN THE HISTORY OF
PSYCHOLOGY AND THE SOCIAL SCIENCES 2**

Proceedings of the Second Meeting of

CHEIRON

European Society for the History of the Behavioral and Social Sciences

held at Heidelberg, september 2-4, 1983

edited by

Sacha Bem
University of Leiden

Hans Rappard
Free University of Amsterdam

Willem van Hoorn
University of Amsterdam

Psychologisch Instituut
van de Rijksuniversiteit Leiden
Leiden 1984

CIP-GEGEVENS KONINKLIJKE BIBLIOTHEEK, DEN HAAG

Studies

Studies in the history of psychology and the social sciences. - Leiden : Psychologisch Instituut van de Rijksuniversiteit Leiden

2: Proceedings of the Second Meeting of CHEIRON, European Society for the History of the Behavioral and Social Sciences held at Heidelberg, September 2-4, 1983 / ed. by Sacha Bem, Hans Rappard, Willem van Hoorn

Met lit. opg.

ISBN 90-70901-02-1

SISO 412 UDC [159.9+33](091)

Trefw.: psychologie ; geschiedenis / sociale wetenschappen ; geschiedenis.

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Printed by Reprodienst, Subfaculteit Psychologie, Rijksuniversiteit Leiden.

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Preface

It is with pleasure that the editors present the Proceedings of the Second meeting of CHEIRON, European Society for the History of the Behavioral and Social Sciences, which took place at the University of Heidelberg (FRG), September 2-4, 1983.

Originally 34 papers were scheduled. Some of these were not presented because the authors were not able to come to Heidelberg. In some cases papers were considered not yet ready for publication by their authors (see enclosed program). In one case two related papers were combined into one, and there was one paper that had been submitted but could not be scheduled. Therefore, 30 papers, are offered here, in most cases entirely reworked.

The First Proceedings were greeted with enthusiasm, which gave the editors the strength to continue and to bring out this volume. We hope that this series will be continued for years.

We want to express our thanks to Yvonne Weber and the printing-office of the *Psychologisch Instituut* of Leiden University.

Sacha Bem,
Hans Rappard,
Willem van Hoorn.

Leiden, November 1984.

PROGRAM

(with the original titles of the papers)

Eröffnungsvortrag C.F. Graumann (Heidelberg)

First Session: Paradigmen der Psychologie

Vorsitz: Jaques Vonèche (Genève)

- van den Braembussche, A.: Paradigms, normal science and the growth of historical knowledge.
- Hoppe, S. & Bringmann, W. (University of South Alabama, Mobile, USA): Wilhelm Wundt's principles and modern psychological psychology: A comparison.

Second Session: Geschichte einzelner Disziplinen

Vorsitz: Mitchell Ash (Westberlin)

- Ray, L. (Lancaster): Moral metaphors of the body in the formation of psychiatry
- Desmarez, P. (Bruxelles): L'utilisation du concept de système et la diversification du champ des sciences sociales du travail (États-Unis, 1939-1946)

Third session: Wissenschaftliche Entwicklungen in einzelnen Nationen

Vorsitz: Siegfried Jaeger (Westberlin)

- Erös, F. (Budapest): Historical issues in contemporary Hungarian Psychology
- Henry, P. (Paris): Spencer en France et ailleurs
- van Strien, P. (Groningen): Four stages in the relationship of Dutch psychology to its audience

Fourth Session: Begriffsgeschichte

Vorsitz: Christfried Tögel (Sofia)

- Lubek, J. (University of Guelph, Canada): Three conceptualizations of human social violence (1900-1980): A discussion of their socio-historical specificity and transhistorical comparability
- Plon, M. (Paris): Repères théoriques pour une approche de la question du grand homme
- Métraux, A. (Heidelberg) & Veličkovskij, B.M. (Moskva): Über die Maschinen-Metapher in der Psychologiegeschichte

Abendsveranstaltung: *Warum Geschichte? / Why History?*

Vorsitz: Erika Apfelbaum (Paris)

Fifth Session: Geschichte einzelner Disziplinen

(Gruppe A)

vorsitz: Larry Ray (Lancaster)

- Lovie, A. (Liverpool): Aspects of neogenesis: Spearman's system of cognition and applied psychology
- Danziger, K. (Toronto): Statistical method and the historical development of research practice in American psychology
- Jaeger, S. (Westberlin): Volkspsychologie, Statistik und Sozialreform - ein vergessenes Kapitel aus der Geschichte der Psychologie

Psychoanalyse und weibliche Sexualität (Gruppe B)

Vorsitz: Michel Plon (Paris)

- van Hoorn, W. (Amsterdam): Freud's theory of human sexuality
- Fischer, A. (Leiden): Femal psychoanalysts' reactions towards Freud
- Jansz, J. (Leiden): Femal sexuality and psychoanalysis in the 70's: The case of France and the USA

Sixth Session: Philosophie, Theorie und Methodologie

(Gruppe A)

Vorsitz: Hans Rappard (Amsterdam)

- Ricci, C. (Roma): 'Molecularism' et 'Holism' dans la première génération des behaviouristes
- Beck, I. (Budapest): Psychological classification and new perspectives of scientific methods
- Meijering, G.T. (Nijmegen): Mechanism and the rise of an information theory of perception
- Tweney, R. (Bowling Green, Ohio, USA): Cognitive psychology and the history of science: A new look at Michael Faraday

Geschichte der Psychologie (Gruppe B)

Vorsitz: Ferenc Erös (Budapest)

- Cochart, D. (Amiens): Un aspect des romans naturalistes au XIX^e siècle - l'enquête sociale
- Legrenzi, P. (Trieste): Trends in American psychological research: 1900-1940
- van Ginneken, J. (Leiden): The debate on the origins of early crowd psychology
- Behrens, P. (Pennsylvania State University, USA): The first Ph.D. Programs in experimental psychology at the Leipzig University, Germany, and the Johns Hopkins University, USA.

Seventh Session: Begriffsgeschichte

Vorsitz: Kurt Danziger (Toronto)

- Janig, M. (Klagenfurt): Zur Konstituierung des psychologisch-wissenschaftlichen Jugendbegriffs
- Smith, R. (Lancaster): Inhibition: Notes on context in the nineteenth century
- Bobryk, K. (Warszawa): Historical changes of a cultural picture of the world and individual's self-knowledge

Eighth Session: Psychologie in der UdSSR und Kritische Psychologie

Vorsitz: Alexandre Métraux (Heidelberg)

- Rappard, H. (Amsterdam): The concept of activity and Soviet psychology as a theoretical psychology
- Tuender-de Haan, H.A. (Amsterdam): Critical psychology: A semi-participant observation
- van der Veer, R. & van IJzendoorn, M.H. (Leiden: Vygotskij's theory of higher psychological processes: Some criticisms

Ninth Session: Entwicklungen der Psychoanalyse

Vorsitz: J. Bobryk (Warszawa)

- Schorr, A. (München) Clark Hull und die Psychoanalyse
Tögel, C. (Sofia): Wundt und Freud
- Carpintero, H., Delgado, E. & Mestre, M. (Valencia):
Introduction and development of Freudism in Spain (1930-1936)

Business Meeting

Vorsitz/Chair: Hans Rappard (Amsterdam)

PARADIGMS, NORMAL SCIENCE AND THE GROWTH OF KNOWLEDGE IN
HISTORY AND THE SOCIAL SCIENCES: A CRITICAL APPROACH TO KUHNIAN
INTERPRETATIONS

A.A. Van den Braembussche
Erasmus Universiteit
Rotterdam

Summary

This paper is mainly devoted to the question whether Kuhnian interpretations of history and the social sciences can be successfully maintained. The answer being negative, the question is finally raised which theoretical framework would represent a better alternative.

The criticism of Kuhnian interpretations proceeds along the following strategy of inquiry. The first line of argument concerns the normative implications of Kuhn's theory. Indeed, in view of the many loose and divergent interpretations of this theory, one is entitled to ask: what is the hard core of it, which sets definite limits to possible interpretations? The second line of argument concerns the empirical verification of some Kuhnian interpretations of economics and history, which are examined more in detail in order to pinpoint some crucial and acute problems of corroboration. The third line of argument considers the Kuhnian interpretations of the social sciences in general. The question is not only an empirical one, viz. whether Kuhnian interpretations of all social sciences are subject to the same fatal shortcomings, exemplified above. It is also a normative one: is Kuhnian science in principle possible and even desirable for the social sciences in general?

From the objections raised to the Kuhnian approaches, one can meaningfully ask which alternative theory of scientific growth better fits the actual progress and nature of knowledge in history and the social sciences. A thorough reflection on Laudan's theory of scientific progress, offers the main ingredients of a new and more promising approach.

1. Introduction

Kuhn's theory was greeted with much enthusiasm by many historians of the social sciences, with the result that, in addition to the traditional methodologies of the social sciences, one is confronted with a huge body of Kuhnian interpretations¹. All these interpretations concern the structural characteristics of the growth of the different social sciences. Methodology of history, however, has rarely dealt with questions about the growth of historical knowledge. As a consequence there are relatively few Kuhnian interpretations of the history of history. But they do exist². Since one finds plenty of counter-arguments in the work of Kuhn himself, it is worthwhile to examine in a critical way the Kuhnian applications to both the social and historical sciences.

It seems to me that a critical analysis of the Kuhnian approach cannot bypass, firstly, the initial problem of an adequate understanding and interpretation of Kuhn's theory itself, and secondly, the overall question whether this theory is indeed applicable to the growth of the social sciences in general.

The strategy of inquiry will be as follows. First I will ask which elements of the Kuhnian theory represent its nucleus. In other words: What makes Kuhn's theory really a theory instead of a body of mere tautological statements? What are the normative implications of the theory which do, in fact, set definite limits to the range of possible interpretations? (par. 2). Secondly, I will examine more in detail Kuhnian interpretations of economics and history in order to pinpoint some crucial and acute problems of corroboration. From this examination it ensues that Kuhnian interpretations of economics and history are subject to the same fundamental shortcomings. The case-studies having proved negative, I have gone on to consider, thirdly, the problem with respect to the social sciences in general. The question is not only an empirical one, viz. whether Kuhnian interpretations of all social sciences are subject to the same fatal objections, exemplified above. It is also a normative one: is Kuhnian science in principle possible and

even desirable for the social sciences in general? (par. 3). Given the objections raised to the Kuhnian approach to both the social sciences and history, one can meaningfully ask which alternative theory of scientific growth is more appropriate to do the job (par 4).

2. *The normative implications of Kuhn's theory: "normal science" as the basic notion*

Many commentators and followers of Kuhn's approach have overlooked the fact that the theory has not only an *empirical-descriptive* but also a *normative-prescriptive* nature as well; in other words, the theory not only describes how scientific progress actually takes place, but moreover how it *should* take place. According to Homa Katouzian, Kuhn's theory has too often been regarded as purely descriptive³. Even Kuhn himself has acknowledged that his theory comprises a descriptive part and a normative counterpart⁴. Consequently, his theory could be restated as follows: "*as a matter of fact* sciences progress through the process described by Kuhn; they *ought* to progress that way. More explicitly, normal science is what normal scientists do, and what normal scientists do is *right*"⁵. It is important to realize that such a reasoning amounts to no less than a naturalistic fallacy. Kuhn's theory doesn't teach us anything about the *causes* of scientific progress. Hence it would be equally possible to argue that scientific progress has occurred *in spite* of the kind of resistance to change which characterizes normal science.

Whilst one is prepared to accept the *normative* implications of Kuhn's theory, an application of it to the social sciences and history doesn't seem very plausible at all. But even on a purely empirical-descriptive level one can show with reasonable success that even the history of the natural sciences invalidates in many cases the process described by Kuhn⁶. These case studies demonstrate that the distinction between "mature" and "immature" sciences is in practice untenable. This has normative consequences

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Whilst one is prepared to accept the *normative* implications of Kuhn's theory, an application of it to the social sciences and history doesn't seem very plausible at all. But even on a purely empirical-descriptive level one can show with reasonable success that even the history of the natural sciences invalidates in many cases the process described by Kuhn⁶. These case studies demonstrate that the distinction between "mature" and "immature" sciences is in practice untenable. This has normative consequences

too. As a monoparadigmatic situation seems to be the exception rather than the rule in the natural sciences, as Lakatos and Laudan have argued, the main criterion for distinguishing "mature" from "immature" sciences becomes invalid. Indeed, the most important demarcation line between "mature" and "immature" sciences is, according to Kuhn, the fact that only the "mature" sciences are ruled by one and the same paradigm, whereas "immature" sciences, which are still trying to escape their prehistoric stage, precisely show a great number of competing "paradigms" or "schools". Personally I am convinced that this demarcation line is only a partial one. As soon as one rejects the fact that the monoparadigmatic situation is a prerequisite for true science, then the Kuhnian theory starts to crumble. Because *without* a monoparadigmatic situation there is no normal science. Normal science - a basic notion of Kuhn's theory - only appears because of the exclusive rule of one and the same paradigm. *Without* a monoparadigmatic situation one can also forget "scientific revolutions" and "paradigm shifts". Indeed, a "scientific revolution" (in the Kuhnian sense) only occurs when two important conditions are fulfilled, first, the appearance of a crisis, caused by anomalies, i.e. unsolved empirical problems, second, the choice of *only one* alternative paradigm, which allows for a new stage of normal science. If one abandons or rejects the criterion of demarcation, one deprives the Kuhnian theory of all its strength and selectivity. It is precisely this criterion which makes Kuhn's theory a theory instead of a sheer tautology!

To sum up: it is the notion of "normal science" which represents the central tenet of Kuhn's theory. All other concepts, such as "scientific revolution", "paradigm shift" (or even the notion of "incommensurability") derive their validity and significance from this basic unit. Therefore "normal science" has *normative* implications: only those sciences which are capable of being normal science can be regarded as truly "mature". Here one has to be careful in applying this framework to the social sciences and history.

3. *On paradigms and revolutions in history and the social sciences*

In a brilliant criticism of Kuhnian applications to the history of economics, H. Katouzian first tries to explain why the theory of Kuhn had such extraordinary success among social scientists. According to him this success cannot be explained only by the novelty and fashionableness of Kuhn's theory: "(...) it was also its peculiar appeal both to conservatives and to radicals on the basis of their (widely differing) predilections. The conservatives (rightly) thought that the new theory would justify their habitual resistance to change; the radicals (wrongly) believed that it could be used to sound the bells of (scientific) revolutions"⁷. The second strategy is indeed not uncommon. The appeal to Kuhn's theory becomes questionable and uncritical as soon as social scientists or historians use the concept of "paradigm shift" to justify their position rather than to characterize the development of the science itself. In such cases Kuhn's theory solely serves as a means to legitimate one's own practice or to propagate a new concept of the discipline. There are many examples of this, both in the social sciences and in history⁸. "Paradigm shifts", when proclaimed by scientific renewers or radicals, almost unavoidably take on the function of a self-fulfilling prophecy. K.G. Faber calls it a "strategy of immunization" (*Immunisierungsstrategie*)⁹.

In his amply illustrated criticism of Kuhnian interpretations of economics Katouzian argues that Kuhnian economists do not as a rule take account of some, if not of all, of the central tenets of Kuhn's theory. First, they forget that normal science excluded the existence of competing "paradigms". Second, they are not aware of the fact that a scientific revolution is impersonal and unpredictable, that it doesn't occur because a group of dissenting scientists proclaim it. Third, they completely disregard that a crisis (which is the necessary condition for a scientific revolution) must show a persistent incompatibility between the dominant paradigm and empirical findings. Fourth, they forget that a crisis

is not in itself a *sufficient* condition for a revolution: a new paradigm must emerge which is accepted by the scientific community as being superior to all others. Fifth, they disregard the fact that the victorious paradigm must at least be incompatible, if not *incommensurable*, with its predecessor.

Whereas, for instance, in Kuhn's theory a new paradigm, which is accepted by the "scientific community" as a superior framework, must rule out all competing paradigms, Katouzian points to the impossibility to substantiate such claims with respect to the history of economics. So, even if one assumes that the "neo-classical synthesis" is the unique paradigm in economic theory, one cannot deny that it contains many divergent and competing "paradigm parts": "Consider the Keynesian and the monetarist controversy; the marginal productivity theory of wages versus the bargaining models; the two competing models of international trade theory both of which are used in teaching and research even *by the same economists*. The fact that at least five models of inflation are currently taught, and only some of which are subject to theoretical controversy; that in their research activities some development economists regard the real value of labour as determined by its shadow price in the rural sector, and some others as defined by its market price in the urban sector (many adopt a 'hybrid' position); that in the theory of macro-economic dynamics capital is sometimes regarded as finance, sometimes as machines and perhaps more often as 'putty clay' ... So much for the Kuhnian credentials of economic theory as a 'mature science'".¹⁰ Equally, one can reasonably expect to meet with some of the main difficulties outlined by Katouzian, when one makes a critical approach of the Kuhnian "applications" to the growth of historical knowledge.

Let us briefly consider one of these difficulties. According to Stoianovich, historical science passed through three stages, respectively the exemplary, evolutionary and functional-structural paradigm¹¹. The "*exemplary paradigm*" (antiquity to the 19th and 20th centuries), which was characterized by the attempt to select the relevant example (*paradigma*, *exemplum*) in the didactic sense of

being illustrative of what the society desires to inculcate and what it wants to warn against, is by no means a good example of a Kuhnian paradigm. First, the didactic function and social relevance of exemplar history doesn't make it incompatible (let alone incommensurable) with the other "paradigms" mentioned by Stoianovich. Second, within the exemplary paradigm there is no consensus at all as to which methods should be used in history. Stoianovich speaks about "three forms of expression" through which evolved exemplar history: from history as narration or chronicle (Herodotus), to history as systematic explanation (Thucydides) and then to "the methodical application of the systematic human studies to the explanation of the historical context" (Polybius). The three different forms of expression suggest important disagreements about the nature of historical knowledge: they could easily be regarded as embryonic forms of different "paradigms". Indeed, could one think of a greater distinction than that between "narration" and "the methodical application of systematic human studies"? Third, it seems a bit ill-considered to claim the existence of a paradigm in history long before this was the case in the so-called advanced sciences. At this point the inapplicability of Kuhn's theory becomes very manifest indeed. In view of the fact that the "exemplary paradigm" covers even the prehistory of a scientific discipline, one may not use the concept of "normal science" at all.

As regards the evolutionary and functional-structural paradigm too, one is confronted with huge and unsolvable problems. In the first place the *lack of consensus* within the proposed paradigms is again remarkably evident. As a result of this, neither the evolutionary nor the functional-structural paradigm show the characteristics of "normal science". There is even a high degree of discontinuity between the "universalistic" and "particularistic" orientations within the evolutionary paradigm. The particularistic orientation, which can readily be identified with German *Historism*, indeed constituted a radical reaction to the broad and rationalistic conceptions which were so typical for the Age of the Enlightenment. In the eyes of Leopold von Ranke and others, rationalistic

historiography, which culminated in Hegel's philosophy of history, represented a purely speculative and unscientific enterprise. Therefore great stress was laid on the development of severe source criticism and the faithful and concrete description of the past. This went together with the appearance of the professional historian and the final institutionalization of historical science within the universities. The radical breakthrough of "scientific history" in the course of the nineteenth century, with its conscious rejection of universalistic, rationalistic historiography in favour of a patient and critical delving into the particular and irreducible facts of the past, prevents us from regarding both kinds of history-universalistic and particularistic-, as exemplifying one and the same paradigm. The "functional-structural paradigm" too, with Stoianovich identifies with the French Annales School, shows anything but the characteristics of a "normal science". It is true that the Annales Group has promoted, at least during the years of Braudel's leadership (ca. 1950-1970), the study of long enduring structures¹², although the relation to the structuralist models that dominated French social science during this period, is somewhat misleading¹³. Moreover, when Stoianovich characterized the Annales as "functional-structural", then he mistakenly represents the more recent tendencies towards the construction of time series (i.e. *histoire sérielle*) as being fully integrated into the older, Braudellian unity of method and outlook. Though the development of the Annales School can indeed be traced through different stages, which show some kind of unity¹⁴, one is nevertheless compelled to admit that the "school" has always been characterized by an enormous diversity of approaches and even by more or less opposite tendencies¹⁵. This undoubtedly explains why Fernand Braudel, in his foreword to Stoianovich's book, rejects the whole idea of an "Annales-paradigm"¹⁶.

In the second place one looks in vain for the structure of scientific revolutions, as it has been described by Kuhn. Stoianovich totally fails to demonstrate that historiographical progress really corresponds to this structure. The necessary and the suf-

ficient conditions for a scientific revolution are fulfilled. The subsequent transitions from the exemplary to the evolutionary, from the evolutionary to the functional-structural paradigm are not occasioned by anomalies, but rather appear to be the result of some redefinition of the historical discipline itself. Even the "crisis of historicism" which Stoianovich, rightly I think, associates with the transition from the evolutionary to the functional-structural paradigm, has not been caused by anomalies in the Kuhnian sense. Moreover, it is really question-begging to postulate that only one paradigm has evolved from this crisis. Some historians remained largely committed to the evolutionary paradigm, whereas others have sought to renew historical research in other ways than those promoted by the members of the Annales School¹⁷.

In the third place, it seems that the different paradigms, as construed by Stoianovich, are by no means incompatible (let alone incommensurable) with one another. If this were so, how could Stoianovich assert that the functional-structural paradigm is "at least in part, the probable product of the inner dialectic of developmental history, or the confrontation between the universalistic (...) and particularistic development modes"¹⁸.

So far, we have seen that the Kuhnian "applications" to economics and history are not only founded on a serious misinterpretation of Kuhn's theory, but that once Kuhn's theory is properly understood, they are also subject to the same crucial and fatal falsifications. So much for the possibility of applying Kuhn to the history of economics and history.

An interesting point of consideration is the broader question whether the above also holds true for the social sciences in general. For the sake of clarity, one should disentangle two important aspects: the empirical aspect, whether Kuhnian applications to all of the social sciences are subject to the same empirical shortcomings, exemplified above; and the normative aspect, viz. whether Kuhnian science is *in principle possible and even desirable* for the social sciences in general?

With respect to the first aspect, our case studies undoubtedly have some *prima facie* validity. The empirical objections, specified above, seem to hold good for each of the existing Kuhnian approaches to the *different* social sciences. Therefore, one is not surprised to meet with some of the main difficulties in, for instance, human geography¹⁹ or psychology²⁰. No doubt it would take a very extensive work in order to show at length how and why Kuhnianism in the different social sciences cannot be corroborated by practical examples. The argument would, moreover, be much more complex, because different interpretations of Kuhn's theory can sometimes be very elucidating, especially on a purely heuristic level. But, in my view, it is only thanks to a *misunderstanding* of Kuhn's theory, that some interpretations seem to be verifiable.

Taking into account the normative implications of Kuhn's theory, one can raise serious doubts as to whether the theory really is *appropriate* for the social sciences, as far as epistemology is concerned. Though history cannot be simply identified with the social sciences, I think both history and the social sciences are *de facto* pluralistic. Moreover, I would say that this state of affairs is not deplorable, but an essential and even a *desirable* feature of our historical and social-scientific knowledge. Every research tradition which believes that it is telling the ultimate truth is in constant danger of degenerating into dogmatism; it easily becomes anachronistic and sterile. Research traditions in history and the social sciences which constantly revise and adjust themselves and which constantly try to integrate new approaches in their research programmes are likely to be the most fruitful ones. This is not a plea either for anarchism or for immunization: the final test of a research tradition is whether it deals in an optimum way with both the empirical and rational problems it is supposed to solve. In the last section we will discuss in greater detail such an alternative framework.

4. *The search for an alternative framework: the "problem solving"-model of Larry Laudan.*

The PS model advanced by L. Laudan in his *Progress and its Problems*²¹ seems to offer us a more adequate framework for the study of the growth of historical knowledge. The fundamental objections to the Kuhnian approach are in one way or another neutralized by this model.

One of the fundamental objections to Kuhnian approaches was that, in the social sciences and history, monoparadigmatic situations belong to the realm of wishful thinking. According to Laudan, however, scientific progress is precisely characterized by a *continuous co-existence of competing research traditions*. This is a very dynamic viewpoint which is, I think, more appropriate to the growth of historical knowledge. Even *within* research traditions Laudan allows for a never ending, internal development. In this respect, he speaks of a natural evolution in the research tradition. The emphasis is on the *relative continuity between successive stages* in the evolution of a research tradition. The elements of a research tradition change and continually change through time.

A second fundamental objection was that the growth of historical knowledge did not fulfil the necessary condition for a scientific revolution, i.e. the evolving of anomalies. In Laudan's theory one finds a greater number of both empirical and conceptual problems which can bring about the scientific change. Internal conceptual problems, such as conceptual ambiguity or circularity of theories, and external problems, such as reinforcement and inconsistency between theories, are very important agents of progress. Laudan's treatment of the causes of conceptual problems offers a very relevant approach which can account for the numerous and diverse ways in which the social and historical sciences develop through time. Internal-scientific, normative and world view-difficulties are characterized in such a way that empirical support is decisively facilitated. This approach can even be used to elucidate the relationship between history and the social sciences.

A third fundamental objection was the non-occurrence of the sufficient condition for a scientific revolution, i.e. the victory of one paradigm. According to Laudan, scientific revolutions (in the Kuhnian sense) seldom occur, even in the natural sciences. In most cases "scientific revolutions" are the outcome of *integration of already existing research traditions*, which produces a synthesis which is progressive with respect to both of the former research traditions. Indeed, progress in the social sciences and history normally takes place, "not by the articulation of research tradition whose *ingredients* are revolutionary and new, but rather by the development of a research tradition whose novelty consists in the way in which old ingredients are *combined*"²². Clearly, such a vision implies that the idea of incommensurability of research traditions must be abandoned.

Though Laudan's PS model is to be considered as more appropriate to social-scientific and historical knowledge than Kuhn's theory, it does not reduce scientific progress to mere coincidence or complete anarchism. On the contrary, according to Laudan, research traditions are susceptible to rational procedures: one only has to compare the relative merits of existing traditions in order to determine which tradition shows a more problem-solving effectiveness. This entails a wide programme of research into the growth of historical knowledge, which will at least be more fruitful and better empirically substantiated than the Kuhnian approaches.

Notes

1. See for instance in sociology: Friedrichs (1970) and Ritzer (1975); in politics: Pocock (1971) and Wolin (1968); in economics: Coats (1969), Peabody (1971) and Bronfenbrenner (1971); in psychology: Palermo (1971), Weimer and Palermo (1973); in human geography: Chorley and Haggett (1967), Berry (1973) and Taylor (1976); in the study of literature: Jauss (1969), Göttner (1969) and Schmidt (1980). Kuhn's views are expressed most prominently in his well-known Kuhn (1970).

2. See, for instance, Stoianovich (1976) and Rüsen (1976).
3. See Katouzian (1980).
4. See Kuhn (1980, p. 237).
5. Cfr. Katouzian (1980, p. 99).
6. Even the 'Copernican Revolution' which Kuhn presented as the prototype of a scientific revolution, appears to be, according to R.J. Hall and others, an empirical anomaly. See for more detail: Bayertz (1981, p. 74-6).
7. Cfr. Katouzian (1980, p. 100).
8. E.g. Peabody (1971) and Groh (1973).
9. Faber (1980, p. 581).
10. See Katouzian (1980, p. 105-6).
11. Stoianovich (1976, p. 25-39).
12. Most empathically expressed in: Braudel (1958).
13. See for this convincing argument: Kinser (1982, p. 64).
14. For a brief outline of the different stages through which the *Annales* developed, see Van den Braembussche (1979).
15. See, for instance, Revel (1979).
16. Cfr. Stoianovich (1976, p. 11).
17. For a more pluralistic point of view, see: Iggers (1975).
18. Stoianovich (1976, p. 38).
19. Stoddart, D.R. (1981).
20. Briskman, L.B. (1972).
21. Laudan (1978).
22. Ibid., p. 104.

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HUNDRED YEARS OF PHYSIOLOGICAL PSYCHOLOGY:
SIMILARITIES AND DISCONTINUITIES¹

Susan A. Hoppe and Wolfgang G. Bringmann
University of South Alabama,
Mobile, Alabama, U.S.A.

Summary

The publication of Wilhelm Wundt's *Principles of Physiological Psychology* in 1873-1874 is generally regarded as a major event in the history of scientific psychology. Nevertheless, few psychologists are familiar with the content or have even seen a copy of this famous work. The present research describes the historical and biographical context in which this handbook was written. The content of the book is also compared with that of ten modern texts in physiological psychology, to identify changes in the field over the last hundred years. Modern physiological psychology was found to be more narrowly defined than in Wundt's times. Although important technological advances have been made in neurophysiology and neurochemistry, an outline of Wundt's text could be used as the basis for contemporary instruction in the field.

1. Background

The recent celebration of psychology's centennial has resulted in increased interest in *Wilhelm Wundt* (1832-1920) and his work (Bringmann & Tweney, 1980). In particular, his pioneering work in the field of physiological psychology and his classic textbook, *Principles of Physiological Psychology* (*Grundzüge der physiologischen Psychologie*, 1873-1874) have attracted considerable attention.

Although most psychologists learn, in the course of their studies, that Wundt's handbook, *Grundzüge der physiologischen Psychologie*, is a most significant contribution to the development of modern scientific psychology, few have ever seen a copy of it. A Russian translation of the first edition was printed in 1880 and a French translation in 1886, however no English translation of this edition exists.

Titchener's partial translation of Wundt's *Principles* is based only on the first part of the fifth edition (Wundt, 1902). In addition, original German editions of the book are scarce, fragile and not readily available for examination to most psychologists (2). Thus, most modern psychologists know little about the content of this famous work.

Much is known, however, about Wundt's interests in the newly emerging field of physiological psychology and about the circumstances under which his classic text was written and published. Wundt's interest in the field of physiological psychology can be traced to his research, "On the sense of touch with special consideration of its function in spatial perception", for which he collected the data in the Winter of 1855-56 and which was published in 1858 (Bringmann, 1975).

In the Fall of 1858, Wundt was appointed by *Hermann Helmholtz* (1821-1894) to be his teaching assistant in the Physiological Institute of Heidelberg University (Bringmann, 1975). Wundt's research in physiological psychology fully emerged during these years. Approximately half of his publication record of this period deals with topics which can still be found today in almost every textbook of physiological psychology (Bringmann & Oostveen, 1975) and much of his writing in the field was based on his own experimental researches.

According to the comprehensive list of lectures and seminars taught by Wundt during the years at Heidelberg (1857-1874), he first offered a formal course in Physiological Psychology during the winter semester of 1867-68 (E. Wundt, 1927). Since Wundt usually repeated his course offerings on a regular basis, it comes as a surprise that he did not teach Physiological Psychology again for the next five years (E. Wundt, 1927). Nevertheless, it is very likely that the initial teaching of this course in 1867 was the second step leading to the publication of the *Principles*. It had long been Wundt's acknowledged custom to acquaint himself with a new field of knowledge by teaching a course in it. Although his notes were never literally included in the eventual book, they

often served as the core of his treatment of the subject matter (Bringmann & Oostveen, 1975).

In the Fall of 1872, Wundt once again offered a course in Physiological Psychology. He had obviously done a massive amount of work on the topic, for on December 8 of that year he wrote a letter to Wilhelm Engelmann (1807-1878), an influential Leipziger publisher, informing him that he had nearly completed a lengthy manuscript, which he offered for publication under the title, *Grundzüge der physiologischen Psychologie*. He began by introducing Engelmann to the place and importance of physiological psychology as an "intermediary between physiology and psychology" and then outline his book. After addressing himself to such details as the length of the manuscript and the need for woodcuts as illustrations in the first two sections, Wundt emphasized that this manuscript was almost completed and that printing could start within two months.

The book was originally published in two softbound volumes. The first volume was issued in 1873 and largely contained material on the anatomy of the nervous system, as well as on sensation and perception. The remaining portion was published in 1874. A comparison of Wundt's original outline for the second part, as contained in his 1872 letter to Engelmann, with the actual content of the printed second volume reveals substantial changes in content and organization. While the proposed section on involuntary movement was present, Wundt's criticism of the work of previous authors and their theories, originally intended to be in a section of its own, was relegated to the appropriate individual chapters. A proposed theoretical section was compressed into exactly five pages. It may well be that Wundt's publisher asked him to increase the empirical content and cut the polemical and more specialized theoretical sections.

Both parts were apparently sold as one combined volume after the final section had been published. For this reason, much confusion exists concerning the actual data of the first edition of the *Principles*. Even Wundt's daughter reports, in her bibliography of

her father's work (1927), that the *Grundzüge* was first published as one volume in 1874.

Interestingly, Wundt never again taught a formal course in Physiological Psychology. The *Grundzüge*, however, went through six editions between 1873 and 1911 (E. Wundt, 1927). The first four editions (1873-74, 1880, 1887, 1893) have two volumes each with 900 to 1300 pages. The fifth (1902-03) and the sixth editions (1908-11) have three volumes of text and a separate volume with indexes totaling 2000 to 2300 pages.

The first part of Wundt's book was reviewed on February 21, 1874 by the prestigious German weekly, *Literarisches Centralblatt* (pp. 225-228). In the tradition of this journal, scholarly reviews were published anonymously. The critique of the *Grundzüge* was about four times as long as a typical review article. The reviewer credited Wundt with "great industry, vast knowledge and an independent outlook". The major points in Wundt's work were summarized in an objective and fair manner, and the critic clearly liked the book. A review of the second part of the *Principles* was published, apparently by the same reviewer, on November 7, 1874 (Pp. 224-225). This commentary, although much briefer, was exceedingly complimentary.

The combined edition of 1874 was noted 'in highly positive terms by the German philosopher, *Wilhelm Dilthey* (1833-1911), who reviewed Wundt's monumental work in 1876. He correctly predicted that Wundt's *Principles* would become the "basic handbook" (*Grundbuch*) for the field of physiological psychology and replace Fechner's *Psychophysik* in importance. Like the field itself, it would evolve and expand along with that important new area of knowledge. In contrast to Titchener (1910), it was Dilthey's opinion that Wundt's writing "...supported by woodcuts, was readily understandable for the broader public".

William James (1842-1910) was a young instructor of physiology at Harvard University in 1875 when he reviewed the *Principles* in a lengthy essay of seven closely printed pages for the *North American Review*. Like Dilthey, James utilized the one-volume 1874 edition

for his comments. James began his review by comparing Wundt and the other "new prism, pendulum and galvanometer philosophers" with the "heaven scaling Titans" of classical German philosophy. Skillfully, he summarized Wundt's whole career and his own reaction to it in one breathless paragraph that can hold its own with any passage in Wundt's work. It contains the full paradox of James' reaction to Wundt. On the one hand, he was awed by the accomplishments of this "singularly acute and learned author" (James, 1875, p. 201). On the other hand, however, the sheer "scale" of Wundt's efforts cooled his "ardor" to follow in Wundt's footsteps.

Thus, although much has been written about the development of Wundt's interest in physiological psychology, little has been written about the content of his *magnum opus*. One exception is a 1932 article by Feldman in which an attempt was made to summarize the content of the first edition. Unfortunately, the first section of Wundt's text was excluded and the remaining chapters were summarized only in the most cursory manner. A second article about Wundt's *Principles of Physiological Psychology* appeared in 1979 (Bringmann, 1979). The historical context in which the *Principles* was written and the reception of the book in Germany and the United States were discussed, but little information concerning its content was included.

It was the aim of the present study, therefore, to provide more complete information about the content of the first edition of Wundt's *Principles of Physiological Psychology* and to compare this classical work with contemporary textbooks in the same field in the hope of assessing some of the changes which have occurred in physiological psychology during the past century.

2. Materials

The first edition of Wilhelm Wundt's *Principles of Physiological Psychology* was originally published in two parts in 1873 and 1874 (Bringmann, 1979). A one-volume edition of the entire book which appeared in 1874 was used for the present study. Ten recent texts were first published between 1975 and 1981 were selected from

GRUNDZÜGE
DER
PHYSIOLOGISCHEN PSYCHOLOGIE.

VON
WILHELM WUNDT,
PROFESSOR AN DER UNIVERSITÄT ZU HEIDELBERG.

MIT 153 HOLZSCHNITTEN.

LEIPZIG.
VERLAG VON WILHELM ENGELMANN.
1874.

Figure 1 Title Page of Wundt's *Principles*

a collection of works in physiological psychology. All chosen texts included the words "physiological psychology" in either their titles or their subtitles.

3. Procedure

The first stage in the research involved preparation of an English translation of the *Table of Contents* of Wundt's textbook. Next, a list of fifty non-overlapping topics which are covered by most textbooks in physiological psychology was derived from the tables of contents of Wundt's work and the ten recent publications. The *Thesaurus of Psychological Index Terms* (American Psychological Association, 1982) was used as an additional source of important topics. Finally, each text was carefully examined in a page by page search and the number of pages devoted to each of the fifty topics was recorded on a separate worksheet for that text. The actual number of pages devoted to each topic was converted to per cent of pages of text to allow a comparison between texts. In addition, the references and illustrations in each text were counted and recorded. Other distinctive or unique features of the texts were also noted. The results were then organized to be displayed in four tables to permit comparison between the contents of Wundt's classic and modern texts in physiological psychology.

4. Results

The first edition of Wilhelm Wundt's *Principles of Physiological Psychology*, published in one volume in 1874 by Wilhelm Engelmann in Leipzig, Germany, was 871 pages in length, 863 pages containing textual material, and there 155 illustrations prepared from woodcuts. The text is divided into six subdivisions: Introduction (20 pages, 2.32% of text), First Section - Physiological Characteristics of the Nervous System (252 pages, 29.20% of text), Second Section - On Sensations (191 pages, 22.13% of text), Third Section - On Nature of Images (243 pages, 28.16% of text), Fourth Section - Consciousness and its Influence on Ideas (113 pages, 13.09% of text), and Fifth Section - On Movement (44 pages, 5.10% of text).

Table 1

WILHELM WUNDT'S "PRINCIPLES OF PHYSIOLOGICAL PSYCHOLOGY"

Table of Contents	Length In Pages	Per Cent
Preface	4	0.46
INTRODUCTION		
1. The tasks of physiological psychology.	7	0.81
2. Basic psychological concepts.	13	1.51
FIRST SECTION. PHYSIOLOGICAL CHARACTERISTICS OF THE NERVOUS SYSTEM.		
1. The general relations of the nervous system to the total organism.	6	0.70
2. Structural elements of the nervous system.	16	1.85
3. Morphological development of the central nervous system.	60	6.95
4. Course of nervous pathways.	70	8.11
5. Physiological function of the central nervous system.	62	7.19
6. General physiological mechanics of the nervous system.	35	4.40
SECOND SECTION. ON SENSATIONS.		
7. General characteristics of sensations.	9	1.04
8. Intensity of sensations.	33	3.82
9. Qualities of sensation.	111	12.86
10. Sense-Feelings.	38	4.41
THIRD SECTION. ON THE NATURE OF IMAGES		
11. The concept and nature of images.	6	0.70
12. Images derived from touch and movement.	26	3.01
13. Images derived from hearing.	26	3.01
14. Visual images.	121	14.02
15. Images of fantasy	22	2.56
16. Complex images, general images, and different forms of perception.	26	3.01
17. Aesthetic feelings.	16	1.85
FOURTH SECTION. CONSCIOUSNESS AND ITS INFLUENCE ON IDEAS		
18. Consciousness and attention.	19	2.20
19. Duration and association of images.	74	8.58
20. Emotions.	20	2.31
FIFTH SECTION. ON MOVEMENT.		
21. Reflexive and voluntary movement.	18	2.08
22. Expressive movements.	20	2.32
23. Final considerations.	6	0.70
Index	7	0.81

Table 2

A DESCRIPTION OF RECENT TEXTBOOKS IN PHYSIOLOGICAL PSYCHOLOGY

AUTHOR	DATE OF PUBLICATION	TOTAL PAGES	PAGES OF TEXT	ILLUSTRATIONS	REFERENCES	INDEX
1. Beatty	1975	357	307	99	295	yes
2. Brown	1976	413	310	75	780	yes
3. Brown & Wallace	1980	634	527	182	647	yes
4. Bruce	1977	416	363	179	273	no
5. Levitt	1981	594	483	195	1323	yes
6. McFarland	1981	437	359	127	861	yes
7. Plotnik & Mollenauer	1978	416	224	102	351	yes
8. Schneider & Tarshis	1975	529	422	88	577	yes
9. Thompson	1975	669	587	194	720	yes
10. Watson	1981	552	460	170	450	yes
MEAN	1977.9	501.7	404.2	141.1	627.7	---

Table 1 present a summary of Wundt's detailed Table of Contents including main chapter headings only. The length of each chapter in pages is included as well as the per cent of pages which that chapter comprises. (A complete translation of the Table of Contents of Wundt's text is available from the authors of this paper (3)).

Table 2 contains descriptive data for each textbook examined, including the author, date of publication, length, and number of illustrations and references.

The recent texts, on the average, were found to be slightly more than half as long as Wundt's text (57,6%). While the number of illustrations varied considerably (from 75 to 195), the mean number of illustrations, 141.1, was not much different from the 155 illustrations found in Wundt's text.

Summary dating concerning the content of the textbooks examined are found in Table 3. Data are given concerning the extent of coverage of each of twenty-three major topics for each of the texts examined. Comparing the content of Wundt's text with that of recent texts, it was found that the list of topics which comprise 100% of recent texts make up only 57% of Wundt's text. The content of recent texts corresponds roughly to the first three and the sixth sections of Wundt's text.

The topics which were found to be unique to Wundt's text or to the contemporary texts are listed in Table 4.

5. Discussion

Although more than one hundred years have elapsed since Wilhelm Wundt published the first edition of his classic book, *Principles of Physiological Psychology*, the text is not as out-dated as might be thought. Although advances in knowledge have occurred, the overall scope of the discipline has not changed as much and there is much overlap between the basic content and organization of Wundt's text and modern texts. Specifically, if one were to outline the first, second, and fifth sections of Wundt's text, that outline would be consistent with the content of most modern texts. In

Table 3

A TOPICAL COMPARISON OF WUNDT'S TEXT WITH RECENT TEXTS*

TOPICS	WUNDT	1	2	3	4	5	6	7	8	9	10	MEAN
Introduction	2	2	0	2	2	0	1	0	1	0	0	1
History	2	0	9	0	3	0	0	0	3	0	3	2
Methods	1	0	0	0	0	0	2	0	4	0	1	1
Neuroanatomy	10	3	8	4	11	6	5	5	6	8	8	6
Development	5	0	4	8	12	7	5	21	4	9	9	8
Neuron	6	8	7	7	8	8	11	8	7	7	4	8
Sensation	2	15	5	3	13	6	1	1	4	7	5	6
Vision	7	11	2	6	5	4	11	4	9	3	7	6
Audition	3	7	1	6	4	3	4	2	5	2	5	4
Chemical Senses	0	0	1	6	4	2	2	3	3	0	2	2
Somesthesis	0	5	3	7	4	4	3	2	5	2	8	4
Motor Systems	8	7	8	5	4	8	6	0	5	3	5	5
ANS	0	1	2	1	5	2	1	5	3	0	0	2
Motivation	0	3	5	6	2	9	10	7	8	8	8	7
Sex & Hormones	0	0	1	8	0	11	8	4	3	3	1	4
Emotion	2	2	8	5	1	4	7	6	6	7	3	5
Sleep	1	13	1	8	4	7	8	5	3	9	4	6
Reinforcement	0	1	0	2	1	5	0	0	2	2	0	1
Learning & Memory	0	9	9	9	5	10	10	6	9	15	7	9
Cortex	1	12	9	8	6	2	4	5	4	12	6	7
Brain Disorders	0	0	4	2	1	3	3	6	4	0	10	3
Consciousness	2	0	5	0	2	0	0	1	1	0	3	1
Altered States	3	0	8	0	4	0	0	8	0	3	2	3
TOTAL	57	100	100	100	100	100	100	100	100	100	100	100

* Numbers in heading refer to recent textbooks as listed in Table 2. Numbers in body of table are per cent of text pages. Computed mean is based on recent texts only, excluding Wundt.

Table 4

CONTENT DIFFERENCES BETWEEN WUNDT'S TEXT AND RECENT TEXTS*

UNIQUE TO WUNDT	UNIQUE TO RECENT TEXTS
Psychophysics (4)	Chemical Senses (2)
Sensory Feelings (4)	Somesthetic, Proprioceptive and Vestibular Systems (4)
Sensory Theories (4)	Autonomic Nervous System (2)
Aesthetic Feelings (2)	Motivation (Hunger & Thirst) (7)
Images (General) (1)	Sexual Behavior & Hormones (4)
Spatial Images (3)	Reinforcement (1)
Auditory Images (3)	Learning and Memory (9)
Visual Images (13)	Brain Disorders & Behavior (3)
Complex and Abstract Images (3)	
Relation & Association of Ideas (8)	

* Numbers in parentheses are per cent of text pages.

addition, with only relatively minor changes and additions, one could use that outline to teach a modern course in physiological psychology. Wundt would certainly be able to recognize a modern textbook or course of physiological psychology for what it is.

Differences do exist, however, between Wundt's text and modern texts. It seems that the field of physiological psychology for Wundt was much more broadly defined than it is today. Two major areas which were part of physiological psychology in Wundt's day, perception and cognition, have become separate fields. In addition, Wundt's text included much information that would normally be found in textbooks of experimental psychology. In general, almost half of Wundt's *Principles of Physiological Psychology* cover a very broad range of topics in experimental psychology that would today be covered in modern textbooks of cognition, perception, emotion, and the philosophy of science.

In contrast, physiological psychology is now a highly specialized field which depends heavily on advanced technological developments. Much of the anatomy of the brain had already been described in the 1800's and the basic functions of the neuron and the various parts of the brain were known. However, the connections between brain nuclei, as well as neurochemical and neuro-hormonal processes were unknown in Wundt's time.

It would seem appropriate that future work should include an accurate English translation of Wundt's first edition (1874). In addition, it would be of interest to compare earlier and later editions of his handbook to better understand the origins and development of this important branch of psychology.

Notes

1. This work was supported in part by University of South Alabama Research Grant # 3-61170, (May 1980 to December 1982).
2. A facsimile reprint of the complete first edition of Wundt's (1874) *Principles of Physiological Psychology* is to be published by the VEB Verlag der Wissenschaften in East Germany and the Springer Publishing Company in West Germany.
3. A complete translation of the table of contents of the complete first editions of the *Principles* with page information can be obtained from: Wolfgang G. Bringmann, Ph. D., Department of Psychology, University of South Alabama, Mobile, AL, 36688, USA.

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MORAL METAPHORS OF THE BODY IN THE
FORMATION OF PSYCHIATRY

L.J. Ray

University of Lancaster

Summary

This paper addresses the ambiguity of psychiatric language in nineteenth century Britain, which combined a search for neurological origins of mental disorder with a guilt-laden language of 'moral causes' and hence responsibility. Later in the century mental disorder was seen as a condition in which bad morals acted on the body, perhaps over several generations, to bring physical decay, deficiency, and insanity. At the same time the new asylums were filled by the pauper insane. It is argued that the increasing numbers of the pauper insane posed a threat not only to the self-image of the emerging science of psychiatry, but also to the enlightenment promise on which it was founded. In an attempt to reconcile this problem, while defending the legitimacy of the Victorian social order, psychiatrists looked to the poor in general and mentally ill in particular, as responsible for their own condition. It was their degenerate life-style that left them subject to pauperism, illness, criminality, and mental disorder. This confirmed the late Victorian world-view, through which the middle classes looked with alarm at the growth of trade unions, working class political militancy, and the threat of social disturbance in the cities. This analysis has implications, then, for the scientific status of psychiatry during this formative period, and the extent to which it was embedded in social and political conflicts, generating metaphors of disease that reflected prevailing social tensions.

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The aim of this paper is to examine the ways in which moral metaphors were entwined with the language of disease in psychiatry throughout the nineteenth century. The importance of this is that suffering was endowed with metaphysical meanings in such a way that people could be held responsible for their own suffering. (This

point has been elaborated by Doerner, 1981). This paper suggests that the curious ambiguity of psychiatric language in the nineteenth century - the fusion of medical and moral meanings - was part of an attempt to resolve a number of social ambiguities. In particular, those involved the attempt to reconcile the poverty of the urban, unskilled, migratory population with liberal expectations of progress and self-regulation through the market. They also involved, though, the ambiguity of psychiatry itself, as a social reform movement claiming the authority of medicine, but also defining its object as non-medical, as 'mind'. In this period, psychiatry attempted to define its professional boundaries, whilst maintaining a position both within and marginal to, general medicine.

The discussion proceeds as follows. First, the juxtaposition of 'reason' and 'myth' in psychiatry is considered as a programmatic statement of its enlightenment promise.¹ Secondly, moral meanings involved in the notion of the 'pauper insane' in later nineteenth century psychiatry are discussed. Finally, it is suggested that psychiatry was constituted by a dualistic conception of body and mind, or illness or morals. It addressed itself to questions of social and political organization, as well as neurological disorder.

T.S. Clouston, Resident Physician at the Edinburgh Royal Asylum, claimed that, 'It is no doubt true that the restraint and management of the reproductive instinct give most youths much trouble, and as medical men, the priests of the body and the teachers of the truths of medico-psychology and physiology, we can often help them by our counsel and our knowledge' (Clouston, 1887, p. 490). Though clearly metaphorical, Clouston's reference to 'priests of the body' perhaps illustrated a medical view of the social role of the psychiatrist in late Victorian Britain, and of the prevailing relationship between medical authority and esoteric knowledge of the body. The unintentional irony of Clouston's analogy with a medical priesthood can be seen if we compare his view with that expressed forty years earlier by the French alienist, Jean Etienne Esquirol.

In 1845 Esquirol had announced the discoveries of medical psychology as representing the triumph of reason over myth and superstition. Initially, he argued, man was 'dependent by his organisation' upon external influences, passing alternately from well-being to sorrow, pain to pleasure, fear to hope, and was led 'naturally' to reflect upon nature and good and evil. Finding consolation in the idea of a good being and a malevolent spirit presiding over his fortune, it was only one more step to a system of theology. Physical conditions were threatening. From sadness to fear and terror there was but one step. It was this 'religious melancholy', redescribed by Esquirol in medical language, that gave rise to magic and witchcraft, oracles, astrology, demonic ceremonies, that were all 'children of fear'. Now, he claimed, a better education and the progress of knowledge have gradually dispelled these fatal errors. According to this view, the mythic power of nature is dispelled by reason and science. Psychiatry promises to disenchant the mystery of the occult by exposing it as insanity. The occult is exposed as nature itself, as disease, since insanity is the incursion of nature into reason. The physician is the agent of the positive age, using reason and science to exorcise the irrational (Esquirol, 1845, pp. 235-52).

Esquirol's essentially positivist conception of psychiatry, or medico-psychology, was a view that has been echoed by many historians of medicine and psychiatry. Zilboorg, for example, depicts sixteenth and seventeenth century witchcraft beliefs as pre-scientific attempts to understand mental illness. Witches were mentally ill - they were schizophrenics and religious maniacs, displaying paranoid delusions: beliefs about their ability to influence natural and social events, and fantasies of a pact with the devil. The Inquisition too was a pre-scientific attempt to control the public threat posed by these psychotics, although here again madness entered - hence the tortures and brutal public executions, themselves the product of mentally disordered authorities (Zilboorg, 1949).

Historical orthodoxy has established a divide between science and religion, reason and myth, and hence between priestly and medical roles. However, this orthodoxy was challenged during the 1960s and 70s by writers both within and outside psychiatry who argued: (1) that psychiatry was an instrument of political and social control, rather than a neutral science of mental pathology; and (2) that there were close historical and institutional similarities between the practice of psychiatry and the earlier practices of the sixteenth and seventeenth century Church and Inquisition: the witch-hunts, heretic-burning and pogroms. For these 'anti-psychiatrists', psychiatry was after all an extension of religious mystification, intolerance and control. Amongst writers such as R.D. Laing, D. Cooper, or T.J. Scheff, the American psychiatrist and psychoanalyst, T.S. Szasz has argued in particular against an enlightenment view of the emergence of psychiatry. Szasz argues that psychiatry serves as a convenient way of avoiding confrontations with moral conflicts and social problems. These 'problems in living' are ubiquitous since life necessarily involves moral choices and moral conflicts. These might result in suffering, despair, misery and torment, but if we attempt to 'neutralize' these conflicts - to ascribe a social group (psychiatrists) with the power to intervene, rectify and resolve these conflicts, then we court the hazards of political tyranny disguised as psychiatric therapy. As the witch was the scapegoat for social problems in the declining middle ages, so the modern psychiatric patient is the scapegoat for contemporary problems in living. The declining middle ages experienced economic conflicts arising from crop failures and the agricultural trade cycle; religious conflict during the Reformation and Counter-Reformation; and conflict between an exclusively male profession of medicine and the existence of traditional female healers. Modern societies experience inter-generational conflicts, arising from cultural and value diversity, resistance to mass consumerism and the work ethic, and problems over the definition of sexual 'normality'. The earlier period attributed its problems to witchcraft, the modern age attributes its problems to mental illness (Szasz, 1973b).

Indeed, for Szasz, there was a direct transition between the persecution of witches, located in the religious world-view of the late middle-ages, and the rise of psychiatry, located in the scientific world view of the modern age. Modern psychiatric ideology is an adaptation to a scientific age of the traditional ideology of Christian theology. Instead of being born into sin, man is born into sickness. 'In short', he says, 'whereas in the Age of Faith the ideology was Christian, the technology clerical, and the expert priestly; in the Age of Madness the ideology is medical, the technology clinical, and the expert psychiatric' (Szasz, 1973a, p. 5).

It is by no means the least interesting aspect of Szasz's critique that he aims to undermine the scientific credibility of psychiatry with reference to its priestly functions. This intention is rooted in the terrain mapped out by psychiatry itself: in its constitution of irrationality on two levels. The irrational is both disease, the true meaning of madness, and myth itself, in the form of pre-scientific superstitions. To what extent then, does psychiatry represent a new priesthood - a functional equivalent to the Church, still embedded in a language of guilt and redemption? It will be suggested here, first that the formation of psychiatry in the nineteenth century did indeed incorporate ideas of guilt and moral control; and secondly that there are some sociological similarities between the role of the priest and that of the Victorian medico-psychologist.

The formation of psychiatry advanced the claim that has been adopted by its advocates during the subsequent two centuries, namely, that insanity is a 'disease like any other'. This is a dubious claim, in that from its inception, psychiatric practice has been infused with moral meanings. Indeed, the early decades of the nineteenth century saw the development of a practice that referred explicitly to a moral sphere.² Early treatments adopted the ideas of 'moral treatment' or 'moral management' advanced by the French physician, Pinel (1806). These ideas, which became current in England through the works of the Tukes, Gardiner-Hill, Burrows, Crowther, Conolly and others, claimed that the violent paroxysms of

insanity could be controlled through 'moral' techniques of manipulation. Their medical content was negligible. Nature left to itself would effect a cure. Robert Gardiner-Hill, physician at Lincoln Asylum, argued that medicine is of little avail in the treatment of insanity: 'Moral treatment with a view to induce habits of self-control is all and everything' (1839). Thomas Graham, the popular medical writer, described moral treatment as 'a union of firmness with mildness, blandness of manners, benevolence' (1845). Its aim was to encourage self-control and self-discipline through re-education. The insane, said George Mann Burrows, 'are but children of a larger growth' (1828). In particular, religion, it was believed, exercised controlling calming considerations which would counteract moral or even physical causes of derangement. Reason was like the immortal soul, it could be perverted but not obliterated. The body was the vehicle of the mind, as it was the vehicle of the soul, thus the objective of the new asylums in the early nineteenth century, such as Samuel Tuke's (Quaker) Retreat at York, was to redeem the 'power of the patient to control the disorder' (Tuke, 1813).

Further, the new asylums were to resemble families. They were to balance fear, discipline, religion, and firm kindness, in order to re-socialize the lunatic's unruly passions. Not only this, however, but advocates of the new system also advanced what Doerner calls a 'Romantic-Conservative' critique of the actual family in early nineteenth century society. They saw the actual family as casual, negligent, indulgent, and failing to inculcate respect for authority. With moral management then, the psychiatrist appears as head of a reconstituted patriarchal order; based on the family as the agent of education, developing self-control through discipline and religion. It was this idea that was institutionalized in the asylums of early Victorian England. It was essentially moral rather than medical or scientific.

Now, it might be argued that this was only the case during an early period in the formation of psychiatry, a period in which it had still to free itself from common sense and prejudice. As the

century continued, however, moral ideas did not disappear but became incorporated into new theories combining a moral and physiological etiology. Charles Bucknill and Daniel Tuke, for example, argued that the predisposing cause of insanity was physiological, but that exciting causes included: intemperance, immorality, disappointed affections, domestic troubles, grief, anxiety, wounded feelings, fright, and over-study (Bucknill and Tuke, 1858). Later theories achieved a combination of the moral and physiological through the concept of 'constitution', which subsumed both biology and moral power. Thus a 'weak constitution' might predispose one on the one hand to immorality, alcoholism, criminality, and on the other hand, to epilepsy, TB, cancer, and insanity. With this conception insanity remains a failure of will, the product of moral weakness, but is also associated with a social and biological class of degenerates. The population of the 'pauper insane' in asylums was increasing more rapidly than that of private patients. Therefore, it was argued that insanity like immorality and disease, was a condition of the 'residuum' (the urban unskilled and *lumpen proletariat*).

Following Morel's theory of degeneracy, writers such as Maudsley and Clouston believed that evolution established a continually operating law of gradual growth. Hence the immoral habits of parents (alcoholism, pauperism etc.) might be transmitted to their offspring, who would suffer from life-shortening diseases. Violation of moral law was translated into violation of natural law, and carried with it the 'wages of sin', namely death. This transposition of religious into medical rhetoric was common amongst later Victorian psychiatrists. Writing about hereditary insanity, Maudsley said, 'Because the fathers had stoned the prophets, therefore it was that the children rejected Him who sent unto them' (Maudsley, 1868), and continued that 'sins of the parents are visited upon the children'. So important was this view amongst psychiatrists that Clouston continued into the twentieth century to defend the idea of the transmission of acquired characteristics (Clouston, 1926).

Late eighteenth century psychiatrists, such as Arnold, had argued that illness was a consequence of 'unruly passions'. Hence the notion of mental disease as punishment served to sanction the moral order, and threaten good citizens. Later nineteenth century psychiatrists, such as Maudsley and Clouston, adopted a similar theme. Madness was a consequence of a degenerate life-style. Hence the urban poor, amongst whom 'pauperism', criminality, epilepsy, and other diseases were endemic, had only themselves to blame.

Why was it important for these doctors to combine moral and physiological concepts - the languages of guilt and of science? The answer might be found in the theoretical constitution of the concept of mental disorder. From around mid-century psychiatrists had been claiming that insanity was caused by lesions of the brain or nervous system. The implication of this view was that, as the German physician Griesinger put it, 'psychiatry will become a branch of neurology' (Griesinger, 1867). If mental disorders were really diseases of the brain, then the independent status of psychiatry as a profession and discipline was open to question. This was a conclusion that medico-psychologists were reluctant to accept. In 1856, the *Asylum Journal of Mental Science* (published by the Association of Asylum Medical Officers and superintendents) carried two articles that highlighted this dilemma. First it is claimed that 'Insanity is held by educated public opinion in England to be a disease of the brain, as are apoplexy or phrenitis' (*AJMS*, 1856, p. 111). However, if insanity is simply a physical disease, then why treat it in special institutions, with a staff trained in a language and practice esoteric to general medicine? Because we are told later, mental disorder is 'a condition of the brain perverting thought and feeling to the destruction or impairment of moral liberty' (*AJMS*, 1856, p. 188). Insanity had both moral and physical connotations. This dualism constituted psychiatry within and yet distinct from general medicine. Psychiatric claims to treat the mentally ill as if they were sick, hence removed from a language of guilt, were not entirely valid.

Despite the research into pathological anatomy undertaken by later nineteenth century psychiatrists, it was the mind rather than the brain that remained the seat of disorder. Clouston, Maudsley, Carpenter, Griesinger, all insisted that they were not materialists. Postmortem examinations would seek specific anatomical lesions, which they hoped would correspond with specific mental disorders. This would not, however, imply a collapse of psychiatry with neurology, since 'not every disease of the brain belongs to the class of mental disorders' (Griesinger, 1867, p. 20). The mind 'is that marvellous energy which cannot be grasped and handled' (Maudsley, 1868, p. 41). It might be affected by the condition of the brain, but is relatively autonomous, hence it is possible for medicine to combine scientific and moral judgements.

Clouston's reference to 'priests of the body', then, in part reflected his profession's retention of a language of moral judgement and guilt. It also in part reflected a gradual re-definition of the social role of the psychiatrist late in the century. The pessimistic prognosis implied in constitutional and hereditarian theories resulted in reduced emphasis on the idea of the asylum as a curative institution. The therapeutic optimism of the 1820s and 30s was replaced by attempts by the profession to extricate themselves from dealing with chronic, imbecile, deficient, or other difficult patients (see for example Granville, 1877). Instead, properly medical work was seen first in terms of family counseling. Regarding the latter, Maudsley said, 'Instead of limiting attention to the individual, we scan the organic evolution and decay of a family ...' (Maudsley, 1868, p. 246). The psychiatrist's role was to become increasingly pastoral, as they were to use their knowledge of heredity, constitution, and moral habits to advice couples whether or not to marry, whether or not to have children, and how to raise children according to the discipline of 'mental hygiene'. Clouston could claim in 1908 that people 'now are disposed not only to ask medical advice about marriage and procreation, but they actually follow it'. Fear of inherited insanity, or of incubating insanity through a too indulgent upbringing, were ideas that en-

abled doctors to adopt the role of priests, and attempt to cement the authority of the family. This was during a period in which social changes - especially the demands for women's emancipation - threatened this authority.

Doctors themselves were caught up in these social changes. As the Victorian social order was threatened by renewed working class militancy (the 'New Unionism' of the 1890s), demands for social welfare, and the inclusion of women into political society, professional groups increased their power and status. Doctors, statisticians, psychologists, sociologists, many of whose views were expressed through the Eugenics Society early in the twentieth century, offered solutions to the problem of social reintegration. In these solutions the family played an important role. It was to generate stability as parents were educated in techniques of appropriate habit-formation, family-planning, and taught how to recognise the onset of mental defects in children (see for example Clouston, 1929). These served, of course, to reduce the marginality of medical and para-medical professional occupations.

How then, does this leave our assessment of psychiatry during this period? Esquirol promised a psychiatry that would carry through the enlightenment programme of emancipation from myth through reason. In practice, the distinction between the esoteric, ritual, guilt-laden language of religion, and the rational, scientific, guilt-free language of medicine has not been clearly or consistently maintained. The priests of the body established their authority on the basis of techniques of patient control (moral management), combined with the myth that disease was a failure of moral will. Their solutions to the growth of social instability rested upon a reintegration of authority through the family, with guidance from the doctors. In this sense, psychiatry was constituted by a metaphor of illness embedded in themes of guilt, responsibility and redemption, rather than purely empirical concepts.³

Notes

1. This discussion of myth and metaphor in psychiatric language has been prompted in part by Susan Sontag's *Illness as Metaphor* (London, Allen Lane, 1979). She draws an important distinction, discussing TB, cancer, and other Victorian diseases, between the 'metaphor' of illness, and the empirical reality of the disease process. Such a materialist position avoids the shifting sands of relativism and idealism by distinguishing the socially constructed meanings of illness (the metaphor) from a scientific commitment to research and discovery, which the former might constrict. A similar view appears to be taken by Doerner (1981, pp. 85-94).
2. 'Moral' in Nineteenth Century French and English had two connotations: first, it meant ethical or morally evaluating; secondly, it meant mental, intellectual, psychological, or simply non-physical. Victorian 'psychological medicine' often contrasted its 'moral' management with the 'terrific' or physically restraining system of the pre-reformed asylums. Patients were to be managed through appeals to their 'higher sentiments', as opposed to physical coercion. However, once madness was viewed as a disease with 'moral' (non-physical) causes, then morally evaluative explanations, such as pauperism, indolence, or sexual excesses, were permitted within the discourse.
3. The question of whether modern psychiatry's concept of illness is still embedded in guilt is pertinent - but beyond this discussion. Major epistemological problems remain though, on at least two levels. First, the demarcations between 'psychiatric illness', 'personality disorder', and moral evaluation as accounts of a person's behaviour remain blurred, and psychiatrists like lay-people often slip between them. Whilst 'experts' themselves are unclear and inconsistent over differen-

tial diagnosis, there remains wide scope for using psychiatric intervention (the authority of the expert) to resolve social and political conflicts. Secondly, psychiatry (in common with other disciplines and many lay-people) clings to a concept of stress-induced illness and psychosomatic etiology. This is conceptually loose, and threatens to lead diagnosis away from consideration of physical disease to look too readily to psychological and 'blame' models of explanation.

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L'UTILISATION DU CONCEPT DE SYSTÈME ET LA
DIVERSIFICATION DU CHAMP DES SCIENCES
SOCIALES DU TRAVAIL (ETATS-UNIS, 1939-1946).

Pierre Desmarez

Aspirant au Fonds National de
la Recherche Scientifique.

Université Libre de Bruxelles.

Summary

The use of the "system" concept and the diversification of industrial research (United States, 1939-1946) - This study is concerned with the reasons which led Warner, Chapple and their groups to leave harvard University at the end of the 1930s or at the beginning of the 1940s. By comparing the three approaches of these groups, we show that their leaving can only be understood if one refers to their theoretical ambitions and their career strategies simultaneously. The growing interest of business in the social sciences and the status of Henderson's model at Harvard are among the causes of disagreement between the teams under study. Their definitions of the concepts of "system" and "equilibrium", as well as the place they give to the individual within the system, cannot be separated from the practical use of their models. The epistemological aspects of their different approaches only make sense if these are connected with their desire to design systems, the behavior of which is predictable.

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A la fin des années 1930 et au début des années 1940, certains chercheurs proches des auteurs des célèbres expériences de la Western Electric à Hawthorne quittent l'université de Harvard pour poursuivre ailleurs leurs recherches sur l'industrie. C'est sur cet épisode du processus de structuration du champ des sciences sociales du travail que porte cette communication.

Avant 1940, les études sur l'industrie menées à Harvard étaient réalisées par deux équipes, qui travaillaient en collaboration. Le

premier groupe, que dirigeait Elton Mayo, était attaché à la *Graduate School of Business Administration* et comprenait notamment Fritz J. Roethlisberger, Benjamin M. Selekman, T. North Whitehead. La seconde équipe, qui dépendait du département d'anthropologie, était notamment formée par W. Lloyd Warner, Allison Davis, William F. Whyte, Burleigh Gardner, Eliot D. Chapple et Conrad M. Arensberg. Warner quitte Harvard en 1935, pour aller s'installer à l'université de Chicago, où Davis, Gardner, et puis Whyte, le rejoignent au début des années 1940. En 1943, ils y créent le *Committee for the Study of Human Relations in Industry*.¹ Chapple et Arensberg quittent également le département d'anthropologie, mais ne suivent pas Warner. Chapple s'installe comme consultant en "organisation" à Boston et Arensberg, après un bref passage au M.I.T., devient président du département d'anthropologie et de sociologie du *Brooklyn College* (Université de Columbia). En 1941, ces deux anthropologues fondent, avec F.L.W. Richardson Jr., la *Society for Applied Anthropology Inc.* et la dotent d'une revue, *Applied Anthropology*.²

Commentant cet épisode, Michael Rose estime que des désaccords d'ordre personnel et méthodologique furent à l'origine du départ de ce qu'il appelle la "Chicago School"³. Se référant à la position de Chapple et Arensberg, il soutient que, sur le plan méthodologique, le désintérêt manifesté par les tenants de l'approche clinique d'E. Mayo pour les relations sociales et pour les processus sociaux "dérangeait" les anthropologues.⁴ Or, si c'est effectivement un individualisme méthodologique caractérise les recherches du groupe de Mayo, il nous paraît hâtif d'en déduire un désintérêt pour les relations et processus sociaux. Les textes publiés à l'époque par cette équipe, et en particulier le *Management and the Worker* traduisent en effet, comme nous le verrons, des préoccupations essentiellement sociologiques.⁵

Les deux groupes qui quittent l'université de Harvard développent, au sein des institutions qu'ils animent, les conceptions de l'organisation industrielle qu'ils n'avaient encore jamais eu l'occasion d'exprimer. En ce sens, la diversification du champ des

sciences sociales du travail répond également à des divergences théoriques. Nous tenterons de montrer ici, par une étude comparée de ces modèles, que les trois groupes, qui sont indubitablement ceux qui ont monopolisé la scène de la préhistoire de la sociologie industrielle, s'inscrivent dans des traditions théoriques distinctes, mais entendent tous se placer dans une position optimale pour accéder aux crédits croissants consacrés par les entreprises à la recherche sociale. Leurs stratégies institutionnelles sont à rapprocher à la fois de leur volonté de faire reconnaître la validité des modèles qu'ils utilisent et de leur désir de les mettre au service des entreprises. Nous verrons ainsi comment, sans que ses fondements soient remis en question, l'approche d'un des groupes considérés, le *Committee*, va, sous la pression des exigences de la gestion, se redéfinir, pour converger, du point de vue de l'intervention, avec celle de l'équipe de Mayo, dont elle entendait précisément auparavant se démarquer.

Un aperçu des traditions théoriques.

La démarche anthropologique de Warner s'inspire de celle de celui qui fut son maître: A.R. Radcliffe-Brown. Warner, comme Radcliffe-Brown, entend poser les bases d'une "sociologie comparative" qui serait à même de comprendre toutes les sociétés, industrielles ou pas, à l'aide d'une même grille d'analyse générale. Leurs objets sont identiques: il s'agit, chez Warner, comme chez l'anthropologue anglais, d'entités territoriales et culturelles bien définies et considérées comme des totalités. Pour ces chercheurs, les différentes institutions d'une société déterminée sont liées par des relations fonctionnelles dont le sens ne peut être appréhendé que par rapport à la totalité sociale. Dans cette perspective, l'analyse d'une institution sociale isolée, comme l'entreprise industrielle, n'a pas de sens, puisque seules les caractéristiques de la totalité territoriale et culturelle permettent de comprendre l'agencement de chacune de ses parties.

Les membres du *Committee* partagent également la conception de la démarche scientifique de Radcliffe-Brown; pour eux comme pour lui,

la définition d'un cadre théorique est le point de départ de toute analyse sociale.⁶ Les autres anthropologues que sont Chapple et Arensberg, par contre, prétendent pouvoir se passer de grille de lecture de la réalité; leur méthodologie et leur philosophie de la science relèvent de l'opérationnalisme tel que Bridgman le définit en physique.⁷ Pour les opérationnalistes, les aspects subjectifs des phénomènes sociaux ne peuvent être étudiés que sur base de leur manifestations explicites. L'étude de la vie de groupe est possible car la séquence des interactions entre les participants -et des actes qui les composent- ainsi que leur durée et leur fréquence peuvent être comptabilisées. En procédant ainsi, selon Chapple et Arensberg, l'analyste se met à l'abri de tout présupposé sur la nature de la vie sociale.⁸ Ils estiment que leur discipline ne peut devenir "vraiment scientifique" que si elle se limite à l'analyse des questions susceptibles d'être opérationnalisées et quantifiées. Ainsi, pour Chapple, "anthropology is a natural science, which gives every promise of becoming an exact one."⁹

Enfin, les membres de la Business School qui restent à Harvard continuent à employer le modèle théorique défini par L.J. Henderson à partir de la sociologie de Pareto, qui leur a servi lors de l'interprétation des résultats des expériences de Hawthorne. Henderson, en transmettant les théories parétiennes, les a transformées, faisant du concept de système social un concept utilisable par les différents spécialistes des sciences sociales et de l'administration. Le système social peut être analysé comme une entité relativement isolée et relativement autonome par rapport à l'environnement dans lequel elle est plongée. Selon Henderson, les éléments du système social sont des unités concrètes (les individus) et les limites des systèmes sociaux sont susceptibles d'être cernées avec précision.¹⁰

Cette approche domine les sciences sociales à Harvard depuis le début des années 1930.¹¹ De ce fait, il est illusoire, pour les tenants des autres approches, d'espérer pouvoir développer à Harvard des théories scientifiques différentes de la théorie dominante.¹² Pour satisfaire leurs ambitions scientifiques tout ayant

accès aux des fonds croissants consacrés par les entreprises industrielles à la recherche sociale, Chapple, Warner, et leurs collaborateurs doivent donc quitter Harvard.

Les ambitions et les stratégies de séduction.

Promouvoir la coopération et l'harmonie entre les individus et les groupes impliqués dans l'entreprise, contribuer au développement de l'efficacité de l'organisation industrielle et assurer la paix sociale, telles sont les ambitions des trois équipes de chercheurs examinées ici. Selon eux, leurs sciences respectives permettent, si elles sont appliquées massivement, d'atteindre ces résultats.¹³

Le fait de s'abreuver à des sources théoriques diverses n'empêche en effet pas ces chercheurs de partager un certain nombre de conceptions du monde social et de la démarche scientifique. Pour eux, la société est avant tout une société d'ordre, une entité intégrée et régulée. Les explications portent sur cette intégration et sur ces régulations. Produites au cours d'une période de troubles sociaux et politiques, ces explications portent aussi sur les écarts par rapport à l'ordre postulé, sur les "risques" de "désintégration" et de "dérégulation" et sur les remèdes à y apporter.

Tous estiment également que les méthodes mises en oeuvre par les sciences expérimentales sont des modèles que les sciences humaines doivent suivre.¹⁴ La science ainsi conçue repose sur des normes générales et universelles qui définissent la "juste ligne" des explications des phénomènes observés. Si ces normes étaient respectées, les sciences sociales seraient capables non seulement de fournir une analyse objective de la société et de ses composantes, mais aussi d'en prédire l'évolution.

Si, au cours des années 1940, des moyens financiers de plus en plus importants sont consacrés à la recherche en "relations humaines dans l'industrie"¹⁵, c'est que les dirigeants des entreprises espèrent que les méthodes des spécialistes des sciences sociales leur permettront de maîtriser les comportements individuels et collectifs. Les trois groupes de chercheurs que nous étudions dé-

clarent qu'ils sont à même de définir des systèmes qui autoriseraient les prédictions nécessaires à cette maîtrise. L'observateur, personnage commun aux trois approches, détient l'objectivité et est à même de modifier, par des interventions concrètes et locales, les modalités de fonctionnement du social, sans pour autant en altérer l'intégration.

Les "images de marque" que les trois équipes présentent à leurs clients potentiels sont nettement différenciées; elles défendent trois conceptions distinctes de la mise en oeuvre de l'intervention en entreprise.

Les membres du groupe de Harvard disent aux gestionnaires qu'avec un effort, ils pourront manipuler les systèmes sociaux comme ils le désirent; les anthropologues appliqués soutiennent qu'aucun effort n'est nécessaire si ils utilisent leur formule et enfin, les chercheurs du Committee proposent de laisser les spécialistes des sciences sociales définir les mesures susceptibles de résoudre les "problèmes sociaux" de la gestion.

Pour Mayo comme pour Henderson, le spécialiste des sciences sociales doit agir concrètement sur les relations humaines, contribuer à leur administration. Et les gestionnaires d'entreprises sont appelés à devenir des spécialistes de ce type. Les scientifiques et les dirigeants d'entreprises deviennent des êtres d'exception, capables de se débarrasser de leurs propres émotions et sentiments pour utiliser ceux de leurs subordonnés: "The administrator of the future must be able to understand the human-social facts for what they actually are, unfettered by his own emotion or prejudice."¹⁶ En ce sens, l'analyste est le détenteur privilégié d'une vision objective sur le social.

Pour les opérationnalistes, il n'est pas nécessaire d'éliminer la subjectivité de l'observateur; le raisonnement explicatif est un raisonnement où sa subjectivité intervient. Pour eux, en effet, l'explication est obtenue lorsque les solutions à un problème sont exprimées en langage familier. Comme le dit Bridgman, "l'essence d'une explication consiste à réduire une situation aux éléments qui sont si familiers que nous les acceptons comme tels, comme allant

de soi, afin que notre curiosité soit apaisée."¹⁷ Les méthodes du "génie anthropologique" (*anthropological engineering*) qu'ils définissent sont utilisables par tout un chacun; à condition de les acheter, tout gestionnaire peut s'improviser anthropologue et gérer les aspects humains (ce que Chapple appelle l'"organisation") de son entreprise à sa manière. Le but des anthropologues du groupe de Chapple est de mettre un "système de contrôle" à la disposition des responsables d'organisations.¹⁸ Ceux-ci pourraient ainsi effectuer un inventaire permanent de l'état des "relations humaines" dans l'industrie qui serait "as objective and as accurate and as capable of being used in abstract form as a basis of executive decision, as any similar 'control system' of financial accounting, physical inventory, or production units. (...) The engineer, the man skilled as a diagnostician of worker's social situations, and the executive will be on common ground, when this (...) step in the direction of a 'control system' for industrial relations is successfully taken."¹⁹

Warner et ses collègues estiment que les actions sur les systèmes ne peuvent être réalisées que par des individus qui, de par leur formation, sont habilités à saisir la théorie des systèmes sur lesquels ils travaillent; selon eux, les professionnels des sciences sociales qui agissent ponctuellement et de l'extérieur sont les mieux placés pour mener à bien ce type d'intervention.

Examinons à présent les caractéristiques des systèmes sur lesquels ces chercheurs prétendent pouvoir agir.

Les systèmes et leur limites.

Les membres des différentes équipes étudiées souscrivent tous, plus ou moins explicitement, à la définition générale que Roethlisberger et ses collaborateurs donnent du "système". Il s'agit de "something which must be considered as a whole because each part bears a relation of interdependence to every other part."²⁰ Ainsi, conçoivent-ils tous l'entreprise comme un "système".

Pour Mayo et ses collaborateurs, le système social de l'entreprise industrielle est formé par les réseaux de relations interperson-

nelles résultant des associations entre individus. Les éléments qui composent ce système sont des individus dont les sentiments et les intérêts sont mutuellement dépendants²¹ et qui, au sein de ce système, qui est, normalement, un système à l'équilibre, jouent un rôle spécifique séparé des rôles qu'ils jouent dans d'autres systèmes sociaux (famille, associations, etc.); les travailleurs agissent "together toward a common end."²² Selon eux, l'entreprise est située dans un environnement duquel elle est relativement indépendante. L'objet "entreprise" se donne à voir comme "naturel" et "autonome"; c'est ainsi que les membres de ce groupe le considèrent, sans jamais en évoquer la nature contingente ni la genèse. Pour Warner et son équipe, une telle approche n'est pas concevable. En effet, d'après eux, il existe dans chaque société une structure sociale -dite intégrative- qui détermine non seulement les opportunités des individus dans l'espace social, y compris dans l'entreprise, mais aussi les caractéristiques de l'organisation de la production, donnant, dans certaines sociétés, naissance à une forme "entreprise".²³ L'entreprise n'en reste pas moins une entité identifiable, même si elle comporte un système de statuts qui renvoient à la stratification de la structure fondamentale (intégrative) de la société en question hiérarchisée de la structure intégrative. Mais le système de l'entreprise n'est plus alors un système relativement autonome par rapport à un environnement indifférencié; il est un système hiérarchique relié à d'autres systèmes hiérarchiques. Et les modalités de fonctionnement de chacun de ces systèmes ne peuvent être appréhendées indépendamment de celles des autres systèmes. Ce qui fait le spécificité de l'entreprise, ce sont uniquement les types d'actions qui s'y déroulent, les finalités qu'elles poursuivent et les motivations qui les sous-tendent. Cette conception est bien éloignée de celle du groupe de Mayo, où l'entreprise est considérée comme une entité naturelle, nullement structurée par son environnement, et où règnent, normalement, une stabilité et une harmonie qui ne peuvent être perturbées que de l'extérieur.

Pour Chapple et Arensberg, le système que constitue l'usine est formé par les interactions entre les individus. Ce système d'interaction est un système de relations que Chapple appelle "organisation": "Every organization consists of individuals in interaction, each of whom has to achieve and maintain a state of equilibrium."²⁴ Si ce système d'interactions est, comme chez Mayo, considéré comme relativement autonome par rapport à l'environnement dans lequel il se situe, Chapple et Arensberg estiment, à la différence de leurs collègues, que les sentiments et les intérêts des individus ne peuvent pas être analysés; seules la fréquence et la durée de leurs relations sont prises en considération.

Enfin, tous les chercheurs en présence s'intéressent à la distinction résultant des découvertes "historiques" des expériences de Hawthorne entre les aspects formels et les aspects informels de l'organisation de l'entreprise. Toutefois, alors que les collègues de Mayo et de Warner mettent ces aspects informels en évidence et s'efforcent d'en définir les lois de fonctionnement, les anthropologues (qui parlent d'organisation formelle et d'organisation réelle (*actual*) entendent réconcilier les deux aspects de l'organisation. Selon eux, les conflits qui se produisent dans l'entreprise résultent du décalage qui survient entre ces deux aspects.²⁵

Ainsi, si, à Harvard comme à Chicago, il est question de système social à propos de l'entreprise industrielle, le concept n'a pas le même sens ici et là.²⁶ Dans les théories du groupe d'Elton Mayo, il est un système situé dans un environnement indifférencié alors que pour les membres du *Committee* que nous étudions ici, il est un système de relations entre des actes aux finalités bien précises et définies par la structure sociale intégrative.

Pour les membres du *Committee*, cette dernière détermine les associations des individus tout au long de leur vie. A la position de l'individu dans la structure sociale est associé un système de satisfaction et de sécurités. La structure sociale définit donc les opportunités d'individus dont les actes et les relations relèvent d'un des systèmes d'interaction sociale. Si, pour les membres de

l'équipe de Mayo, les systèmes sont a-historiques, les individus ne le sont pas vraiment; ils sont faits de petits rôles joués successivement dans différents systèmes sociaux. La juxtaposition des rôles définit la personnalité des individus. Chez Warner, Gardner et Whyte, par contre, les systèmes d'interaction sociale s'interpénètrent et convergent vers les individus placés dans la structure. Cette position détermine leurs rôles et donc leur personnalité sociale.²⁷ L'organisation industrielle n'est plus alors un système situé dans un environnement mais un système mis en relation avec les autres systèmes par des individus dont les rôles sont définis par la structure. Chez eux, la société est la production du comportement des individus et produit ces comportements. Tout en prédéfinissant les catégories, le groupe de Warner classe les comportements a posteriori, en fonction de buts qu'ils poursuivent. Cette distinction n'a de sens que si la dimension temporelle apparaît dans l'analyse; elle n'en a donc pas dans l'approche défendue par l'équipe de Mayo. Dans cette perspective, les individus sont supposés interchangeables; la place qu'ils occupent "librement" et "volontairement" dans un système social ne serait due qu'à des caractéristiques individuelles. Pour les collaborateurs de Warner, les individus sont d'abord placés, en termes de classes sociales notamment, et cette position explique leur comportement en tant qu'il relève d'une organisation sociale.

Pour Chapple et les anthropologues de son équipe, les individus sont également placés sur un pied d'égalité dans les systèmes de relations. De plus, leur comportement n'est pas déterminé par des lois psychologiques ou sociologiques, comme c'est le cas chez Mayo et Warner; ce sont des lois physiologiques qui régissent la manière dont tous les organismes (dont les êtres humains) réagissent aux modifications de leur environnement.²⁸ L'étude de l'homme est l'étude d'un organisme qui possède des propriétés déterminées et qui s'adapte (*adjustment*) continuellement à un environnement constitué par d'autres organismes et phénomènes naturels. Cette adaptation se fait grâce aux relations inter-personnelles. Dans cette perspective strictement interactionniste, les groupes qui forment

l'organisation se résumant à la somme des individus en relation qui les composent.

Recherche et intervention

Comme nous l'avons dit plus haut, les trois équipes soutiennent qu'une intervention sur les modalités de fonctionnement des systèmes est possible. Les types d'intervention qu'il prônent sont indissolublement liés aux caractéristiques qu'ils attribuent aux systèmes qu'ils analysent.

Pour Chapple et Arensberg, la stabilité de la fréquence et de la durée des interactions traduit l'existence d'un état d'équilibre.²⁹

Si cet état est jugé insatisfaisant par le responsable de cette organisation, il peut être modifié par une action sur les interactions entre individus.³⁰ Cela implique alors que l'on aille à l'en-

contre d'une "tendance naturelle" du retour à l'équilibre. Dans tout système, il existe des méthodes (routines) qui servent à maintenir l'équilibre ou à le restaurer chaque fois qu'il est menacé. L'administrateur est chargé de contrôler ces routines, de manière à orienter le système vers l'état d'équilibre qu'il désire instaurer. Les états d'équilibre sont nombreux et ne se valent pas tous. A première vue, ils adoptent une perspective identique à celle que prônent Mayo et ses collègues, pour qui l'état d'équilibre du système social, état stable, est l'objectif à atteindre.³¹

Chez Mayo, c'est l'encadrement qui est chargé de conserver l'"équilibre de l'organisation sociale de manière telle que les individus, en contribuant par leurs services au but commun à tous les membres de l'entreprise, obtiennent les satisfactions personnelles qui les rendent désireux d'y coopérer."³² La tâche du chercheur est de découvrir la nature des déséquilibres et des interférences.³³

Mais, alors que pour les membres de l'équipe de Mayo, l'état d'équilibre est une norme sociale, résultant de l'interaction des intérêts et des sentiments des individus qui composent le système, pour Chapple et Arensberg, le critère de l'équilibre est un critère de stabilité d'interaction, sans contenu. Pour eux, c'est en agissant sur le processus culturel (sur l'organisation) que le système

interactionnel peut être contrôlé et son fonctionnement "amélioré". Etant donné le poids des déterminants physiologiques dans le comportement des individus, il est, selon Chapple et Arensberg, impossible d'agir sur celui-ci; il faut, selon le résultat que l'on désire atteindre, encourager et stabiliser certaines interactions, et en décourager d'autres.

En revanche, pour les membres du Committee, comme pour ceux de l'équipe de Mayo, l'intervention porte sur l'individu; c'est en agissant sur l'individu que l'on peut modifier le fonctionnement du système. Ces interventions se font néanmoins sur base de principes différents, liés aux caractéristiques de leurs systèmes. Comme, pour Mayo et ses collègues, les individus ne sont présents dans l'entreprise que pour jouer leurs rôles de travailleurs (leurs autres rôles relèvent de l'environnement du système, hostile et inaccessible), c'est sur les motivations de l'individu-travailleur que repose la philosophie de l'intervention. En revanche, comme chez Warner et ses collaborateurs, l'entreprise est un système d'actes relié à d'autres systèmes d'actes par l'intermédiaire des individus, c'est sur l'ensemble des frustrations et des satisfactions relatives à leur position dans la structure sociale (leur "personnalité sociale") que porte l'intervention. Les comportements au travail ne peuvent être modifiés que si la vie hors travail est prise en considération.

Si, chez Mayo et ses collègues, la relation entre l'approche théorique de l'entreprise et la conception de l'intervention est évidente, elle l'est beaucoup moins dans le cas du Committee. Comment, plus précisément, arrivent-ils à mettre au service des entreprises un modèle dans lequel la référence à la totalité sociale est fondamentale? De fait, la rencontre entre leur position théorique et leur conception de l'intervention va, non pas remettre en question les bases de leur démarche, mais bien néanmoins produire un effet paradoxal. Pour répondre à la demande de leurs commanditaires industriels, ils vont en quelque sorte pousser à son terme, mais pas dans n'importe quel sens, la logique de leur approche du social. Sans pour autant renier la production sociale de l'institution

entreprise, fruit du développement d'une structure sociale intégrative, ces chercheurs vont "clôturer" le système qu'ils doivent contribuer à gérer. La situation de travail, les systèmes de statut, l'entreprise elle-même, seront pris comme donnés; l'analyse globale est délaissée au profit de la gestion locale. Et, pour pouvoir proposer des réformes locales sans avoir à remettre en cause tout le système (c'est-à-dire la structure sociale intégrative de la société considérée), les membres du *Committee* vont jouer sur le seul élément qui n'a pas de rôle actif dans leur analyse sociale: l'individu. C'est en formulant des recommandations visant à améliorer l'adaptation des individus aux exigences de la structure sociale (en l'occurrence de l'entreprise) qu'ils vont aborder le marché de l'expertise. C'est désormais uniquement par l'intermédiaire des individus que la société va pénétrer dans l'entreprise: leurs comportements sont déterminés par des caractéristiques sociales variées dont il convient de tenir compte pour gérer les "relations humaines". Le résultat paradoxal de cette évolution est que la démarche de Warner et ses collègues, sur le plan pratique, rejoint celle de l'équipe de Mayo, tout en la raffinant, puisqu'elle précise les traits de ce qui, du point de vue de Mayo et ses collègues, est une des principales causes potentielles de rupture de l'état d'équilibre du système social.

Conclusion

La volonté d'obtenir une position optimale dans le marché de l'expertise sociale en entreprise, au moment où le modèle de Henderson domine les sciences sociales à Harvard, constitue le principal déterminant de l'éclatement du groupe impliqué avant 1939 dans les travaux menés à la *Harvard Business School*. Toutefois, les constructions conceptuelles sur lesquelles reposent leurs conceptions de l'intervention sont cohérentes avec les traditions théoriques dans lesquelles s'insèrent les différentes équipes que nous avons comparées. Leurs visions de l'entreprise et de son fonctionnement répondent simultanément à la "demande sociale" émanant des entreprises et à leurs credos scientifiques.

La rencontre avec cette "demande sociale" fut plus ou moins réussie, selon les groupes. Le succès remporté par ces trois approches auprès des gestionnaires d'entreprises susceptibles de financer des interventions fut en effet inégal. Peu de commanditaires paraissent avoir manifesté de l'intérêt pour la recette opérationnaliste dans la forme pure qu'elle a à cette époque. Seules quelques entreprises dont la *Southern New England Telephone Cy*, semblent avoir recouru aux services de l'équipe de Chapple.

Le groupe de Mayo par contre a réalisé d'assez nombreuses interventions; la *Western Electric Cy*, *General Motors* et *Macy's* ont notamment fait appel à lui. De plus, le *War Production Board* a chargé cette équipe de remédier à l'absentéisme dans trois entreprises de fabrications métalliques. Toutefois, la lutte entre approches concurrentes pour la conquête de marché de l'expertise sociale en entreprise a été, sans aucun doute, remportée par le *Committee*. De très nombreuses entreprises, dont *Sears*, *Roebuck and Cy*, la *Container Corporation of America*, *Libby*, *Mac Neill and Libby*, *S. Buchsbaum and Cy*, et la *Visking Corporation*, recourent à leurs services. Encouragé par ce succès, Gardner ouvre, en 1947, une officine d'expertise dont Warner sera conseiller. Au détriment de la démarche formaliste des anthropologues appliqués et de l'approche limitée aux rôles des individus dans l'entreprise, c'est le modèle axé sur les transformations du comportement de l'individu conçu comme appartenant à plusieurs sociaux interdépendants qui triomphe. C'est autour de ce modèle qui, du fait sa clôture, rejoint celui du groupe de Mayo, que les praticiens des "relations humaines" se retrouveront, après la seconde guerre mondiale.

Notes

1. Nous ne nous intéressons ici qu'aux travaux des chercheurs qui ont enseigné ou fait leurs études à Harvard. Nous ne parlerons donc pas, par exemple, des membres du *Committee* qui proviennent d'autres horizons. De plus, nous nous limitons à la période 1939-1946 (de la parution du *Management and the Worker* à la reconnaissance de la sociologie industrielle par l'*American Sociological Association*), ce qui nous amène à éliminer de notre analyse la plupart des travaux de G. Homans et de W.F. Whyte.
2. Elle sera sous-titrée *Problems of Human Organization* à partir de vol. 1, n° 4, july august 1942.
3. A tort, puisqu'il y inclut des travaux de Chapple et Arensberg. Cf. Rose (1975, p. 142 et p. 147).
4. Rose (1975, p. 147).
5. Roethlisberger et al. (1939, p. 551).
6. Warner, Lunt (1941, p. 14 et p. 28 sq); Radcliffe-Brown (1961).
7. Bridgman (1927).
8. Chapple in collaboration with Arensberg (1940).
9. Chapple (1943, p. 24). Pour une position similaire en sociologie, cf. Lundberg (1942).
10. cf. Desmarez (1983).
11. cf. Russett (1966, pp. 111-124).
12. Il est symptomatique de constater que les travaux menés à Newburyport entre 1930 et 1935 par l'équipe de Warner n'aient été publiées qu'à partir de 1941, et à Yale.
13. Whyte (1946, p. 2); Mayo (1945); Roethlisberger (1941); Chapple (1941a, p. 2).
14. Warner, Lunt (1941); Mayo (1945); Chapple (1941b, p. 4).
15. cf. par exemple: "New Help in Field of Social Studies", *New York Times*, May 12, 1946; Whyte (1969); Baritz (1960).
16. Mayo (1945, p. 122); Henderson (1941-1942, p. 83).
17. Bridgman (1927, p. 37).
18. cf. Chapple (1941b, p. 4).
19. Arensberg (1941, p. 57).
20. Roethlisberger et al. (1939, p. 551).
21. Roethlisberger et al. (1939, p. 364, note).
22. Roethlisberger et al. (1939, p. 553).
23. Warner Lunt (1941, p. 14 et p. 28 sq) Cf. aussi Gardner (1945) et Gardner (1946).
24. Chapple (1942, p. 165).
25. Arensberg, Mc Gregor (1942, pp. 33-34).
26. Gardner, Whyte (1946, p. 511).
27. Ces auteurs empruntent, sans le signaler explicitement, leur définition de la personnalité sociale à Radcliffe-Brown (1940, p. 279).
28. Chapple (1941b, p. 4).
29. Chapple, Arensberg (1940) et Chapple (1942).
30. Chapple, (1941b, p. 32).

31. Roethlisberger et al. (1939, p. 567).
32. Roethlisberger et al. (1939, p. 569). Cf. aussi Mayo (1945, pp. 69-70).
33. Cf. Mayo (1933, p. 26).

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HISTORICAL ISSUES IN CONTEMPORARY HUNGARIAN
PSYCHOLOGY

Ferenc Erös

Institute of Psychology of the Hungarian Academy of
Sciences, Budapest.

Summary

This paper examines the problem why both the historical consciousness of psychology and the history of psychology as a field have been relatively underdeveloped in Hungary. It describes the vicissitudes of psychology under Stalinism, then it gives a picture of the circumstances in which psychology was "rediscovered" in the late fifties and early sixties. It attempts to explain why and how disturbing questions about the role of psychology in society started to be raised by a great number of psychologists during the last few years. Finally, the paper claims that psychologists in Hungary now are faced by a dilemma: either to accept the guidance of state "paternalism", or to explore new, alternative ways and possibilities of their presence in society and of their conceptions of the nature of "social problems".

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Up to the present, there was no serious interest in the history of Hungarian psychology in Hungary.¹ Although psychology in this country has a past of more than eighty years and in the course of its history a number of prominent Hungarian contributors, like Paul Ranschburg, Géza Révész, Sándor Ferenczi, Géza Róheim, David Rappaport and others, obtained international reputation, the average Hungarian psychologist is, as a rule, much better informed on the history of, let us say, American psychology, at least on the subsequent phases of the development of behaviourism, than on the history of the field in his or her native country. There is a number of factors behind this neglect of history. I will enumerate here only those which I consider to be the most important ones.

1. Psychology in Hungary, like in other East European countries, practically ceased to exist after the Stalinist ideological

and political turn in 1948/49, thus sharing the fate of the other so-called "bourgeois pseudosciences" of this period, like genetics or cybernetics. The only areas which have been cultivated between 1949 and 1959 were Pavlovian psychophysiology and - to some extent - applied research in education.

2. As a consequence of "banning" their profession and eliminating the scientific and institutional network of the field, psychologists found themselves almost in a void by the late fifties and early sixties, when most of the ideological arguments and reservations against psychology were officially removed or, at least, their strength decreased gradually. The historical continuity of Hungarian psychology was interrupted in the period of silence; as a consequence, a wide generational gap resulted between the older and the younger generations of psychologists.

3. In the period of rebirth and rapid reestablishment of the field in the sixties, Hungarian psychologists were, perhaps understandably, less concerned with the past when they tried to catch up with the developments of contemporary psychology - mostly through assimilating modern, predominantly American, theories and methods. In the course of this assimilation, the ahistorical metatheory and positivistic methodology of "mainstream" American psychology were incorporated. In addition, academic training in psychology was based on positivism and the field was defined almost exclusively as a natural science. The "new field" attracted a great number of people with backgrounds in exact sciences such as biology, medicine and technical sciences, who obtained degrees in psychology as a second diploma. In such circumstances, the general level of knowledge and interest in social and historical problems remained, as a rule, very low among the majority of psychologists of this era. It should be added to this general picture that the reception of Soviet psychology in Hungary was highly ambivalent. Much "lip service" was paid to the works of Rubinstein, Vygotsky, Leontiev and others, but a deeper understanding of the basic problems and principles of Soviet psychology was practically missing. Soviet psychology, notably the cultural-historical school of Lev Vygotsky

and his followers, had a more serious impact only on the discussions about the general theoretical problems of psychology, that is, on the few Hungarian attempts to establish a relationship between Marxist anthropology and psychology.²

As far as the role of Marxism in general is concerned, it is interesting to note that it was largely external. In most of the cases, it did not go beyond some references to the abstract principles of dialectical materialism, or to the "dialectics of nature". The new trends in the Marxism of the sixties - what Georg Lukács called "the renaissance of Marxism" - did not get any serious response on the side of psychology. This silence was partly due to the "anti-psychological" bias which is more or less characteristic of the work of Lukács and some of his followers.

4. The rapid economic development of the sixties, and the extensive changes in the socio-economic structure of the country, created a favourable atmosphere for the development of the social and behavioral sciences, including psychology. This development gave rise to the illusion that psychology may play a unique role in making "human relations" more human in a socialist society.³ In these conditions of relative prosperity, a powerful practicicism started to dominate the field. Psychologists wanted to - and also, had to - prove the usefulness of their activities and the relevance of their knowledge. To show their "usefulness" also seemed to be a way of legitimating the whole profession and defending it against the threatening revivals of the "anti-psychologism" of dogmatic ideology.

5. In their attempt to possibly avoid all conflicts with ideology, psychologists tended to stress very carefully the *limits* of their field and to separate it from other disciplines such as sociology, history and philosophy. Even "interdisciplinary" fields like social psychology, have been more influenced by general psychological theories and laboratory research techniques than by current social thought. This fact also explains why Hungarian psychology as a whole was so irresponsive to the critical, anti-positivist wave of the sixties in Western psychology. Attempts to

raise similar issues would have been - and were in fact - strongly refused by the majority of psychologists, as constituting extra-scientific, ideological criticism.⁴

6. It must be noted that the neglect of the historical problems of the field is not unique to psychology; for various ideological and political reasons, intellectual history has been, in general, a relatively underdeveloped field in Hungary.

After a period of reconstruction, however, Hungarian psychology reached its more or less "normal", "mature" phase in the late seventies, in terms of publications, research facilities, organizations and institutions, training, professional standards, legal aspects, international contacts, etc. Meanwhile, a new generation of psychologists has grown up - a new generation whose members did not share the bitter ideological experiences of their older colleagues. For younger people, the raising of disturbing questions about the role of psychology is no longer necessarily equal to a Stalinist claim of "banning" or restricting the field. The sources of these disturbing questions are manifold. In general, there is a gradual disappointment concerning the earlier ideas about the role psychology could play in changing the social life of the country for the better. I mention here only some problems which may be the causes of the present discontent, which are broadly shared by psychologists.

1. Government boards and organizations expect psychologists to do their share in solving such large scale social problems as the growing dissatisfaction and the various forms of so-called deviant behavior among the youth; the psycho-social consequences of technological rationalization and social mobility; alcoholism and drug abuse; the permanently high suicide rate - which is, according to the latest statistics, one of the highest in the world -; the cultural deprivations and achievement failures suffered by people belonging to the economically and socially underprivileged groups. There is, however, a growing awareness of the fact that, on the one hand, despite all developments, Hungarian psychology still lacks a

satisfactory institutional network for providing society with adequate therapeutic aid for those who really need it; on the other hand, "psychologization" of social problems, the "blame the victim" approach, can become an excuse for them to postpone radical changes in the government's social policy.

2. The professional identity of psychologists, their role as state agents becomes more and more doubtful or ambivalent. The centralistic conception of the aim and scope of their activities seems to be extremely narrow if one takes into account the multiple problems of different social groups such as youth, women, mental patients, the poor, ethnic and other minorities etc., which do not possess the proper democratic forms of the representation of their interests.

3. Psychologists have to work in a social atmosphere which is still permeated with prejudice, intolerance, authoritarianism and the like, which result from unsolved and accumulated historical conflicts and which are also reproduced continuously by present social tensions.

Due to these recent changes in the conceptions the psychologist's role and possibilities in contemporary Hungarian society, there is a growing awareness of the importance of the socio-historical factors which are responsible for the present state of psychology in Hungary. I will now summarize the historical issues which seem to be the most relevant ones.

First of all, it is necessary to modify the standard image of the linear and cumulative development of Hungarian psychology, interrupted only by a decade of "banishment" in the fifties. Psychology as an intellectual endeavor has a long past in Hungary indeed: from the beginning of the century on, it acquired an outstanding role in the ideological debates about the nature of man and society.⁵ One should, however, be reminded of the fact that, for example, even by the late thirties there were not more than three persons in the country who had obtained a diploma in psychology and each of them obtained their degree abroad. The Hungarian Psychological Association was founded as late as 1928 and the

Institute of Psychology of Budapest University was opened even later, in 1932, without, however, offering a full training program in psychology. People who called themselves psychologists were other professionals by training and education: physicians, psychoanalysts, philosophers, etc. The government gave hardly any support to its small number of public institutions which deal with educational and vocational guidance. It is symptomatic that such prominent psychologists like Paul Ranschburg and, later, Lipót Szondi had to work mostly with unpaid collaborators. These laboratories were funded by charitable organizations. On the other hand, from the late twenties on, industrial and commercial psychology became a fairly advanced area adopting contemporary American and German psychotechnical methods; psychotechnics was supported and fostered by private organizations. Another advanced area of Hungarian psychology between the two World Wars was psychoanalysis, which was fairly popular among the Jewish middle class and intelligentsia, without having, however, an institutional background.

All these facts show that the institutionalization of Hungarian psychology was a late phenomenon; it was completed only in the past twenty years. The government started to seriously realize the importance of the expansion of psychological services not earlier than in the period of war: it was in the framework of the army that systematic testing and other psychotechnical methods of vocational guidance were introduced.⁶ The introduction of army psychology was the real beginning of the institutionalization of Hungarian psychology. This trend continued in the brief period between 1945-48, when psychological testing methods were adopted for the selection of gifted young people from the peasantry and from the working class for higher education as well as for various administrative jobs and political posts.

In summary, we might say that the development of psychology in Hungary is characterized by some specific features. An essential characteristic of this development is that the systematic applications of psychological knowledge only started with considerable delay and even this slow process of development was interrupted

during the fifties. The circulation of psychological ideas as "free knowledge" preceded their social functioning by a long time.⁸ However, as soon as the necessity of application on a social scale became fully recognized, psychological knowledge and techniques started to be used and exploited under the guidance of the central state administration which tended to see in them a tool for achieving some political and social goals: first of all, the *selection*, i.e. the regulation and controlling of social mobility. Professionalization, i.e. the appearance of groups of specialists which monopolized psychological knowledge, is an even later development and also strictly controlled by the state. Thus, psychology as a profession was fully legitimated in Hungary only in the sixties.

The present alternatives for Hungarian psychology can be characterized in the following way: it is a relatively new profession, which has grown up under the protection of state paternalism. It has, however, a long intellectual tradition in liberal and radical values which are contradictory to the spirit of that very paternalism and conservatism. Psychologists now have to face a dilemma: either to follow the guidance of paternalism and consequently, to ignore the challenges which are threatening its conservative values, or to explore alternative ways and possibilities; i.e. to investigate how psychology could contribute to the critique of society and in which ways psychological knowledge could be integrated into the intellectual resources of the radical reform movements which aim at the transformation of present social relations into a pluralistic democratic socialism.

Notes

1. A comprehensive history of Hungarian psychology has still to be written. For a short outline of the history of Hungarian psychology in the 20th. C. see Pataki (1977).
2. See Garai (1969) and Erös (1974).
3. On the roots of this illusion see Garai (1973) and Garai *et al.* (1979).
4. See for more details Garai *et al.* (1979).
5. On the political and ideological contexts of the early history of Hungarian psychology see Erös (1982).
6. The use of psychological methods in the selection of army personnel was introduced in Hungary mainly as an import from Germany. It is an interesting task for a historian to study how and to what extent "Nazi psychology" had an impact in East and Central European countries under German influence. On German psychology in Nazi times see Geuter (1982).
7. On the specific features of the East and Central European ways of development in psychology as compared to the North American way see Erös (1981). See for the more general context Danziger (1979).

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SPENCER EN FRANCE ET AILLEURS

Paul Henry

C.N.R.S.-E.H.E.S.S.

Paris

Summary

Few English scientists are honoured so much and so rapidly by the French as Spencer. From 1871 onwards - the first edition of the translation of the *First Principles* by E. Cazelles - till the end of the century hardly one year went by without a new publication by Spencer being brought out. Up to the second World War his work had become a major reference in France - his writings, his theses, his name, were touched upon in numerous discussions. In this paper the extent and the dimensions of Spencer's reception in France up to the Second World War will be traced.

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Si on laisse de côté ses premiers ouvrages (*Social Statics*, la première version - 1855 - des *Principles of Psychology* et les *First Principles*, qui n'eurent pas en Angleterre même le retentissement escompté par leur auteur), la rapidité avec laquelle la plupart des ouvrages de Spencer ont été traduits et édités en français, le grand nombre de rééditions successives de ses traductions, voire la publication de nouvelles traductions, attirent l'attention. Peu d'anglais ont été à ce point favorisés par leur voisins d'Outre-Manche. De 1871 (première édition de la traduction des *First Principles* par E. Cazelles) jusqu'à la fin du siècle, il ne s'est quère écoulé d'année sans que paraisse en français un nouvel ouvrage de Spencer. Jusqu'à la seconde guerre mondiale son oeuvre était devenu en France une référence de premier plan. Ses écrits, ses thèses, son nom ont été évoqués dans de très nombreuses polémiques. Voici quelques repères permettant de circonscrire l'étendue et les dimensions de son audience en France jusqu'à la seconde guerre mondiale.

Dès 1870, Ribot (le traducteur avec Espinas de la deuxième version des *Principles of Psychology* parue en 1875) consacre un

long chapitre à Spencer dans sa *Psychologie anglaise contemporaine*. Dans la seconde édition, parue elle aussi en 1875, ce chapitre devient le plus long de l'ouvrage. Spencer y est présenté comme l'égal de Comte et comme ayant surpassé Leibniz et Hegel dans l'analyse concrète du progrès. A. Espinas lui-même, dans son *Des sociétés animales: études de psychologie comparée*, dont la seconde édition est parue en 1878, se réfère à Spencer et au parallèle entre organisation biologique et organisation sociale pour développer sa comparaison entre sociétés animales et sociétés humaines. Liébault, dans son *Ebauche de Psychologie* de 1873 faisait déjà état des conceptions évolutionnistes de Spencer. Celui-ci est également mis à contribution dans l'un des tous premiers articles de Binet "Le raisonnement dans les perceptions", parue en 1883 dans la *Revue Philosophique*. Renouvier, l'un de ceux qui dès les années 1840. avec Cournot, ont lancé l'appel au "retour à Kant" dans la philosophie française pour combattre l'éclectisme des disciples de Victor Cousin, a consacré tout, un ouvrage à l'examen des *First Principles*, paru en 1886. La question des rapport entre Comte et Spencer a fait l'objet d'un livre de E. Roberty paru en 1894. Durkheim, dans *Les Règles de la méthode sociologique* (1894) a pris soin de se démarquer de l'individualisme spencerien en même temps qu'il soulignait le caractère à ses yeux essentiellement philosophique de la question traitée par Spencer dans sa volumineuse sociologie, celle de l'application de la loi universelle de l'évolution aux sociétés. Pour Durkheim, soucieux de fonder l'autonomie méthodologique et théorique de la sociologie, la question spencerienne ne soulevait pas les problèmes méthodologiques spécifiques à l'observation des faits sociaux et le "continuisme" de Spencer était un danger. Il opposa donc Comte à Spencer. Spencer est à nouveau évoqué dans le contexte de l'analyse des rapports entre sociétés animales et sociétés humaines dans *Organisme et Société* de R. Worms, publié en 1896. Dans la *Grande Encyclopedie*, publié entre 1885 et 1902, figure, sous la signature de André Berthelot, un long article, fort complet, consacré à Spencer. Celui-ci est évidemment en bonne place dans *l'Evolution créatrice* de Bergson (1907), bien

que Bergson substitue à l'évidence empirique du phénomène de l'évolution l'expérience immédiate de l'Etre en tant que changement et mouvement. Emile Meyerson, lui, dans *Identité et Réalité* (1907), s'est intéressé aux conceptions de Spencer en matière de "physique théorique" (conservation de la matière, inertie, conservation de l'énergie), ainsi qu'à sa théorie des "demie-périodes d'organisation et désorganisation" cycliques. Meyerson a souligné ce que l'on peut appeler le "paradoxe de Spencer", celui auquel il a été conduit pour soutenir (dans une perspective néo-kantienne) le caractère de vérité déductible a priori, axiomatique, des principes de conservation, tout en expliquant pourquoi ses principes n'ont été reconnues incontestables que très tard dans l'histoire des sciences. Selon Spencer, rappelons-le, si l'on a souscrit pendant longtemps au contraire de ces vérités aprioriques (en considérant par exemple que la matière pouvait devenir inexistante pour surgir du néant), c'est parce qu'on n'avait pas pensé effectivement ces contre-vérités, qui ne sauraient être "réalisée par la pensée". De la sorte, concluait Spencer, "les hommes ont supposé qu'il croyaient quelque chose qu'ils ne croyaient point"¹. En 1914, dans un très curieux ouvrage du père Muñiz, publié sous le titre *Etudes de Positivisme métaphysique*, qui est une défense positiviste des dogmes créationistes, la cible privilégiée est Spencer. Par le nombre des références, il arrive juste après saint Augustin, Aristote et Kant, bien avant Darwin qui est relativement ménagé. Plus près de nous Spencer est une référence clef dans les différents volumes du *Nouveau Traité de Psychologie* publié sous la direction de G. Dumas entre 1923 et 1938. Enfin, dans *Phénomène social et sociétés animales* (1937) de Etienne Rabaud, Spencer figure en bonne place à côté de Comte comme l'un des précurseurs de l'organicisme, son originalité par rapport à Comte étant, selon Rabaud, de s'être engagé dans une voie plus directement biologique, ce qui aurait donné à sa démarche un tour plus moderne.

Ce rapide inventaire des références à Spencer, qui ne prétend nullement à l'exhaustivité, permet de se faire une idée de la

variété des domaines et des polémiques dans lesquels son oeuvre ou son nom ont été mis à contribution. On en retiendra en particulier que ce sont les psychologues qui se sont sentis le plus concernés par l'oeuvre de Spencer, alors que les sociologues, notamment Durkheim, plus fidèles à Comte, s'en sont démarqués assez vite. Un nom ne figure pas dans cet inventaire: celui de Gustave Le Bon. A l'oeuvre de celui-ci, dans son rapport à celle de Spencer, il convient de faire une place à part. Il n'est pas question d'exposer ici même une comparaison systématique des positions de Spencer et de Le Bon. Pour qui connaît peu ou prou ces écrits, toute une série de rapprochements s'impose. Notamment l'exploitation de l'opposition entre homogène et hétérogène, la conception de l'évolution comme passage de l'homogène à l'hétérogène, (déjà posée par von Baer et prenant la place de l'opposition simple-complexe chez Lamarck), le rôle du couple différenciation-intégration (déjà exploité lui aussi par Lamarck), l'idée du caractère cyclique de l'évolution avec des demi-périodes d'organisation et désorganisation (s'opposant au "plan de la nature" lamarckien). Les rapprochent surtout leur évolutionisme généralisé encore plus radical chez Le Bon que chez Spencer, puisque le premier est allé jusqu'à soutenir, par exemple, que la loi d'évolution applicable aux êtres vivants l'est également aux corps simples, les espèces chimiques n'étant, selon lui, pas plus invariables que les espèces vivantes. Sur d'autres points Spencer et Le Bon ont soutenue des positions diamétralement opposées, notamment sur l'indestructibilité de la matière et de l'énergie. Celles-ci étaient pour Spencer des vérités axiomatiques, dont le contraire serait impensable, alors que Le Bon postulait non seulement la transformation de la matière en énergie mais encore le possible "évanouissement" de l'une comme de l'autre. Sur le terrain de la philosophie politique, le libéralisme radical, tout évolutioniste qu'il soit, de l'auteur de *L'individu contre l'Etat* et le désir du psychologue des foules de donner aux hommes d'Etat les connaissances de base pour ne point se laisser entièrement gouverner par les foules, paraissent également s'opposer. Enfin il n'y a rien d'équivalent aux "instincts héréditaire de la

race", à la croyance, la suggestibilité et l'imitation de Le Bon chez Spencer. La sympathie et l'altruisme instinctuels de Spencer (fondements selon lui de la socialité humaine ou animale) en paraissent, par comparaison, être des figures inversées. Enfin Le Bon est un de ceux qui ont le plus contribué à la popularisation de l'idée de "crise" ou d'"anarchie dans les sciences", ce qui n'est pas du tout le cas de Spencer en qui les "rationalistes" ont pu voir un allié.

Il y a donc bien une thématique commune à Spencer et à Le Bon, même s'il s'y présente des inversions. Pris chacun isolément les éléments de cette thématique ne leur sont pas spécifiques. Certains avaient déjà à l'époque une longue histoire. Il n'en va pas de même de leur articulation laquelle constitue la charpente théorique d'une thématique globale.

Il se trouve que Le Bon n'a qu'exceptionnellement fait référence à Spencer, ce qui peut surprendre vue l'audience incontestable de Spencer. En tout état de cause Le Bon ne s'est pas référé à Spencer, pas plus qu'à quiconque pour ce qui concerne son évolutionisme généralisé pour lequel il pourrait pourtant apparaître comme son disciple fort peu reconnaissant. Il est vrai que Le Bon, dans son souci constant de faire apparaître sa propre pensée fondamentalement originale, n'a fait que de façon rarissime état de ses emprunts théoriques (ce qui n'est pas le cas de Spencer). Le Bon fut moins avare de références à Darwin bien qu'il considérât que sa théorie de l'évolution était en passe d'être dépassée par celle des "mutations brusques". Le caractère beaucoup plus limité de l'entreprise de Darwin pouvait peut-être lui permettre de se réclamer de lui tout laissant place à ce qui aurait constitué son apport propre. Pourtant, par les caractéristiques de son évolutionisme, Le Bon est beaucoup plus proche de Spencer que de Darwin. C'est de Lamarck que leur évolutionisme rapprocherait le plus Spencer et Le Bon à ceci près que, on aura beau chercher, il n'y a chez Lamarck aucune trace de psychologie aussi bien à la Spencer qu'à Le Bon. Spencer est à peu près aussi avare de référence à

Lamarck que Le Bon l'est à l'égard de l'un et de l'autre. Mais ce qui en fin de compte, au-delà de leurs divergences, rassemble Spencer et Le Bon, c'est la manière dont ils ont mis l'évolutionisme à contribution dans la constitution du champ de la psychologie, qu'elle soit individuelle ou collective.

Au regard de l'analyse de la signification historique de l'audience de Spencer en France, il est de peu d'intérêt de s'engager dans la discussion de savoir si Le Bon, quoiqu'il en laisse paraître, ne doit pas être tenu pour le principal disciple de Spencer en France, tout comme on soutient que Spencer est celui de Comte en Angleterre. Il serait tout aussi peu pertinent de les suivre l'un et l'autre dans leurs efforts quelque peu paranoïdes, nettement plus accentués chez Le Bon, de faire apparaître leurs idées comme intrinséquement originales. De même, en rester à la célébrité acquise par Spencer Outre-Manche pour expliquer son audience en France (ou ailleurs) resterait bien superficiel. De la comparaison qui précède, aussi schématique qu'elle soit dans cette présentation succincte, on peut tirer d'autres indications, de plus de portée, relatives à l'historicité des doctrines et de la thématique en question, à leur ajustement, coordination ou adéquation avec une conjoncture historique déterminée. La grande popularité de l'oeuvre de Le Bon, son exceptionnelle diffusion, permet d'expliquer au moins partiellement l'audience de Spencer en France, jusque dans la différence spécifique de cette popularité et de cette audience.

Spencer ne s'est pas heurté comme Le Bon à l'ostracisme des universitaires français, bien au contraire comme on a pu le voir. Il y a là une différence significative. L'oeuvre de Spencer fut produite à un moment où l'Angleterre, avec notamment l'abolition des lois protectionnistes sur les grains et d'autres mesures politiques y assurant la dominance structurelle du capitalisme industriel sur le capitalisme foncier, s'est engagée concrètement dans la voie du libéralisme économique de façon complète et avec les

contre-parties politiques et idéologiques que cela supposait. C'est à cette conjoncture que l'oeuvre de Spencer est coordonnée (ce qui ne veut pas dire que cette conjoncture "explique" cette oeuvre). L'évolutionisme généralisé de Spencer, allant sans *solution de continuité* de l'atome jusqu'au Cosmos en passant par les éléments, la matière, le vivant, l'homme, la société, trouve place dans l'ensemble des tentatives contradictoires de reformulation des doctrines libérales qui sont apparues en Angleterre à ce moment là et qui visaient à ajuster celles-ci à la nouvelle conjoncture en se démarquant de l'artificialité du politique impliquée dans les doctrines issues de Hobbes et de Locke. En Angleterre, pendant cette période, a été réalisée une refonte générale de la philosophie politique libérale qui s'est étendue bien au-delà de celle-ci. Dans cette refonte, même si elles ont visé globalement les mêmes objectifs, des voix discordantes se sont fait entendre. Aux thèses de Spencer se sont opposées celles de l'école d'Oxford et notamment de T.H. Green, qui, sur un fond de néo-hégélianisme, a mis en question le laissez-faire spencerien et a insisté sur l'action positive de l'Etat. La rébellion contre l'Etat, chez Spencer et chez Green, n'a pas le même sens. Chez Spencer elle vise à limiter strictement le rôle de l'Etat par une critique du well-fare-state et au nom du "laissez-faire". Chez Green, elle n'a de sens que pour obliger l'Etat à assumer ce qui fait sa raison d'être: assurer, par la loi et le respect de celle-ci, les conditions du progrès moral individuel et, par là de la réalisation individuelle de soi. Le libéralisme radical de Spencer avec son fond de "continuisme évolutioniste" et le conservatisme de Green se présente l'un et l'autre, sur des bases différentes, comme des théories du devenir historique, du développement et du progrès, sans rupture, sans discontinuité, bref sans révolution. Quelqu'aient été les sympathies de Spencer pour l'Owenisme dont il s'est peu à peu éloigné, il n'en reste pas moins que sa philosophie est une philosophie de la non-révolution. C'est dans cet espace qu'il convient de situer la psychologie spencerienne en tant que maillon essentiel d'un dispositif philosophique et idéologique. Cette psychologie

problématise le développement de la pensée et de la conscience, en rupture avec la tradition empiriste, notamment avec Stuart Mill. Mais, ce faisant, elle introduit une problématique qui n'avait pas de place dans l'évolutionisme lamarckien (il faut rappeler que pour Lamarck, l'homme a une origine différente des animaux, que ce n'est que du point de vue de son *organisation* qu'il peut être considéré dans l'évolution des espèces animales). La psychologie de Spencer postule bien que la pensée et son développement à l'échelle de l'individu participe du processus général de l'évolution. Tel est le noyau de sa psychologie qui reste une psychologie du "laissez-faire", dans laquelle toute intervention externe est perturbatrice du développement naturel qu'elle ne peut que retarder. On retrouve dans la psychologie de Spencer ce qui, sur un autre plan, le sépare de Green, lequel considère comme nécessaire une intervention externe, celle d'une "volonté générale" pour que l'individu se réalise. En d'autres termes on pourrait dire que pour Spencer la loi du développement est interne alors que pour Green elle ne saurait être qu'externe.

Si maintenant nous nous tournons vers la France à l'époque où l'audience de Spencer s'y est développée, la situation idéologique et politique y était fort différente de ce qu'elle était en Angleterre à la même époque. En 1870, c'était la Commune, venant après toute la série de révolutions et de coups d'Etat qu'elle avait connu depuis 1789. Le second Empire avait réalisé les réformes et mis en place les structures y assurant la dominance du capitalisme industriel sur le capitalisme foncier mais la question de la révolution politique y était posée d'une manière fort différente de ce qu'elle pouvait l'être en Angleterre. La IIIème République a été appelée, non sans raison la "République des professeurs". C'était là le signe d'un rapport particulier de l'Université à la politique, relativement nouveau en France. Que les universitaires français aient trouvé dans la philosophie de Spencer matière à penser leur propre rapport à la politique en des termes qui semblaient pouvoir contourner la perspective révolutionnaire, ou tout

au moins d'en faire l'économie, est concevable. L'universitaire français type de l'époque est bien un notable libéral. Il veut croire à la possibilité d'un progrès qui ne passe pas par des convulsions révolutionnaires. Et il est jaloux de l'autonomie de l'Université par rapport au pouvoir politique. La position de Le Bon est toute différente: il est proprement hypnotisé par la menace des désordres révolutionnaires. Le "laissez-faire" pour lui ne peut conduire qu'au désordre. Il croit aux grands-hommes. Jusqu'à un certain point, on peut dire que les positions de Spencer et celles de Le Bon pouvaient, sous des modalités ajustées à la conjoncture françaises, représenter le même couple de positions que celle de Spencer et de Green pour l'Angleterre. C'est dans ce contexte que s'est développée la psychologie moderne en France. On ne saurait dire que la psychologie universitaire française est d'inspiration spencerienne. On peut tout au plus remarquer qu'on y retrouve beaucoup de positions qui furent aussi celles de Spencer. Avec d'autres facteurs, ceci a donné à cette psychologie une forme particulière qui peut expliquer les résistances à la pénétration du behaviorisme en France². C'est en effet dans certains courants du behaviorisme que l'on peut trouver quelque chose qui, sous certains de ces aspects, nous rapproche de la position de Le Bon. Je pense en particulier à Skinner en ce qu'il considère comme nécessaire une détermination externe, le conditionnement par lui-même pouvant produire le meilleur comme le pire. Car la dualité de la psychologie de Ribot ou de Binet et de celle de Le Bon se retrouve dans la psychologie d'aujourd'hui sous d'autres modalités. C'est donc vers l'historicité de cette dualité que l'analyse de l'accueil réservé en France aux oeuvres de Spencer nous incite à nous tourner en ce que, par comparaison, elle peut jeter quelque lumière sur celle-ci.

Notes

1. Herbert Spencer: *First Principles*. Londres, 1863, p. 241.
Emile Meyerson: *Identité et Réalité*. Paris, 2^o édition, 1911, pp. 129-130 et 192-193.
2. Je ne développerai pas ici la question de l'audience de Spencer aux Etats-Unies. Elle fut considérable et contradictoire.

Elle mériterait à elle-seule une étude à part. J'indiquerai seulement que le behaviorisme n'est pas directement issue du naturalisme spencerien. Spencer a été lu aux Etats-Unis avant tout à travers William James et la refonte que celui-ci a opéré du naturalisme anglais et de l'évolutionisme de Spencer. Cette refonte a en particulier affecté les rapports entre savoir et croyance tels qu'ils étaient posés par Spencer. Elle a ajusté la conception de ces rapports à la manière dont ils étaient, pour des raisons idéologiques et politiques propres à la formation social des Etats-Unis de l'époque, posés concrètement. Cette conjoncture était en effet caractérisée à cet égard par l'absence de solution de continuité entre science et croyance (ainsi que l'a exprimé le pragmatisme, en particulier celui de James). Cette manière de poser les rapports entre science et croyance était incompatible avec l'agnosticisme du naturalisme anglais de Mill, Spencer, Lewes ou Bain. Sur ce point le lecteur pourra se reporter à mon ouvrage: *Le Corps du Délit (Etudes d'histoire politique de la Psychologie)*, Paris, P.U.F., 1984. J'ajouterais encore ici que l'une des conséquences paradoxales du souci de James de reconcilier l'expérience religieuse et la connaissance objective (exigence qui répondait à cette conjoncture que je viens d'évoquer), lui a permis d'être l'un des premiers à avoir su dégager avec précision ce qui séparait Darwin de Spencer en ce qui concerne la question de l'évolution (cf. W. James: *Principles of Psychology*. (1890, 2^e édition 1918), New York, Dover Publications, 1950, vol. II, pp. 678-688).

PSYCHOLOGY AND ITS SOCIAL LEGITIMATION*

The case of the Netherlands

P.J. van Strien

Department of Psychology

State University of Groningen

The Netherlands

Summary

By means of a content analysis of publications of Dutch psychologists it is shown that the audience on which psychologists focus their attention, the choice of the medium, and the kinds of arguments used to demonstrate the value of psychology, change in the course of time. Four stages are distinguished in the development of psychology in the Netherlands: I. A pionering phase (up to 1918) in which psychology is mainly used by non-psychologists to enlighten their fellow-men about the problems of a rapidly changing world. II. The interbellum (1914-1940), in which chairs for psychology are created at the universities and occupied by psychologists with a strong practical orientation. "Psychotechnical" application for economical and personal ends becomes the primary social legitimation of psychology. III. Establishing the profession (1945-1965), a period of rapid expansion and deep penetration into all structures of the modern welfare state which developed after the war. In addition to the external audiences of psychology (the wider academic public, society at large and the client public), an internal audience of professional colleagues with a close in-group character develops. IV. Catching up with international science (from 1965), a period in which the internal scientific communication with other members of the (international) scientific community gets the better of the communication with the three other audiences. The stricter control by government, as part of the retrenchment policy of the eighties, confronts Dutch psychologists with a new audience of bureaucrats. This may lead to a new stage in the social legitima-

* The author wishes to thank Prof. Dr. A.D. de Groot, Prof. Dr. J.Th. Snijders and Dr. Van Rappard for their helpful comments on an earlier version of this paper.

tion of psychology.

From its nascence in the early twentieth century to its reaching professional maturity in the early sixties, the segmented "pillari-
zed" structure of Dutch society along the lines of religious af-
filiation, appears to have been an important vehicle in the re-
ception of psychology and its deep penetration into all layers of
society. This might be one of the main reasons for the exception-
ally high density of psychologists in the Netherlands.

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In this study a special aspect of the social context of the
development of psychology is investigated: the way in which psycho-
logists justify their existence and the relevance of their work to
society. In order to get a clearer picture four kinds of "audien-
ces" will be discerned:

- a. the internal audience of the professional and scientific
community (colleagues).
- b. the wider "academic community" (scholars of other disciplines)
- c. the "society at large" in which the psychologist lives
- d. the specific clients who make use of the (professional) servi-
ces of psychologists (including private and governmental
sponsoring bodies).

The internal audience (a) forms the critical forum for the justifi-
cation of scientific findings. It is an essential element in the
current philosophy and sociology of science. In several studies the
present author (Van Strien, 1978, 1983a, 1983b) has broadened the
concept of the audience to include also the three other publics (b,
c and d). In this way the "*social context of justification*" of
scientific activities obtains its due place next to the scientific
context of justification, which is the cornerstone of critical
rationalism.

In his history of psychology in the Netherlands Eisenga (1978)
distinguishes three periods (see also Van Hoorn, 1976 and Van
Strien a.o., 1985):

1. The first pioneers (up to the end of the first World War)

II. The interbellum (1919 to the beginning of World War II)

III. The maturation of psychology (the period after World War II).

For our purpose the period after 1945 can be divided in two phases:

- The establishment of the profession (1945-1964)
- Catching up with international science (from 1965). The year 1965 has been chosen as a turning point, because in that year an "Association for experimental and mathematical psychology" was formed, which led to the establishment of the "Foundation for Psychonomics" in 1968. Other developments also indicate priority being given to academic science along international lines in the course of the sixties.

With the help of a longitudinal analysis of publications of Dutch psychologists it will be shown that the relationship with the different audiences has a specific pattern in each of the four periods distinguished above. At the moment psychology seems to be on the threshold of a new stage, the features of which can be dealt with only in speculative terms.

A postscript deals with the penetration of psychology in Dutch society as this has been influenced by the segmented organization of that society along the lines of religious denominations ("pillarization").

2. Documents used and method of analysis.

Our study is based on two investigations:

- A. A content-analysis of about 90 publications of psychologists directed to a broader audience, published in the Netherlands in the period up to the second World War.
- B. A more global survey of the publications in the period from 1945 up to the present time with the same categories in mind as used in investigation A. Apart from this the present author has been a personal witness of the larger part of the developments in this period. His interpretations have been checked in interviews with several "old timers" in Dutch psychology.

The available space does not permit a detailed description of

investigation A*). The documents referred to consisted of: inaugural addresses and other academic speeches, lectures for a non-scientific audience (often printed in brochures), popular books and articles, articles in journals of related professions, reports of committees and reports of institutes, and similar documents. Pertaining to the post-war developments (investigation B) we also inspected speeches made at professional meetings, statements of policy of the Dutch Psychological Association, discussions in the journal of this association, and other professional journals and documents of governmental committee on science policy with respect to psychology. In study A files were made of relevant documents with a categorization according to:

- type of publication (a categorization of the kinds of documents mentioned above)
- status of the author (psychologist, member of a related profession, other position)
- audience(s) to which the message was directed (often more than one)
- fields covered in the publication studied
- kinds of arguments used
- statements about organizational matters (the best way of organizing psychological services; responsibility of the government; the training of psychologists and so on).

The results of the content-analysis were tabulated according to the two periods concerned (I and II). In order to get a more refined picture, period II (the interbellum) was divided in two sub-periods: IIa: 1919 to 1929 and IIb 1930-1941 (during the first years of the German occupation (1940-1945) there was still a limited possibility to publish). As has been stated earlier the stages III (1945-1964) and IV (1965 up to the present time) were analysed in a more global manner (study B).

*) A detailed report (Heymans Bulletins, Psychologische Instituten R.U. Groningen, HB-83-644^a-EX) can be obtained on request.

3. *First stage: psychology as a compass in a rapidly changing world.*

The beginning of Dutch psychology was not unlike that of German psychology: on the one hand a forerunner like Donders, the physiologist-ophthamologist (1818-1889), experimenting on reaction-times at the University of Utrecht, on the other hand the philosopher Heymans (1857-1930), investigating optical illusions and psychological inhibition in his newly founded (1892) psychological laboratory at the University of Groningen, both publishing their results in international scientific journals.

Whereas at the turn of the century psychology had gained a respected place as a scientific discipline at most German universities, the Groningen laboratory remained an isolated instance in the Netherlands up to the end of the first World War. Not until 1918 does a second author, who can be considered a "real" psychologist (Roels, who studied under Michotte at Louvain), present himself in our material. The majority of the authors (10 out of 12) publishing on psychology in this phase are psychiatrists, pedagogues, theologians and jurists, exploring the perspectives opened up by the new science. Some of them, like the psychiatrist Wiersma, a colleague and friend of Heymans at the University of Groningen, were interested in fundamental problems like attention and "secondary function". Other psychiatrists turned to psychology as an antidote against a narrow somatic approach to mental disturbances.

The motivation of the majority of those engaged in psychology, however, was not primarily theoretical but practical; psychology was seen as the key to a new understanding of man at a time when the old certainties had lost their appeal. Several writers explicitly tried to give psychology a religious basis, in order to demonstrate that Christian faith and modern science were not irreconcilable. The way in which both Protestant orthodoxy and Catholicism, in their struggle for emancipation, tried to make use of psychology in order to cope with the problems of modern times, can be seen as a typically Dutch peculiarity in the development of psychology.

In this first stage the audience of Dutch psychology is thus only partly an academic one. The message is as much an enlightening as an instructing one, directed to the general public or the individual parish. So we find, in the first decades of the century, that problems of education, marriage, mental hygiene, human relations in industry, religious life, and other problems which traditional wisdom was no longer able to solve, were treated in a psychological way in (confessional) popular magazines and pedagogical journals.

In the first decade of the century Heymans' interests took a similar turn. The only new laboratory experiments he himself engaged in were his famous experiments in evidence of telepathy. His main interest became the typology of human differences ("special psychology"), to which end he made the first correlational-studies in the Netherlands based on large-scale questionnaires about personality traits, which brought him international fame.

From the beginning of his scientific career Heymans took care to report his findings more widely than only in scientific journals. Already as a graduate student he had published a Dutch version of his dissertation under Windelband at Friburg in Germany in the widely-read cultural magazine "De Gids" (1881). In the same magazine he published an article in 1896 in which he presented his new laboratory to the broader public, and made a case for psychology as an independent science in its own right. As a ripened scholar he made efforts to disseminate his philosophical views (psychological monism) and psychological insights (his typology) in broader layers of Dutch society, making use of magazines and brochures. In 1909 he sketched the forthcoming "age of psychology" in his famous rectorial address. In his view, the social problem would be solved and personal needs would be fulfilled by the growing insight into the mental possibilities and limitations of mankind which would develop with the help of psychology (see Van Strien, 1983^C). Though addressed to an academic audience, his message soon spread to a broader public. The same was the case with his "psychology of woman", which made use of the typological

dimensions he had derived from his questionnaires, and with a popular book on "special psychology" that he wrote near the end of his life.

The principal argument used for the legitimization of psychology in our sample of publications belonging to this phase, is the failure of traditional wisdom to cope with the problems of a rapidly changing society and the value of psychological insights in solving those problems. Examples of the practical use of psychology and its value for individuals and society are given, often stressing the scientific (methodological) basis of psychological advice.

4. Second stage: psychotechnics in a segmented society

Whereas Heymans and his generation sought the social legitimization of psychology primarily in the diffusion, and only secondarily in the application, of psychological knowledge, the next generation fully directed its attention to the latter. Although the Netherlands was not directly involved in the first World War, its repercussions were felt nonetheless. At the end of the war a general urge was felt for the renewal of socio-economic and cultural life. In this situation psychological methods as a key to human motives and possibilities were viewed with great expectations. Münsterberg's conception of psychotechnics as the application of psychology to practical problems was hailed as the new way of solving problems. Especially in the domains of vocational counselling and industrial psychology (Van Strien, 1966), the period after the World War became the scene of burgeoning initiatives. Educational psychology developed mainly within the domain of pedagogics, and clinical psychology within that of psychiatry.

Along with this development a new client-public presented itself, asking advice and treatment from those self-styled psychologists, with an outspoken preference for psychologists of their own colour and denomination. Catholic and Protestant bureaus for vocational guidance and 'Pedological Institutes' were founded next to "neutral" ones. The client-public, still latent in the readership of the publications of the first stage, became the predominant

audience in the second half of the second stage (the thirties). The general public, up to the twenties the favoured audience, now had to content itself with the second place. Policy makers, still an absent public in the first stage, became an important audience in the second stage. Obviously psychologists started courting their patronage to procure a firm foothold for their profession in society.

This new wave is clearly documented in printed lectures, brochures, articles in popular magazines and so on. The richly variegated confessional-political corporate life of Dutch society of those days played an important role as an eager medium for the message of psychology.

We should, however, keep in mind that most bringers of the new message hardly possessed a formal training as psychologists. Just as in the first period, in the twenties adjoining fields provided most of the protagonists of psychotechnics: psychiatrists, physicians, educationalists, priests or Protestant theologians. Actually we find an interaction between psychology and society, in which a handful of (partly self-appointed) psychologists created an audience, and this audience in its turn created chairs for psychology at the universities and stimulated people to enter the new field. In the thirties the "real" psychologists began to outnumber the others. Of the related sciences psychiatrists and physicians stayed longest in the field.

The influence of the confessional forces is clearly visible in the way in which the new chairs are filled. The Netherlands cannot boast of a great Wundt-disciple. When Heymans (1928) resigned, his chair was taken by his most promising pupil, Brugmans. Apart from Groningen, the University of Amsterdam was the only one to appoint (1933) a non-confessional: Révész, a Hungarian refugee who had studied under Müller at Göttingen and who had since 1922 lectured as a 'privaat-docent' on industrial psychology. The other chairs in psychology were taken by men with a confessional signature, who often professed the new science with their own specific audience in mind: the Catholic Roels (1918, Utrecht); Waterink (1929, Free

University of Amsterdam), a Calvinist theologian who had studied under Külpe at Bonn; and the Catholic Rutten (1931, Nijmegen), a disciple of Roels. A substantial part of their written output consisted not of scientific publications, but of popular books and articles featuring the many uses of psychology. The same holds for their colleagues in related sciences who ventured into psychology. Thus, with a few exceptions, psycho-technical application in the service of the moral norms of a segmented society became the main context of justification during the period of the interbellum (see Abma 1979, 1981, 1982, 1983, Sanders and Eisenga, 1978).

The arguments used in the ca. 70 documents belonging to this period analysed, as compared with the previous stage, show an interesting shift. The failure of traditional wisdom to cope with the problems of a rapidly changing society - the argument in the first stage - drops to second place in the twenties and to third place in the thirties. The value of psychological insights and their practical utility remain strong arguments. The importance of catching up with international developments is stressed most emphatically in the period after the First World War but in the thirties it is rarely cited anymore.

A conspicuous trend is the increasing emphasis on expertise, methodology (in the design and use of tests) and, especially in the second half of the second phase, statistical proofs of success ("verification"). This points to a growing self-confidence which forms the prelude to the establishment of a professional organization, claiming the monopoly on the services their members are able to provide. Explicit arguments for a professional organization, however, are still hardly given. Apparently they are reserved for private communications between noted professionals. Practically the only organizational problems with which psychologists confront their public are the pros and cons of a central organization and the responsibility of the government to provide consulting services. This is a topic which is typical of the years following the First World War, the period in which most bureaus for vocational guidance are opened. Statements on the way the study of psychology

ought to be organized at the universities, or on psychology as part of the curriculum of a related profession, are mainly made in inaugural addresses of newly-appointed university professors.

5. Third stage: establishing the profession (1945-1964)

The third stage has a prelude in the last years prior to the Second World War. We already noted the growing professional self-confidence manifesting itself in the publications of the second period. In 1938 about a dozen of the pioneers of applied psychology met at Amsterdam and founded the Netherlands Institute for Practising Psychology (NIPP). Because of the war, however, it was soon doomed to a slumbering existence. For this reason the turning point of the establishment of the profession can be located best in the new period of expansion and rebuilding of society which followed the Second World War.

After the war the membership of the NIPP grew rapidly. The training of psychologists at the universities had already been legally regulated during the German occupation, and a growing number of students was now attracted to the new field. The professors of psychology had success in convincing their colleagues and the Board of Trustees of the importance of their subject. Before the end of the forties most universities had a second chair in psychology, and other appointments followed in the course of the fifties. In 1960 each of the six universities had about five chairs in psychology. The number of psychologists increased from roughly 100 in 1950 to roughly 600 in 1960.

In 1950 personnel selection, vocational counselling and "child-psychology" still were the main fields of application. In the course of the sixties these fields were surpassed by clinical psychology, first contributing to the assessment of psychiatric patients, but gradually also penetrating other fields and acquiring a growing share in the "market" of psychotherapy.

One of the first initiatives of the new professional organization was to take steps to get the title of "psychologist" and the professional activities of psychologists legally protected, and to prepare an ethical code (after a number of revisions put into

operation in 1961). At the same time a Disciplinary Board was installed to deal with members trespassing against the code. The main purpose of the code was to guarantee the quality of the services of psychologists and to inspire confidence in the integrity of the profession.

In the first place we find these strivings for professionalization documented in the internal Communication Bulletins of the NIPP and similar documents. Psychologists are also found active in several government- and business-installed committees, trying to control what they considered to be their share in the rapidly expanding public and private services of the modern welfare-state. Confidence in the profession was promoted further by the publication of newly-written or translated popular books on psychological questions, with, however, the typical taboo on the disclosure of test-secrets that was characteristic (also in other countries) of professionalizing psychology. A psychologist who had translated a popular German book on tests which contained a picture of one of the Rorschach-cards was deprived of his membership of the NIPP!

To better promote the interests of psychologists, the NIPP established its own bureau with a professional director. New committees were set up in the same period for dealing with the internal and external affairs of the profession.

In professional practice confessional demarcations still played a role in the "pillarized" organization of a number of professional services: vocational counselling, educational and clinical guidance, martial counselling and so on. Specific interests, however, were brought together under the common roof of federative structures. In this way Dutch psychologists have been very successful in their penetration of all regions of the modern welfare society, especially after the clinics also opened their gates to psychologists, and their clinical services and psychotherapy were subsidized by the state and by medical insurance on the same basis as medical services.

In academic psychology the participation of philosophers and scholars of other professions - typical of the pre-war period - was still notable. They contributed regularly to the leading psycholo-

gical journal. Among the editors there were two psychiatrists, of whom one was a founder of the journal. Psychologists for their part published frequently in philosophical journals and journals of adjoining professions. Here again the influence of confession made itself felt. Within the wider academic community Catholic and (Calvinist) Protestant networks existed encompassing a broad spectrum of academic fields. In the framework of these denominational intellectual networks, quite a few contributed to discussions about current ideological and societal questions. Even theoretical and methodological positions appear to be influenced by religious affiliations (see Van Strien, 1983^a). In the "existential-psychological school", which had a great influence in Dutch intellectual life during the fifties and early sixties, psychologists of a confessional colour held leading positions, the (converted) Catholic psychologist Buytendijk at Utrecht being the principal figure. In addition to this academic audience, psychologists belonging to or inspired by this group also directed themselves to the larger public with messages in the same "pastoral" style as was common during the first two stages.

6. *Fourth stage: from profession to science.*

The rapid expansion of psychology after the Second World War had a side-effect. In order to deal with the growing number of psychology students at the universities (about 2000 in 1963), the staffs of the Psychology Departments had to expand rapidly as well. The number of chaired professorates and lectorates also increased further. Each chair was augmented by a number of academic positions filled by (mostly just graduated) young psychologists. Whereas the number of psychologists employed by Dutch universities was 94 in 1963 this number had risen to 595 in 1972 (Krijnen, 1975). The majority of the staff did not identify itself with the problems of practice, but were heading for an academic career. Scientific research, which had been lagging behind for many decades, was again brought to an international level.

In the course of the sixties the intellectual climate also changed. The orientation towards German-Swiss characterology and German-French "phenomenological-existential" psychology was superseded by a strong orientation towards Anglo-Saxon "empirical-analytical" science. The Utrecht School dissolved itself soon after the "apostasy" of Buytendijk's successor Linschoten in the early sixties. The publication of De Groot's "Methodology" in 1961 was another landmark of the new orientation. In methodological practice holistic assessment-techniques were supplemented or even totally replaced by programs along psychometric lines (first in military psychology, gradually also in other settings).

The new spirit expressed itself in a number of institutional facts. A section for Research was established within the NIPP in the early sixties. We already mentioned the "Association for experimental and mathematical psychology", founded in 1965, which gave birth to the "Foundation for Psychonomics" in 1968. In order to persuade the new scientific psychologists to become, or to remain a member of the professional organisation (NIPP), the former News Bulletin of the organisation was - following the example of the American Psychologist - styled into a professional journal with scientific articles of general interest (1966), and the name was, by dropping the P of "practising", changed into NIP (1968). So the practising psychologists who until the middle of the sixties had formed the vanguard, were forced into the defence.

The Netherlands Journal of Psychology gradually lost its function as a platform for discussions within a wider academic community and took on the character of a specialist scientific journal, intended to inform the Dutch scientific community about the development of psychological research.

The new spirit can also be noted in the presentation of psychology to its external audience. Psychology tried to rid itself of the image of a "soft" speculative subject and to present itself as a real science based on hard experimental and statistic techniques. The moralistic tone which characterized many of the popular books and articles of psychologists of the previous genera-

tion disappeared and was replaced by a matter-of-fact style in dealing with the psychological aspects of practical problems. The taboo on breaking professional solidarity lost its vigour, and the methodological and ethical quandaries of personnel selection and other professional services were discussed by psychologists (mainly from the universities) in public speeches, newspapers and magazines. Proposals in meetings of the NIP to take measures against these "dissidents" were placated.

The primary audience of psychologists however became the internal audience of their own scientific community. The social legitimization of the growing volume and scope of scientific research at the universities and at some newly founded research institutes was found in the adage, 'Give us the chance to do free research and the fruits for society will ripen in due time'. In the following years new circuits for sponsoring applied research were founded. The volume of acquired grants became a direct measure for the social function of psychology as a full-fledged science.

7. The imminence of a new stage? Government as a new audience.

During the second half of the seventies the cultural and political climate in the Netherlands, which up to that moment had been favourable for the social sciences, began to change. In the eighties these changes are making themselves more sharply felt. In the Netherlands, just as in other countries, the value of the welfare state is being questioned, and along with it the whole "soft" non-profit sector of which psychology was considered to be a part. Columnists in newspapers and magazines questioned whether the social sciences can produce anything that surpasses the truisms of common sense. Whereas a de-regulation of free enterprise is being advocated, research-expenditures at the universities and other public institutions are subjected to a much narrower regulation. The Ministry of Education has commissioned reports to be written on the state of the social sciences. Because of drastic reductions in government expenditures, new places for psychologists can't be created, and vacant places cannot even be filled. In 1983 more than

one thousand psychologists were unemployed in the Netherlands, and their number will probably grow to several thousands before the end of the decade.

Developments like these put the social legitimization of psychology - fundamental and applied - again to the test. It is still too early to get a full picture of the resulting shifts in the audience to which psychologists direct themselves and of the arguments used. Newspapers and magazines appear to be used once again as channels to serve the case of psychology. In 1982 a new popular magazine "Psychologie" was founded, following the model of the American magazine "Psychology Today". There is much lobbying going on around governmental departments and councils to win the favour of officials for certain types of research. The rules of this game have still to be defined.

8. Discussion: the psychologist and his audience.

Our survey of publication trends in the history of psychology in the Netherlands shows that the attention given to the different audiences changes with the entry of each new phase.

In the phase of nascence, the wider academic community and society at large had the character of a critical environment, which still had to be convinced of the value of the psychologists' ideas and work: sceptical colleagues and a society which did not (yet) understand. This required all kinds of publicity, emphasizing the possibilities of the new science. In addition to this we also find quite a few of those in related professions making use of psychological findings to renew and strengthen their intellectual leadership in their segment of society. As we have seen, the dividing lines of religious denominations ("pillarization") played an important role in the formation of this kind of intellectual elite, which served as an intermediary in the reception of new scientific insights (see Lijphart, 1968, Ellemers, 1984).

In the second phase the academic audience and the wider society remain important. In addition to this a new public presents itself: the client-public as that part of the general public which

is already interested in the professional services of the new science. The kind and volume of the services in which clients are interested have a steering influence on the way in which a new science develops, on the place it receives in the universities, and on the professionalization of its practice. The scientist for his part must satisfy his clients, and apart from that, has to convince them of the surplus value of his contribution over the traditional ways of solving the problems in question.

In the third phase of professional expansion all three external audiences are served with publications which hope to maintain their favour and to win that of other potential clients and sponsors. In addition, an internal audience begins to take shape, but in this stage more in the form of a closed professional community than in that of an open scientific community.

Only in the fourth phase does the internal audience acquire the character of a primarily scientific community, as a part of the broader international scientific network of psychologists. The range of attention within the wider academic community narrows at the same time to specific inter-disciplinary relationships with researchers who approach the problems studies from a supplementary angle. The public-at-large is not seen any more as a flock of laymen, who have to be guided by professional wisdom and advice, but as taxpayers who have to be convinced that their money is well-spent. Where the client is concerned factual information takes the place of the former magic of tests and professional charisma.

In the last few years it has become clear that psychology, just as other social sciences, must learn to cope with growing criticism of all soft aspects of the welfare state, and with the increasing interference of government bureaucrats with science policy. The growing importance of this new wayward audience may lead to a new stage in the social legitimation of psychology.

The relationship of a science with its different audiences thus appears to be a fruitful approach to the study of the interplay of internal and external influences on its development. To

this end the analysis of scientific publications intended for the scientific community - the principal source of the usual history of a science - has to be supplemented with publications and communications addressed to the other audiences. It is more difficult to get hold of them, because they are not usually filed in libraries, in a manner convenient to the purpose, but taking the trouble to browse among the shelves of libraries and archives certainly pays off.

It would be interesting to compare the influence of the respective audiences in the Netherlands with that in other countries. In at least one respect the Netherlands appear to be a special case, namely in the strong influence of the confessional "pillars" possessing their own intellectual elites and corresponding audiences. In a postscript we will discuss the significance this factor may have had for the amazing growth of psychology in the Netherlands.

Postscript

At present over 8000 psychologists are employed in the Netherlands (which means a density of more than 1:2000, a density which to our knowledge is only surpassed in the USA). Though psychology started late in the Netherlands it had a more successful development there than in many other countries where it began earlier. Germany of course had its own fate, but other countries, such as England, Belgium, France, Austria, Switzerland also had a slower growth (viz. Rosenzweig, 1982).

That practice soon took the lead over theory can be ascribed to the practical what-can-I-buy-for-it mentality of the Dutch public in general, a factor which is exemplary for the orientation of Dutch science in general which, with a few exceptions such as theoretical physics and mathematics, is more practically than theoretically oriented. This, however, does not explain why psychology as a whole penetrated deeper into Dutch society than was the case in most other Western countries.

The main reason may perhaps be found in the success the pioneers of Dutch psychology had in convincing their public of the social relevance of their work. This success can only partly be

ascribed to their eloquence and persuasiveness. A more important reason appears to be the segmented social structure of Dutch society as mentioned earlier. This very segmentation provided for a gamut of intellectual and spiritual opinion leaders who, in line with the two-step theory of innovation (see Katz, 1979) paved the way for the penetration of the new profession in broad layers of society. In the fourth phase, in which secularization and "depillarization" set in, equity in the subsidizing of all "colours" of the cultural spectrum of a pluriform society remained important as a bureaucratic principle. This principle is being questioned now within the neo-liberal mentality which seems to mark the cultural and political climate of the eighties. In the meantime psychological research and practice have, however, obtained such a firm footing in Dutch society that they surely will maintain a strong position compared to other countries in which the same climate makes itself felt.

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THREE CONCEPTUALIZATIONS OF HUMAN SOCIAL VIOLENCE
(1900-1980): A DISCUSSION OF THEIR SOCIO-HISTORICAL
SPECIFICITY AND TRANS-HISTORICAL COMPARABILITY

Ian Lubek⁽¹⁾

Psychology Department,
University of Guelph

Summary

This paper briefly compares three divergent formulations concerning human social violence which have become available to social scientists since the turn of the century. The first perspective is George Sorel's (1908) neo-marxist view of apocalyptic violence; the second, the frustration-aggression hypothesis (Dollard, Doob, Miller, Mowrer and Sears, 1939) and the third is Berkowitz's (1962) social-psychological formulation.

It is argued that the three theories are radically different on a number of dimensions. Sorel's analysis concentrated on a social and societal level of causality, and was permeated with revolutionary values. Dollard c.s. accepted a psychological level of explanation for violence, yet the political realities of the day were not left out of consideration. By the 1960's, much of the impact of real-world socio-political events no longer seemed to permeate the laboratory walls and affect the questions raised.

It is concluded that while the social-political content has been largely eliminated, the scientific credentials of research on aggression have been greatly strengthened.

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This paper briefly sketches three diverse formulations concerning human social violence which have become available to social scientists since the turn of the century. Although each of the

(1) The present paper was prepared with the aid of the SSHRC-CNRS programme of scientific exchanges while the author was visiting the Laboratoire de Psychologie Sociale, Groupe de Recherche au C.N.R.S. associé à l'Université de Paris VII. Mailing address: Psychology Department, University of Guelph, Guelph, Ontario, N1G 2W1, Canada.

selected perspectives was a product of a unique socio-(cultural)-historical milieu, they all shared the common goal of wanting to explain systematically and *scientifically*, the social phenomena of human aggression. The first perspective, George Sorel's (1908) neo-marxist view of apocalyptic violence, was posited against a background of social and political unrest in Third Republic France. The second, the frustration-aggression hypothesis, was developed in the midst of the economic depression by Dollard, Doob, Miller, Mowrer and Sears (1939) from within the academic haven of Yale University's inter-disciplinary Institute for Human Relations. The third formulation involved the revitalization of the second as a "neo-frustration-aggression" hypothesis, and its transfer, by Berkowitz (1962) and others during the 1960s to the domain of experimental laboratory social psychology; this occurred during the era of that discipline's dramatic expansion, and against a background of increasing social discontent, protest movements, and urban unrest.

Previous historical work, viewed from the perspective of a "social psychology of science", had focussed upon problems concerning methodological consensus within a research area such as the social psychological study of aggression (Lubek, 1979) or professional/academic contextual problems involving the dissemination of scientific ideas (Lubek & Apfelbaum, 1979; Lubek, 1981; Lubek & Apfelbaum, 1982; Apfelbaum & Lubek, 1982) and the particular roles of dyadic or small-group decision-making, social influence, and/or "gate-keeping". Such concerns with the continuity of a set of ideas, are juxtaposed in this paper, with the broader examination of each of the (arbitrarily) selected "violence" formulations as they arose within, and interacted with, a specific historical context of socio-political events and movements. A critical evaluation of the specificity and limitations of older formulations is often easier than an objective critique of an on-going scientific research enterprise.

It is hoped that the comparisons (and contrasts) among these three selected frameworks (chosen from three distinct stages in the development of the social sciences), especially as to their shared assumptions about violence - how to study it scientifically and how

to evaluate it - will help shed light on certain limitations of the approach currently taken by social psychologists. It will be suggested that each formulation took advantage of the methodological tools available to social scientists in that respective period: the pioneering era (pre-World War I); the period of broadening interests and scientific aspirations (pre-World War II); and the scientific boom and the age of technical specialization (post World War II). However, as the social sciences in general (and social psychology, more particularly) "progressed" towards their more modern "scientific" manifestations, the links to real-world political problems involving human social violence have become increasingly eroded. One result is that for modern social psychologists, the neo-frustration-aggression hypothesis has become a dominant formulation which, in offering a mechanistic - almost technocratic - "person-blame" approach to violence, manages to ask only a narrow range of questions about the social phenomenon it purports to study scientifically, and therefore, understand. Elsewhere I have argued in greater detail that the 3 theories are radically different on a number of dimensions, as shown in Fig. 1. (Lubek, 1983; Apfelbaum and Lubek, 1983; Lubek, 1979; Lubek, 1984, in press). Some of these dimensions will be referred to when Sorel's (1908) formulation is contrasted with the two "frustration-aggression" approaches.

Let us first consider the *Reflections of Violence* (1908; English translation, 1914) which Georges Sorel began writing in 1905. At the time of the formulation of this perspective on social violence, France's Third Republic struggled with a series of political and social upheavals in the wake of the Dreyffus affair (1894-1906), which necessitated reconstruction of many social institutions (school, military, Church-State relations, etc.). Socialists had entered government for the first time with Cabinet decision-making positions, and a variety of trade-union movements flexed their collective muscles in attempting to ameliorate conditions for the workingman; about 9 1/2 million workers participated in strikes during 1906. At this time, Sorel served as a

theoretician for a group of "revolutionary syndicalists", a movement advocating use of the General Strike as the means of producing a social revolution to return ownership of the "fruits

Fig. 1 Some dimensions for differentiating theoretical formulations of violence

Causality:	individual	social
	person-blame	system-blame
State:	status quo.....	dynamic
	equilibrium	conflict
Evaluation:	dysfunctional.....	functional
	anti-social	pro-social
Praxis:	decrease.....	increase
	inhibit	stimulate
	control	promote
Timeframe:	instantaneous.....	longitudinal,
	ahistorical	historically
		socialized
Source of questions:	scientific.....	realpolitik
	theory,	
	laboratory	

Selected theoretical	Dollard et al (1939)	Sorel (1908)
formulations:	Berkowitz (1962)	Fanon (1968)
	Feshbach (1971)	Sartre (1968)

of labour" to the workingmen themselves. Sorel's neo-marxist, dynamic view of pre-determined class conflict between the proletariat and capitalists saw violence as an inevitable result of deep and historically long-standing class divisions. At the time he was writing his *Reflections*, Sorel observed almost daily that the state and the factory owners were able to use force (the police, army) to suppress worker's strikes, meetings and demonstrations - often with violent consequences. Therefore, Sorel countered, it was totally justified - indeed, to be wished - that the proletariat reply with violence to upset the unjust *status quo*, whereby they were exploited and victimized. The ultimate weapon, to overturn society, was the General Strike - the mythical values of which Sorel also saw. This would signal the moment for the unleashing of apocalyptic violence in the service of the majority of workers. Sorel's value choices indicated that violence was *functional* (in the service of oppressed groups) and to be encouraged and *increased*. He embraced a *conflict* position (Coser, 1956) which served simultaneously as a means of analyzing the sources of human violence, and as a heuristic for praxis in the struggles of the workingman. (Some writers consequently labelled Sorel an apologist for violence). His method, a kind of imperfect functional analysis which he called "diremptions", would today be branded "journalistic", although the systematic attempt to buffer hypotheses with examples, logical arguments, and everyday observations was quite common in the social sciences at the turn of the century.

At the time of Sorel's writing, there was no clearly-delimited discipline of social psychology. Although versed in the individualistic psychological formulations of Ribot and Tarde (he would later show preference for Bergson and the "pragmatic" W. James, not the instinctual) Sorel ignored reverting to models of individual pathology or biological destiny (James, 1890; McDougall, 1908; etc.) to explain social violence, nor did he join sociological writers in America (e.g. Ellwood, Small) who sought psychological bases for social behaviours. At the same time, he proved a harsh

critic of Durkheimian and other academic sociologists in Europe - he found no need to subscribe to such positivistic philosophy or method (e.g., Durkheim, 1895). (Parenthetically, it is interesting to note that he had been one of the very few social commentators to review - not altogether favorably - the social psychological work of Hamon, 1895, who took a system-blame, neo-marxist position on social behaviour. C.f. Lubek & Apfelbaum, 1982; Apfelbaum & Lubek, 1982). Sorel's ideas had little carry-over into psychology and social psychology (although there was a rediscovery of his work in sociology and political science). Only 2 social psychology textbooks, to my knowledge cite Sorel (Katz and Schanck, 1938; Young, 1953); between 1966-1980 there was only one citation of Sorel's theory (Feshbach, 1971) in an American Psychological Association journal, and 4 additional citations in journals that might be read by (social) psychologists. Sorel's choice of a society-blame analysis of violence, whose increase was positively evaluated, resonated only with certain writers about liberations struggles and their critics (Fanon, 1968; Sartre, 1968; Arendt, 1972), and not with modern social psychologists. The formulation shows strong evidence of the intrusion of the *realpolitik* of violence into Sorel's analysis of conflict and violence. Today it would more likely be labelled as a "political polemic" rather than as a "scientific" approach to violence.

Three decades later, in the height of the Depression, an inter-disciplinary formulation linking frustration to aggression appeared in the Yale University seminar of the behaviorist psychologist, Clark Hull (cf. Lubek, 1984, in press). The possibility to question the five major authors of this perspective on social violence (Dollard, Doob, Miller, Mowrer & Sears, 1939) and examine the minutes of the seminars in which the ideas took shape, added an additional historiographical dimension to the standard examination of primary and secondary sources, e.g. as used to analyse Sorel's theory. Although the work is primarily recalled in psychology as a rigorous, scientific hypothetico-deductive generator of testable behavioristic propositions about human aggression (especially the

first three chapters), a complete re-reading of the monograph shows that its inter-disciplinary outreach sought to explain violence in a number of real-world problem areas involving inter-personal and collective violence. The authors were not untouched by the surrounding political and social events. One important example was the violence towards blacks in the Southern U.S.; see Dollard, 1937, for an early discussion of the frustration-aggression relation. As well the confrontation between fascism, communism and democracy looming in Europe, and already manifest the Spanish Civil War, was discussed in chapter 7 of the book. Hull and the seminar participants, despite their inter-disciplinary backgrounds and their enthusiasm for trying to bring Freudian concepts under scrutiny of behavioristic psychology, focussed their efforts on *scientific-theoretical* concerns, and, in particular, on the *individual* and his/her reactions to frustrations.

Here, against a background of social and political events, the authors chose to develop from the psychological point of view, a theoretical approach to violence which nonetheless could be extended to analyses of group and international violence, etc. However, in strong contrast to the Sorelian approach, the values implicit in this monograph indicate that violence is socially *dysfunctional*, and ought to be controlled, inhibited or *reduced*. The social awareness and eclectic range of inter-disciplinary discussions indicate the permeation of the real-world social and political problems of the day. Compared to the Sorelian position, this work absorbed the trend in the social sciences of the 1930's towards a more scientific approach (with physics as the role-model), maintained an interest in real-world violence, chose the psychological level of analysis, and adopted generally liberal values towards the "anti-social" phenomena of violence.

Less than three decades after the publication of the frustration-aggression monograph, there was a dramatic increase in the amount of research conducted by social psychologists on the parameters of human aggression (Lubek, 1979; Apfelbaum & Lubek, 1983). A leading figure in this renaissance of research was a University

of Wisconsin social psychologist, Leonard Berkowitz. It was possible to again combine the textual analyses and interview techniques used previously, along with additional interviews of other researchers, former fellow graduate student and teachers, in order to place Berkowitz's formulation of aggression within the context of the growth of social psychology as an increasingly scientific discipline after World War II. Berkowitz was part of a cohort of post-war social psychologists, whose work was increasingly confined to, and perfected in, the laboratory during the 1960's and 1970's. The intrusions of concurrent real-world events (riots, protests, racism, violence against women & children, Vietnam War-related violence) are rarely detectable in these studies which carefully and rigorously explore laboratory - controlled variables determining the amount of shock-giving behaviour by college students who have often seen a short violent film and been provoked by a confederate of the experimenter. As with the earlier frustration-aggression formulation, the Berkowitz neo-frustration-aggression perspective puts emphasis on the psychological level of explanation of violence, and shares the liberal values of the earlier Yale group, again in sharp contrast to the framework of Sorel. But the major contribution of Berkowitz and his students over the last 25 years has been the refinement of theory and laboratory methods - i.e. while the social-political content has been largely eliminated, the scientific credentials of research on aggression have been greatly strengthened.

We have seen that Sorel's analysis had its genesis in the socio-political events of the epoch, concentrated on social (and societal) level causality, was permeated with revolutionary (or non-liberal) values, and was not what we would today consider as scientific. The frustration-aggression hypothesis evolved at a time when psychology had aspirations of becoming increasingly scientific. In reflecting this trend, and in accepting a psychological-level explanation for violence, there was not as yet a wholesale abandonment of consideration of the social and political realities of the day and how they were related to violence. By the 1960's, as

the neo-frustration-aggression formulation (Berkowitz 1962, 1969) began to dominate the study of aggression by social psychologists, the scientific rigour of laboratory experimentation had been greatly improved. The formulation, although adopted by social psychologists, still remained confined to the individual, psychological level. But now, with liberal values maintained, much of the impact of real-world socio-political events no longer seemed to permeate the laboratory walls and affect the questions raised - deductions from theory held a monopoly here.

Of the three formulations, only Sorel's demonstrated "non-liberal" views about violence (to be used by an oppressed group for social change purposes). In contrast, the two more recent approaches show a preference (shared by many social scientists) for more liberal values vis a vis "anti-social" or "violent" behavior. When combined with a preference for "person-blame" formulations which seek social "causes" at the psychological level, which are technically operationalizable according to rigorous scientific (and mainly laboratory) methodology, and are generally politically-neutralized, a number of potential questions about violence phenomena consequently may become neglected: e.g., violence in various exploitative systems (e.g., imperialism and colonialism as discussed by Fanon, 1968; or industrial conflict); institutionalized violence such as torture, massacres, genocides; racist and/or sexist attacks, both psychological and physical (e.g., rape, harassment, battery, and the long-term effect of institutionalized racism and sexism across generations). Because of self-imposed limits chosen by modern researchers, a narrowed range of questions concerning human social violence will thus be offered to readers of the increasingly specialized social science journals, while the phenomena of violence as they occur in everyday life and touch humans directly (especially away from the shelter of university campuses where research is pursued) are not adequately explained, are not well-understood, and are often simply not studied. The comparative historical perspective is one way of drawing critical attention to the limiting choices placed on the study of violence

by an evolving scientific discipline such as modern social psychology.

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DE QUELQUES REPÈRES THÉORIQUES POUR UNE
APPROCHE DE LA QUESTION DU GRAND HOMME

Michel Plon,

Maître de Recherche au C.N.R.S.

Paris

Summary

This paper deals with the question whether the 'great man' in history and politics could be a topic of research.

If answered positively, a more precise definition of the framework for this approach would be required. Some possible errors are pointed out and tentative directions for research are proposed.

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La question de l'existence réelle, passée, présente ou à venir, de *grands hommes* dont il y aurait lieu de considérer qu'ils ont eu, ont ou auront, un rôle dans l'histoire, cette question, en appelle une autre, prioritaire, consistant à définir qui, et au moyen de quel corps de connaissances, est susceptible d'apporter des éléments de réponse à ses questions de l'existence et du rôle du grand homme.

En d'autres termes, si l'on veut pouvoir attribuer un crédit à une réponse, négative ou positive, apportée aux questions de l'existence et de la fonction du grand homme, il convient d'avoir un minimum de garantie concernant la capacité, chez ceux qui envisagent d'apporter des réponses, d'évaluer la validité théorique des questions.

I. L'approche philosophique

Peut-on considérer que la philosophie dans les multiples phases de son histoire et les philosophes dans leur diversité apportent ou ont apporté, s'agissant du phénomène du grand homme, des informations décisives?

A défaut de pouvoir, ici, se prononcer de manière définitive sur ce point, on doit constater que la philosophie et les philosophes ont fortement structuré le champ du phénomène et cela d'une

manière devenue aujourd'hui tellement évidente que l'on en vient à oublier, le plus souvent, d'en repérer les coordonnées.

Pour évaluer, même succinctement, l'apport de la philosophie sur ce point, on peut schématiquement distinguer deux périodes dans l'histoire de la pensée philosophique, celle précédant Marx et celle qui, lui faisant suite, ne pourra éviter de se situer, positivement ou négativement, par rapport à l'approche marxiste de la question.

Avant Marx, la question du grand homme est diversement abordée, parfois ignorée mais jamais véritablement contestée comme question. Sans viser ici à une rétrospective exhaustive, on se contentera d'évoquer Machiavel et son appel à un sauveur pour défendre l'Italie dans l'ultime chapitre du *Prince*, Montesquieu, qui liait l'ascension du grand homme au degré d'adversité qu'il rencontrait et à l'intensité des revers qu'il lui était donné de connaître, Hegel enfin, qui considérait le grand homme, l'Esprit, comme le révélateur d'une situation historique donnée, le porteur de la volonté intime, caché, d'un peuple.

Avec Marx et avec la constellation marxiste, l'abord de la question se trouve considérablement modifié; il ne s'agit plus de spécifier la place et la fonction du grand homme dans la processus historique et politique, d'en hater ou d'en freiner, illusoirement peut-être, la venue, mais d'en contester, sinon l'existence, du moins l'importance. Cette mise en cause s'effectue tant implicitement, découlant des thèses essentielles de la conception marxiste de l'histoire, qu'explicitement, lorsque, au hasard de lectures ou de correspondances, Marx, Engels, abordent le thème lui-même. La mise en cause implicite vient de l'impossible coexistence d'une conception considérant le grand homme comme un facteur de l'évolution historique avec une autre, la conception que développent Marx et Engels, qui fait des masses et de la lutte des classes les facteurs déterminants et exclusifs de l'évolution historique. Pour ce qui est de la récusation explicite du rôle et de l'éventuelle fonction du grand homme dans l'histoire, on la trouve aussi bien sous la plume de Marx qui, dans une lettre à

Kugelmann, parle du "hasard" du caractère des chefs, que sous celle de Engels qui, dans une lettre à H. Starkenburg, évoque Napoléon comme une nécessité annexe et remplaçable par n'importe quel autre individualité, ou, que sous celles, conjointes, des deux auteurs lorsqu'ils commentent l'ouvrage de Th. Carlyle, *Les héros et le culte des héros* pour en dire, la chose vaut d'être relevée tant elle acquiert de résonnance aujourd'hui, que le culte risque de prendre le pas sur les héros.

Quel que soit le jugement que l'on porte sur l'oeuvre de Marx et sur le marxisme en général, et ça n'est pas ici le lieu de discuter de ce point, on ne peut négliger l'une des conséquences essentielle de ce tournant philosophique quant à la question du grand homme. Conséquence double en cela que désormais, toute démarche prenant en considération la question du grand homme sera d'une part considérée comme "psychologiste" puisque reposant sur l'idée, implicite ou explicite, que le psychisme, la psychologie des individus ne seraient pas entièrement déterminés par les instances économiques, politiques et idéologiques mais pourraient au contraire participer de la constitution et de l'évolution de ces instances et d'autre part taxée, du fait même qu'elle n'épouse pas les principes essentiels du marxisme, d'être idéologiquement anti-marxiste et donc politiquement de droite.

L'effet ainsi produit est, si l'on peut dire, une réussite, puisqu'à la suite du marxisme, il n'est pratiquement aucune philosophie, pour autant que le marxisme en soit une ce qui est encore une autre question, quelle que soit sa position à l'égard du marxisme, qui développera une réflexion originale sur le thème du grand homme.

Quant à l'étiquetage politique, même si on lui oppose des exceptions non négligeables, celle de Trotsky d'un côté, qui évoque le rôle irremplaçable et décisif de Lénine dans la révolution d'octobre, ou celle, à l'inverse, de Tocqueville, considérant que seuls les groupes et les classes sociales ont de l'importance dans le déroulement de l'histoire, il va demeurer très important, allant jusqu'à structurer l'abord de la question dans le champ des sciences sociales qui vont prendre le relai de la philosophie.

II. Les sciences sociales et la question du grand homme

La double partition, théorique et politique, à l'instant évoquée, s'inscrit d'abord dans le champ de connaissance "a priori" concerné, celui de l'histoire.

On peut le cerner au travers de la séparation qui existe entre les historiens du *collectif*, des *mentalités* des *pratiques* et des *institutions*, bref les historiens qui considèrent l'histoire comme étant avant tout sociale et les historiens qui, n'adoptant pas totalement cette perspective, font une histoire généralement taxée par les premiers, d'événementielle, voire d'anecdotique et considérée comme porteuse, à son insu dans le meilleur des cas, de traits idéologiques antinomiques de l'idée d'histoire. Il ne saurait ici être question de s'inscrire dans ce débat qui est avant tout celui des historiens et encore moins de prétendre donner raison aux uns contre les autres ou inversement. Du point de vue que est ici le notre, il est tout à fait certain que cette seconde catégorie d'historiens représentante d'une histoire distincte, dans ses thèses fondamentales, de celle développée par ce qu'il est désormais convenu d'appeler l'Ecole des Annales même s'il y a dans cette appellation un schématisme un peu trop marqué, représentante d'une histoire largement développée aux Etats-Unis notamment, que cette seconde option, donc, fait une large place aux hommes en général et aux grands hommes en particulier. Pour autant, cette attestation ou cette reconnaissance du phénomène du grand homme n'est pas totalement satisfaisante dans la mesure où la catégorie, le concept ou l'idée de grand homme y fonctionnent comme une évidence, un donné échappant à toute question, toute interrogation, à propos desquels aucune théorisation n'est proposée.

De la sociologie, on serait tenté de dire que par essence même elle échappe à ce questionnement s'il ne convenait, dans le cadre de ce très bref survol, de faire une place à part à Max Weber dans le système duquel le grand homme constitue un thème à part entière, puisqu'il représente, selon le grand sociologue allemand, une forme de légitimité politique. S'il fallait trouver une raison au peu de retentissement qu'a connu cette partie de l'oeuvre de M.

Weber, peut-être faudrait-il la chercher du côté justement des explications qu'il donne du phénomène du grand homme, lesquelles s'organisent à peu près uniquement autour de la notion de *charisme*, notion qui, dans ce qu'elle comporte d'irréductible, dans sa fonction de point de butée, n'évite pas l'écueil du psychologisme.

On aurait pu penser que le lieu par excellence de l'étude du phénomène du grand homme serait celui constitué par la psychologie sociale. Mais si l'on met de côté les recherches, sans grande portée explicative, menées par cette discipline sur le thème du *leadership*, force est de constater que la psychologie sociale, trop engluée dans sa recherche d'identité et d'autonomie par rapport à une psychologie fascinée par le modèle biologique, n'a jamais eu les moyens théoriques nécessaires pour constituer une approche théorique sérieuse de la question du grand homme.

III. *L'approche psychologique*

Il peut paraître paradoxal que nous reprenions à notre compte une approche qui apparaît comme l'effet d'un découpage et d'un rejet pour le moins discutables.

Sous l'étiquette de psychologie, on considérera ici les approches qui, quelles que soient leurs différences, leurs désaccords théoriques et institutionnels, ont en commun leur référence à Freud, à l'inconscient, à ce qu'il en a théorisé sous l'appellation de théorie psychanalytique. Il ne s'agit pas là d'une option partisane sans fondements: au regard de la question du grand homme, ce qu'il est généralement convenu d'appeler la *psychologie*, quelles que soient ses origines, américaine ou soviétique, quelles que soient ses orientations, behavioriste ou cognitiviste, n'a pratiquement jamais rien trouvé à dire. Chaque fois, quel que soit l'angle d'approche considéré, qu'une psychologie a abordé cette question, il s'agissait d'une psychologie inscrite, à un titre ou à un autre, dans la descendance freudienne.

A cela il y a au moins deux raisons fondamentales.

D'une part l'intérêt porté par Freud tout au long de sa vie et de son oeuvre au thème du grand homme, à son rôle et à sa fonction

dans l'histoire et la politique. Sans doute, et la chose est suffisamment connue pour ne pas craindre se livrer ici à une quelconque interprétation sauvage, faut-il voir dans cet intérêt persistant la trace des premiers idéaux du jeune Freud qui rêvait alors de devenir un chef politique et même un chef d'Etat et par ailleurs, la marque de la conscience qu'avait le maître viennois d'être un pionnier, un découvreur dont le nom risquait de demeurer dans l'histoire de la pensée humaine.

Mais il y a une autre raison à prendre en considération dans l'analyse des déterminations de l'intérêt porté par la psychologie d'obéissance freudienne à la question du grand homme, une raison à caractère plus épistémologique. Cette question ne manque pas en effet, si l'on y prend garde, d'enfermer celui qui y porte quelque intérêt dans l'alternative stérile qui consiste à opposer, à quelque niveau que ce soit, l'individu, l'échelle individuelle, la psychologie individuelle à la société, l'échelle sociale, la psychologie sociale. Par delà son intérêt marqué pour le phénomène du grand homme, Freud avait cerné ce piège, cette fausse contradiction et il avait entrepris d'en démonter les ressorts et d'en indiquer le possible dépassement dans un texte désormais célèbre, bien que souvent méconnu ou mal lu, *Massenpsychologie und Ich-analyse*. Dans ce texte, Freud établit notamment qu'il ne saurait y avoir émergence et installation d'un chef, d'un leader ou d'un meneur sans qu'il y ait simultanément et de manière dépendante existence d'une foule organisée mais qu'inversement il ne saurait y avoir stabilisation et maintien du lien social qui caractérise une foule organisée sans l'existence extérieure d'un point de référence organisateur, d'une instance unificatrice qui désignera la place du chef, celle généralement du grand homme.

Ajoutons, sans prétendre en aucune manière à résumer ici un texte aussi dense et fondamental que ces assertions trouvent leurs fondements dans la théorie psychanalytique elle-même dont elles participent en retour puisque ce texte a joué un rôle fondamental dans l'élaboration par Freud de sa seconde topique et dans sa conception du moi divisé. Le point vaut d'être souligné puisqu'il per-

met d'éviter deux erreurs aussi voisines que classiques, celle consistant dans l'établissement d'une partition entre les oeuvres psychanalytiques de Freud et ses oeuvres socio-politiques ou anthropologiques dont ce texte ferait partie, celle consistant à parler, à propos de cette seconde catégorie de travaux, de psychanalyse appliquée à des objet ou données dont on poserait a priori qu'ils n'ont rien à voir avec la théorie psychoanalytique et avec l'inconscient. C'est un fait que la plupart des recherches développées sous le label dit de la *psychohistoire* n'échappent pas généralement à ce type d'erreur qui consiste à plaquer sur telle ou telle individualité historique tel ou tel diagnostic, en oubliant au demeurant que l'idée même de diagnostic est étrangère à la psychanalyse, ou encore à "psychanalyser" tel ou tel "grand homme" en arguant de tel ou tel fait lié à sa petite enfance ou à l'histoire de ses géniteurs sans qu'en aucun moment, ne soient posées des questions aussi fondamentales que celle d'abord de l'existence ou du bien fondé de la catégorie de grand homme, celles ensuite des raisons et des modalités de cette existence.

C'est dans la perspective de telles questions que nous proposons à présent sous forme d'hypothèses des directions de travail et de recherche sur ce thème du grand homme dans l'histoire et la politique.

IV. Directions de recherche

A - Sur la nature du phénomène

Posons l'hypothèse que l'histoire a connu, connaît peut-être et connaîtra sûrement des grand hommes dont les discours et les actions ont imprimé, impriment ou imprimeront une certaine orientation aux événements qui constituent la matérialité de l'histoire. Diverses explications ont pu ou peuvent être avancées quant à la particularité de ces hommes là. Explications spiritualistes ou d'ordre religieux arguant de dons, de grace ou de foi qui seraient le fait ou la qualité particulière de ceux là. Explications plus scientistes que véritablement matérialistes, celles se tournant vers la biologie ou la physiologie pour arguer de déterminants

spéciaux. Ces deux types d'explication présentent entre autres inconvénients celui de croire et de laisser croire qu'à l'expression "grand homme" correspond nécessairement une entité concrète, empirique, une catégorie d'individu, dont l'examen, l'observation voire la manipulation pourraient fournir des informations concernant sa nature. Nous proposons en ce qui nous concerne de considérer le phénomène grand homme aussi bien que ses réalisations historiques concrètes comme un *effet*, le résultat de la *rencontre* entre une situation historique donnée et un individu donné dont l'histoire personnelle fait qu'il se trouve à même d'énoncer sur la situation historique considérée des appréciations, des jugements qui confère à celle-ci un sens (historique, politique, philosophique), chose que la dite situation, du fait de ses caractéristiques, n'avait pas pour le grand nombre d'individus qui s'y trouvaient inscrits.

B - Sur la teneur du phénomène

Si l'on admet le principe de l'hypothèse précédente en se réservant d'en établir des éléments de démonstration par ailleurs, encore faut-il avancer quelques idées sur le comment et le pourquoi de ce type de *rencontre*. On peut ici user de la conceptualisation produite par Freud dans *Massenpsychologie* et dire que cette rencontre est elle-même le produit de deux types de liens de nature globalement transférentielle mais néanmoins distincts: un lien d'amour de la part de ceux qui jusque là étaient plongés dans la crainte et la panique du fait de leur incompréhension de la situation vers celui qui, par des mots, donne un sens à cette situation - un sens et non pas le sens comme s'il n'y en avait qu'un qui devrait être révélé selon des modalités qui sont celles des religions; un lien de reconnaissance (identification) entre chacun de ceux qui forment la masse et qui sont tournés vers celui qui a donné du sens: je te reconnais comme mon pareil puisque tu reconnais, comme moi, le bien fondé de la parole du ... chef. Quant au "pourquoi" du phénomène, il soulève une question fondamentale impossible à traiter ici, question qui relève de la philosophie po-

litique, qui touche à la contradiction fondamentale entre l'exigence éthique du fait démocratique qui implique la différence et la division et l'exigence stratégique de la survivance qui implique l'unité et l'homogénéité pour ne pas dire la ... totalitarisation.

C - Sur la matérialité du phénomène

Fondamentalement, le phénomène se situe, bien plus que dans les actes ou les comportements qui sont toujours seconds et subordonnés puisque devant être nommés, dans *l'ordre de la langue*. Il ne s'agit pas ici de renvoyer aux schémas usés des théories de la communication et de l'information dans lesquelles la langue n'intervient qu'au titre d'un instrument ou d'un outil. Si nous disons *dans la langue*, c'est que le processus évoqué du sens donné à une situation n'est pas de l'ordre de la nomination institutionnelle ou de la désignation empirique mettant en relation un signifiant (signe) avec un signifié (chose) mais de l'ordre de la représentation symbolique dans laquelle le signifiant représente la chose signifiée pour un autre signifiant, ce qui veut dire que celui qui se trouve être amené à donner du sens à une situation considérée fonctionne comme un producteur de signifiants: ce qu'il dit est reçu par les autres non comme une explication, comme une démarche pédagogique ou didactique, mais comme quelque chose qui, à leur insu, vient résonner en eux et mobiliser, toujours à leur insu une énergie qui les dépasse en tant que sujets conscients.

D - Sur la spécificité du phénomène

Nous n'avons donné là que l'esquisse, et encore, d'une approche complexe qui nous semble cependant présenter l'avantage, comparée aux autres, biologique, mystique ou psychologue, de prendre en compte tous les éléments concernés par le phénomène, qu'il s'agisse de l'individualité particulière de celui en passe de devenir le "grand homme", de l'ensemble des individus concernés à ce moment là ou de l'histoire comme donnée de base.

Il est une autre approche du phénomène, l'approche que l'on peut dire sociologique qui tend à réduire le phénomène du grand

homme à un effet particulier du phénomène global du pouvoir. Si cette réduction ne nous semble pas recevable, et il faudra s'en expliquer, cette approche a toutefois l'avantage de nous obliger à spécifier le type de relation qui existe entre le phénomène du grand homme dans l'histoire et la politique et l'ordre institutionnel du pouvoir.

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ASPECTS OF NEOGENESIS: SPEARMAN'S SYSTEM
OF COGNITION AND APPLIED PSYCHOLOGY

A.D. Lovie

Department of Psychology,
The University of Liverpool

Summary

The paper deals with Spearman's theory of cognition (called by him noegenesis or the generation of new knowledge) in the context of his philosophy of psychological science and his views on the nature and processes of applied psychology. It is less an exposition of Spearman's theory, although its form and origins are briefly covered, rather it is an attempt to describe how Spearman used noegenesis in both pure and applied settings. Consequently, the bulk of the paper deals with the psychometric work of Spearman himself and the people inspired directly or indirectly by him, for example, Slocombe, Line, Stevanovic, Strasheim, Koussy and Wynn Jones. It is also argued that the *a priori* truth (for Spearman) of noegenesis meant that the theory of cognition was not used as a springboard to its further development (or even evaluation), instead it was used to illuminate psychometric performance, and as a means of developing test items and classifying errors in judgments. This uneasy mixture of a *a priori* science and dogmatic technology ultimately doomed the system as such, although ironically it seemed to be in tune with other quite independent enterprises in both Europe and the U.S.A. The paper finishes with an evaluation of Spearman's system, in particular its empirical base and its relationship with more recent work on cognition. This section also answers certain critics of Spearman, specifically Hearnshaw on noegenesis' apparently impoverished conceptual nature and Robert Sternberg on the lack of an experimental underpinning to the system.

Between the conception
And the creation ...
Falls the Shadow.

T.S. Eliot, *The Hollow Men*

Spearman and his Critics

It is ironic that Charles Spearman's noegenetic system of cognition, which he viewed as being at least as important as his mathematical work on factor analysis, has been dismissed by the foremost historian of British psychology as trite, jejune and too simple (Hearnshaw 1964: 199-200). More recent opinion, however, has been kinder. Robert Sternberg, for example, in his *Componential Theory of Intelligence* (1977) writes that "Spearman's information processing theory is most impressive. Many subsequent theories ... are merely extensions of Spearman's" (page 110). It should be noticed that Sternberg is speaking here of Spearman's contributions to analogical reasoning, while Hearnshaw's remarks concern the whole of Spearman's theory of cognition.

Sternberg speaks even more flatteringly of Spearman in his 1979 review paper on mental abilities: "Much of the serious theoretical and empirical research that has been done on the nature of mental abilities can be traced back to Charles Spearman ..." (page 214). Further: "In retrospect, Spearman's approach to understanding the nature of mental abilities seems to adumbrate a stream of research in which differential and information-processing concepts and methodologies could be close linked". (page 215).

I do not see the historian's role as an evaluator of the past, although inevitably he or she would, for at least some of the time, study highly-regarded historical figures, and hence connive if only implicitly in such judgments. However, I feel that the historical significance of Spearman's work on cognition is sufficiently interesting and (from the opinions quoted earlier) sufficiently poorly understood to be given a proper exposure. In particular, I would argue that an account which places Spearman in his historical

and intellectual context would go a long way to understanding today's confusion over his achievement. Further, it could be argued that the views of Hearnshaw and Sternberg are themselves coloured by their preconceptions as to the nature of cognition and, more particularly, their ideas as to the form that *theories* of cognition should take. Hence, a useful antidote would be a historical review of the work.

Noegenesis - An Introduction

Spearman's system of cognition consists of three basic stages. As Tolman implied in his 1932 treatment of the theory, noegenesis is strong on the input and central processing components of cognition, weak on the output side. Quoting from Spearman (1923), the three stages are:

(1) *Apprehension of Awareness*

"Any lived experience tends to evoke immediately a knowing of its characters and experienter" (page 48). Note that by the term "characters" Spearman means certain fundamental and absolute properties of the environment, for example, quality and quantity - a clear reference to Kantian categories.

(2) *Education of Relations*

"The mentally presenting of any two or more characters (simply or complex) tends to evoke immediately a knowing of relation between them" (page 63). Note the distinction that Spearman makes between "relations" and "characters".

(3) *Education of Correlates*

"The presenting of any character together with any relation tends to evoke immediately a knowing of the correlative character" (page 91). Education, in this and the previous stage, is a form of induction.

There is a clear implication that the completion of each stage is immediate, and hence that there are no more than three stages.

Certainly, no more could be discovered because of the lack of time for introspection or experimental manipulation.

Spearman's originality lay not so much in his invention of the separate parts of his system, but in their *synthesis*. The notion, for example, that people are capable of generating related instances (so-called correlates) of the initial events owes much to Stout's "relative suggestion", which in turn owes much to Kant's apperception. He also lists Bühler, Read and even Hume as influences. Further, the middle process in Spearman's system, the eduction (or extraction) of relationships, comes indirectly from the associationists, but more obviously from Bain and Locke, mainly because of its active nature. Spearman cites more recent work here, including Bartlett on perception.

The origins of the initial stage, the immediate apprehension of the characteristics of the objects of awareness and of the experiencer, are more diffuse but go back at least to Locke and more recently to the neurophysiologists of the nineteenth century whose discoveries about the neural basis of sensations allowed Spearman to categorise the objects of awareness along the familiar dimensions of "quality, intensity, spatiality and temporality" (1923: 49). Külpe and the Würzburg school are all quoted by Spearman here. All these influences are readily acknowledged by Spearman (1923, 1937), although he invariably finds that their views are deficient in ways that only he can make good with his system of noegenesis! The origin and meaning of the term noegenesis are contained in the two roots, *nous* and *genesis*, of the word, that is, the creation of new mental content by intelligent or even self-evident means.

A simple example should be enough to illustrate the system. As will be seen later, my choice of an analogical reasoning test as an exemplar is not entirely random, since there is a strong link with applied psychology in Spearman's work on cognition. First, the test -

BLACK: WHITE :: YES: IF, NO, PERHAPS, WHEN.

The doctrine of noegenesis would first represent the initial

items (the so-called "fundaments") in a form suitable for the next process, that is, as colours (or colour names) with particular values, black and white. Next the relationship of *opposite* would be deduced from the colours. Then, the test fundament *yes* and the deduced relation, *opposite*, are combined to educe (or generate) a correlate of this fundament/relation compound. Finally, this would be treated against the four alternatives, *if*, *no*, *when* and *perhaps*, with hopefully the resulting correct response of *no* being produced. (An interesting noegenetic analysis of this analogical test can be found in Thomas (1935: 185-186). Thomas, who was a student of Spearman, included a primitive but clearly discernable flow-chart on page 186 of his book, complete with arrows, choice points, and alternative exits in his treatment. Slocombe (1926) also claimed that when he asked his student subjects how they solved analogies, they described an exact noegenetic process.) The system also allowed the possibility of relations of relations, that is, a hierarchical arrangement of relationships built upon the fundaments.

Spearman fleshed out this fundamental sequence in several ways, for example, by providing a list of possible relations for the second stage of the process (1923: 65-74; 1927: 168-184). These include space, time, likeness, attribution, causality, identity, conjunction etc. Some of these attributes also encode the elements of the initial parts of the process, that is, the immediately apprehended objects of awareness, thus ensuring a continuity between the two stages through the compatibility of description of these elements.

Spearman also supplemented the qualitative system of noegenesis by the five so-called quantitative principles of limited mental energy, retentivity (this includes inertia, facilitation and association), fatigue, conative control and primordial potency (or individual differences). Although Spearman viewed the noegenetic processes as being in some senses self-evident and creative, he still found it necessary to round out his system of cognition with three mechanical anoegenetic processes. These are reproduction, disparition (the process whereby items sink from consciousness) and clearness-variation.

Noogenesis: Science and Technology?

Until the late 1960s, the relationships between science and technology was assumed to be hierarchical, with the latter characterized as an applied version of the former. Traditionally, psychology has adopted a comparable model. In particular, C.S. Myers, the founder of industrial psychology in Britain, wrote in 1926 that "Industrial psychology is an applied science. It is concerned with applying our knowledge of mental processes to the conditions obtaining in modern industry" (page 11). As we shall see this a view shared by Spearman.

In 1971, however, Layton suggested that the relationship was more like that of mirror-image twins, with science and technology having a semi-independent, semi-cooperative existence. Such a non-hierarchical model implies that technology can progress, even with conservative or obselenscent scientific ideas, since its *raison d'être*, aims criteria of success and systems of rewards are not necessarily those of science itself. Although the demarcation line between the two is not always clear, the current views of historians of technology are strongly non-elitist over the relevance of science to technology (see the comments by Barnes and Edge (1982: 147-154) on current thinking). One implication of these views is that a self-conscious attempt to pursue an applied science on the trickle down principle from science to technology would not be as succesful as one which accorded a more independent and equal status to the participants. Interestingly, this is a view put forward as long ago as 1942 by Hearnshaw.

Part of the thesis that I wish to propound is that Spearman's theory of cognition can be seen not as a strictly scientific one but as an uneasy compromise between science and technology. In particular, many aspects of the theory were dictated by Spearman's wish to systematize test theory and construction, to the detriment of both the theoretical and practical parts of noogenesis.

That Spearman was from the first interested in applied psychology is not often appreciated, yet even in what he later (1950) called the fountain head of his ideas - his 1904 paper on general

intelligence - can be found both a commitment to the practical aspects of psychology and an outline of a possible solution to its problems. "And, indeed, when we without bias consider the whole actual fruit so far gathered from this science (experimental psychology) -which at the outset seemed to promise an almost unlimited harvest - we can scarcely avoid a feeling of great disappointment". (1904: 203). Spearman then claims that neither education, psychiatry, or "any other branch of applied psychology" had benefitted from the advances in experimental, laboratory psychology (1904: 203).

Spearman's solution is a correlational psychology "for the purpose of positively determining all physical tendencies, and in particular those which connect together the so-called "mental tests" with psychical activities of greater generality and interest" (1904: 205). He further writes that "Should this ambitious programme be achieved even in small degree, Experimental Psychology would thereby appear to be supplied with the missing link in its theoretical justification, and at the same time to have produced a practical fruit of almost illimitable promise" (1904: 206). Spearman seems here to suggest that advances on the theoretical and scientific fronts would also imply improvements in the practical applications of the work.

In Lovie (1982) I have argued that Spearman, following Newton and Wundt, was, unfashionably for the time, a realist in his scientific philosophy, since he supported the necessary existence of what he called Conceptual Uniformities, that is, "inward resemblances" of the surface of "Functional Uniformities" which can be directly investigated and established by experimental means (1904: 203). Consequently, the only light that can be thrown on these inner conceptual elements is via the experimental determination of behavioural regularities. Further, the apparent simplicity of, say, Newton's laws of motion, according to Spearman, conceals the fact that they "contain the supreme key to every event on the earth below and in the heavens above" (1904: 203).

Here we can see the basis of a hierarchical model of applied psychology, founded upon a limited but fundamental number of laws which describe the operations of entities lying behind the appearance of behaviour. These laws, which Spearman in his later works identified with noegenesis, were usually described by him as being Ultimate. They were also capable of being elaborated into many different forms, sufficiently varied to explain most psychological phenomena. My final points on the 1904 paper are first, that Spearman, not unnaturally, argues that it is impossible to develop tests of intelligence without some idea of what the concept means. His criticisms of earlier work on intelligence testing from Galton to Thorndike focused on their lack of agreement and generally pragmatic approach. Second, Spearman was not really interested in individual differences but in general laws from which individualities had been eliminated by correlational methods (1904: 207; see also Lovie, 1982).

I have spent so much time on this earlier work because of the remarkable consistency between the views presented at the start of Spearman's career and the opinions exhibited in much later works, including his 1923 *Nature of Intelligence and the Principles of Cognitions* where noegenesis was first expounded. In the first two chapters of this text he reaffirms the existence of all-comprehensive ultimate laws, noegenesis, which "rest in their own right" (page 30) at the base of an increasingly specific structure of explanatory concepts. This edifice is crowned by "the uniformities of occurrence" (page 30) which appear to be the early Functional Uniformities, that is, the consistent observations of behaviour to which the theories are tied. Further, the definition of intelligence and the means for measuring it, that is, mental tests are dependent upon these ultimate noegenetic laws (pages 23, 32 and 351). "... No serviceable definition can possibly be found for intelligence until after having firmly established at least the framework of the entire *psychology of cognition*" (1923: 23, Spearman's emphasis).

It is, however, in Spearman's 1927 *Abilities of Man* that the practical value of noegenesis is most clearly spelt out since it is here that the principle is explicitly linked with his two-factor theory of ability (chapter 12, which, in turn, is seen as having a role in the proper development of mental tests of intelligence (page 77). Chapter 12 is of particular interest here since it outlines the type of research using noegenesis in the years between 1923 and 1927. The work by Hamid (1925), Slocombe (1926), Strasheim (1926) Davey (1926) and the unpublished studies by Spearman and others (1927: 201-202), all take the form of mental tests, as does the evidence he cites in this chapter from studies not directly influenced by the doctrine. In general, the highest correlations with Spearman's general factor, *g*, were with reasoning tests, particularly those that emphasised the extraction of symbolic (usually verbal) relations, although Hamid (1925) found that subjects were readier to educe correlates than relations. For Spearman, therefore, *g*, was primarily a measure of the extent to which the processes of noegenesis were involved in test performance.

The only major exception to this spate of tests was the monograph on the processes of judgement by Stevanovic (1927). Here noegenesis is, as usual not evaluated, rather it is used as a guide in identifying the operations involved in concept formation and in the classification of possible error, for example, mechanically substituting a similar (but wrong) item for one based on the inferred properties of the stimulus (the error of associative reproduction, as against the correct one of educing a correlate).

Although Stevanovic was not a student of Spearman, the doctoral work that forms the monograph had been supervised by Spearman's close colleague, the philosophically inclined Francis Aveling.

The *Abilities of Man* also contains Spearman's usually affirmation of the ultimate nature of the laws of noegenesis. Here they are even referred to as ultimate processes (page 162). The harping on the *a priori*, self-evident, nature of noegenesis is a natural outcome of Spearman's realist philosophy since there is little utility in believing in the ultimate scientific reality of

nothing in particular. Such an attitude, however, means that work using noegenesis is not concerned with establishing its existence - indeed all the work by Spearman and his students assume that it is true by *fiat* - rather it is there to be *exploited* in various ways. For example, many workers employed it as a rational way of devising tests of intelligence and ability (Strasheim, 1926, Hamid, 1925, and Cox 1928), while others, for instance, Line (1931) and to a lesser extent Fowler (1931), used the multi-level, multi-process nature of noegenesis to differentiate between the growth of the various processes, for example, the apprehension of fundamentals and the eduction of relations. Work on learning, particularly massed versus distributed practice, was investigated by Gopalaswami using noegenesis to elucidate the nature of the transfer of training of relationships between the two conditions (1925: 236). Even the work on the errors committed in judgment, for example, Stevanovic (1927), Line (1931), Fortes (1932) and Spearman himself (1929), assume the truth of noegenesis, using its various processes as a norm against which to compare and classify their subjects responses. This is, of course, quite unlike the strategy adopted by more instrumentally inclined experimenters who would have used errors both as a *test* of the value of the concept and as a way of developing it further (see Sternberg, 1977, here for a survey of modern approaches to models of analogical reasoning).

The use of noegenesis as a kind of intellectual *flambeau* can also be seen in the work of various influential British educational researchers of the period, for example, Ballard (1928), Nunn (1930) and Valentine (1942); whilst psychologists as unlikely as Tolman (1932) and even Koffka (1935) were clear on the relevance of noegenesis to their own thinking. One cannot escape the feeling, therefore, that noegenesis, in so far as it emphasised relationships which themselves generated novel psychological content, was in tune with many influential ideas both in Europe and the U.S.A. Even Spearman's polemical article on Gestalt psychology shows him not unsympathetic to one branch, historically the most important, of the philosophy (1925: 220). This movement, according to Spear-

man, conceived perception as a two-stage process, one emphasising analysis, the other a creative synthesis, to arrive at a judgment of shape.

As can be seen from the last paragraphs, Spearman continued to write and to inspire others well after *The Abilities of Man* (1927). I would like to finish this short historical and conceptual survey of noegenesis by mentioning two further works by Spearman himself and one monograph by Koussy. The latter was based on a doctorate supervised by William Stephenson, another close colleague of Spearman.

The Spearman volumes are his interesting and suggestive monograph on the application of noegenesis to artistic reaction and scientific discovery, *Creative Mind* (1930). Although it contains copious references to his own and his students work, as was the case with most of his books, the text also has sections on the pictorial arts, music and poetry. The other text is his two volume history of ideas in psychology, *Psychology Down the Ages* (1937). As a history of psychology it is not really comparable with other historical works of the period. However, the book works better as a historical justification of Spearman's own views, particularly his doctrines of noegenesis and the two-factors, although it does not convince even then. As Hearnshaw rather drily observes "The book is full both of curious erudition ... and of a stubborn obtuseness, and it showed suprisingly little awareness of many vigorous growing points of contemporary psychology" (1964: 201). Perhaps the kindest comment to make would be that any prominent scientist whose life was as long and as active as Spearman's is bound to be overtaken by events. It is interesting to contrast, for example, the review by Nunn of Spearman's 1923 *Nature of Intelligence* where Nunn emphasised the experimental basis of noegenesis, with Zangwill's comment on *Psychology down the Ages* (1937), which he described as being singularly lacking in experimental material. It should be noted that the latter text contains all the ideas from the former, together with most of the psychometric and other work inspired by noegenesis published in the fourteen intervening years. Clearly

what counts as an experiment had changed over the period as had the recognised means of developing a psychological argument and system. Cattell, for example, in his obituary of Spearman, pointed to his continuing use of philosophical ideas and modes of discourse (1945: 89).

The final work to be mentioned is that by Koussy on developing tests for spatial perception (1935). Here he finds evidence for a group factor, K, involved in manipulating internal visual images. This is important for noegenesis since K correlates highly with complex spatial tests for educing correlates (pages 83-84). Equally relevant is that tests which involve the eduction of relations only have a low correlation with K. Here, as before, noegenesis provided both the guiding principle behind the classification of tests and their functional analysis, that is, what operations are assumed or required to solve the test items, and a way of explaining the results.

Spearman's final book, *Human Ability*, was published postumously in 1950. Co-authored by the educationalist, Wynn Jones, his friend and colleague of many years standing, the book was subtitled *A Continuation of "The Abilities of Man"*. It contains much new material, mainly on the development of the two-factor theory. There is, however, little about noegenesis and no talk of ultimate laws. Whether the harsher, more positivst atmosphere of post-War psychology had made such grandiose claims unfashionable or whether the text reflected the more empirical tastes of Wynn Jones is difficult to decide at this time. What is clear, however, is that noegenesis, at least as conceived by Spearman, died with its creator in 1945.

Final Comments

Although my survey of noegenesis has of necessity been brief, I believe that we are now in a position to offer a more balanced view of Spearman's system of cognition than is provided by either Hearnshaw or Sternberg.

First, Spearman's interests were both technological and scientific, with developments in science judged to be prior to, and

hence essential for, developments in technology. The problem, however, is that the improvement of tests of intelligence and more specialised abilities have rarely if ever been dependent upon theoretical developments in psychology. It is worth comparing Spearman's limited impact on the practical business of test construction with Thurstone's more successful, if a-theoretical, foray into tests for selecting engineering students (see Noble, 1979, and Lovie, 1982). With Thurstone the improvements in the mathematical analyses and scaling methodology were a direct result of pressure from the applied side, with little or no consequent improvement to scientific psychology. Indeed, Thurstone openly embraced a faculty theory of psychology (Lovie, 1982). This is still mainly true today, even if one agrees with Hunt when he writes, in an uncanny echo of Spearman, "The psychology of intelligence must be part of the psychology of cognition" (1976). From this point of view, therefore, Hearnshaw's statement (1964: 200) that Spearman's work helped test development should be viewed with some scepticism.

Equally questionable is Hearnshaw's claim that Spearman's system is overly simple and jejune, since it lacks detailed mechanisms for the processes. I would concede that this is true on the response side, a point made by both Sternberg (1977) and, interestingly, Tolman (1932). Sternberg, for example, describes Spearman as leaving his subjects "buried in thought" (1977: 109). However, with three stages, five quantitative principles and three anoenetic processes, Spearman could be accused of having an embarrassment of conceptual riches with a system which was (potentially) complex enough to cover many situations and behaviour. A more plausible criticism is that the system was based on far too few experiments and hence was never specified in detail.

Of course, modern experimenters in cognitive psychology are, whether they like it or not, the children of a positivist and instrumentalist tradition of which even Behaviourism in its various forms is little more than an extreme example. Consequently, what would today be an unacceptable experimental practice, for example, using subjects introspections as the main source of evidence with

little or no controlled manipulation of the variables, would be perfectly respectable in the first two or three decades of this century. One has only to recall the nature of Piaget's early experiments, for example, to realise the extent of the changes in methodology since, say, 1930. To this extent, therefore, Sternberg's criticism that Spearman did not present any experimental results (page 109) is not really fair. However, since I have argued that Spearman was a scientific realist, believing that the truth of noogenesis was self-evident, so the wealth of information processing detail that Sternberg detects in Spearman's work appears to come from thin air. Of course, as I suggested earlier Spearman was in debt to many earlier psychologists and philosophers, but their experimental techniques would have been even more unacceptable to Sternberg. We are, therefore, left with the conclusion that many of our long established *experimental* findings were, in fact, originally put forward on quite different grounds.

Further, Spearman would undoubtedly have argued that since noogenesis was self-evident, its attraction for the experimentalist lay in the utility of its application (and there are many instances here that Spearman could point to), rather than in its ability to explain or predict behaviour, although noogenesis should in theory be capable of both in the appropriate experimental setting. The problem, however, in an applied setting with such an *a priori* theory is that although it offers a mechanical and predictable means of generating test items, for example, it does not by and of itself ensure the kind of flexibility and conceptual open-endedness that is needed by any developing subject, whether science or technology.

This then is the dilemma of noogenesis: Spearman conceived it as an answer to the problems of applying science to technology in psychology. Unfortunately, since as science it was deemed to be prior to the technology, and further since noogenesis was conceived as a complete and self-evident system, it became an authoritarian structure that eventually choked off its own growth both as science and technology. However, the ideas that noogenesis synthesised,

even the synthesis itself, continue to be used, although they often appear under different guises. The apprehension of the objects of awareness, for example, is translated as "encoding" by Sternberg, while the eduction of relations is viewed as inference about a rule (1977: 108). Although the quantitative principles are not a central part of noegenesis, certain of them are of sufficient contemporary interest to be worth mentioning. In particular, the principle of mental energy has recently surfaced as the limited attentional resources which can be allocated either in a controlled or automatic fashion. Since it is assumed to be fixed in size, so allocating resources to one function reduces the amount that can be given to others. Spearman (1927: 263), following many other writers, accounted for the results of divided attention tasks in such terms, in exactly the same way as more modern writers such as Norman and Bobrow (1975) have done. Clearly we cannot escape our past, however hard we try.

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THE HISTORICAL DEVELOPMENT OF RESEARCH
PRACTICE IN AMERICAN PSYCHOLOGY

K. Danziger
York University
Toronto, Canada

Summary

A distinction is drawn between two historically important models of research practice in psychology, the Wundtian and the Galtonian. In the former the primary data are constituted by the properties of individuals, in the latter case the primary data are constituted by the properties of groups. Experimental manipulation was at first limited to the Wundtian model and was designed to explore psychological causal processes operating in individuals. As long as the Galtonian model operated with natural groups causal inferences were logically excluded, except in a Humean sense. With the switch from natural groups to treatment groups constituted by the experimenter it became possible to make causal inferences within the Galtonian model. However, these inferences were still limited to group properties and their extension to individuals raised fundamental problems. There is a demonstrable striking increase in the popularity of the neo-Galtonian model in the American psychological research literature during the first four decades of the twentieth century. It is suggested that this development is a function of the importance which potential educational applications held for much of American psychological research during this period. The neo-Galtonian model of research yielded a form of knowledge which was particularly well adapted to the technological requirements of educational administrators.

The Wundtian model and the Galtonian model

Modern empirical psychological research originated in two entirely different and independent models which may be referred to as the Galtonian and the Wundtian model respectively. The classical Wundtian experiment was designed to throw light on causal psycholo-

gical processes operating in individual minds (See Danziger, 1980). It did this by systematically varying experimental conditions and observing the results. In this respect the psychological experiment was similar to a physical experiment (Wundt, 1880-83), though the theoretically most important conditions to be varied were subjective in nature, such as the direction of attention.

The point is that the relationship between experimental conditions and responses of the subject were not of primary interest in themselves but were simply a means for the exploration of underlying psychological processes with causal properties. These causal processes existed in individual minds and the experiment was designed to explore them in this individual context. The responses of experimental subjects represented the manifestations of an underlying "psychic causality". Up to a point this was analogous to the way in which the responses of a physical body in an experiment represented the manifestations of an underlying physical causality. From this point of view any increase of the number of experimental subjects above one constituted a replication of the experiment. It was sometimes dispensed with altogether, or at best limited to a handful of cases, the results of each one being reported individually.

The Galtonian model for psychological investigation was quite different. Here the aim of the investigation was the establishment of the distribution of qualities in a population and of the co-relation of these qualities in the same or in different populations. The conception of causal explanation was Humean or positivistic (Pearson, 1892)*. What the collection and analysis of data

* Strictly speaking, one should speak of a "Galton-Pearson", rather than "Galtonian" model, because while Galton provided the inspiration, Pearson provided the philosophically and technically complete formulation. However, the term "Galtonian", is more convenient as an adjectival form as long as it is understood as a kind of historical shorthand.

was meant to accomplish was the demonstration of regular associations among observations, not the provision of evidence for the operation of underlying causal processes. Accordingly, experimental manipulation was limited to the construction of instruments for the collection of observations under standard conditions, mental tests and questionnaires, for the most part. These were administered to relatively large populations, so that the distribution and covariation of responses in these populations could be plotted. The properties of interest were population properties, and individual observations were relevant only in terms of their contribution or relationship to population parameters.

The two kinds of research practice implied two basically different conceptions of causality. In the classical Wundtian experiment the establishment of a systematic relationship between two variables always involved an asymmetry which provided the basis for causal attributions. One of the variables was under the experimenter's control and could be "independently" varied. The question was whether this resulted in regular changes in the "dependent variable", i.e. whether a generative causal effect was involved.

The situation is quite different in the Galtonian model. Here the investigator attempted to impute relationships in situations over which he had no control. Such relationships had therefore to be symmetrical, because the investigator was unable to influence the situation so as to give the status of cause to one of the variables and the status of effect to the other. What he could hope for was a measure of concomitance or covariation. The descriptive statistics of the Galton-Pearson school provided him with such measures. This involved a switch to a positivist conception of cause as mere concomitance without reference to generative processes. In this model the establishment of statistical correlations among attributes of natural populations became the goal of research, while in the Wundtian model statistics had been at best a means for ensuring the reliability of observations.

The switch from the Wundtian to the Galtonian model of investigation can be documented in the American psychological re-

search literature over the first four decades of the twentieth century. If we examine the experimental reports published in the major journals we find that in the earliest days of American experimental psychology most of the published reports describe investigations on individual subjects. These investigations are designed to explore actual psychological processes taking place in identified people.

But we also get the first instances of another type of investigation. Here data are collected from relatively large groups of subjects without any attempt at studying the effects of variations in experimental conditions. The most characteristic feature of these studies is the interest in *group* rather than individual data. Individuals only enter the picture in terms of their locations with respect to some property characterizing the group. This type of study gradually becomes more frequent.

The reasons for the switch to this new model of psychological investigation are connected with the fact that the Wundtian model had a limited range of application. Wundt himself saw only a restricted scope for experimental psychology in his sense, and he actually opposed both the independence of psychology from philosophy and the application of scientific psychology to real life problems (Wundt, 1913). This attitude was emphatically rejected by some of the younger German psychologists and it was never accepted by any but a tiny handful of American psychologists. The desirability of making psychology relevant to real life was already proclaimed by William James (1892), and the question of how to do this remained a major preoccupation among the leaders of early American psychology (Danziger, 1979; O'Donnell, 1979). The future of the discipline in their society plainly depended on this.

The early years of the twentieth century were therefore a period during which many psychologists were attempting to break the narrow bounds which the Wundtian model had prescribed for them. The alternative Galtonian model provided them with a heuristic that was singularly appropriate for their purposes. Indeed, there were very real parallels between the situation faced by Galton and that which confronted early twentieth century American psychologists. Galton's

abiding interests lay in the creation of a science of heredity, but his problem was one of establishing such a science in the absence of controlled experimentation. The psychologists' problem was one of promoting a socially relevant science involving areas of human functioning that were not accessible to precise experimental study under controlled conditions. Descriptive statistical measures of group attributes, had seemed to provide a solution to Galton's problem. His example proved attractive to the many psychologists who followed in his footsteps.

From the point of view of the establishment of psychology as a profession the adoption of the Galtonian model proved to be a huge success. It provided psychologists with a readily marketable social skill, namely intelligence testing, which demonstrated the social utility of psychological knowledge and firmly established the psychologist as a recognized technical expert. The mass testing of recruits for the American army in World War I greatly accelerated this process of professionalization, which had just begun to get under way (Samelson, 1977).

Neo-Galtonianism and the treatment group

The original Galtonian groups were natural groups, that is to say, they were drawn from actual socially defined populations, men and women, sophomores and freshman, nine year olds and ten year olds etc. But in due course psychologists began to constitute research groups on a different basis.

One of the early results of American psychologists' interest in finding practical applications for their work could be seen in studies of the effects of training and fatigue on functions like motor skills and rote learning. Although these topics were of little significance in terms of the then prevalent theoretical goals of psychology, their relevance to practical situations in education and industry was obvious. But what requires attention in the present context are the consequences which this type of content had for research methodology. The most apparent immediate effect is the tendency to report the results of such studies in the form of

group data. While individual results are often still given in the published reports there is a slowly growing tendency to group the results from a number of subjects exposed to similar experimental conditions in aggregate values. It is these latter that come to be treated as the real results of the experiment, the individual departures from the aggregate values being treated as "error". Eventually, the actual performances of the individuals in the experiment are not reported at all, and only group values are given and judged worthy of scientific discussion.

At first, these aggregate values are simply used to summarize the performance of a single group of subjects taken through a particular programme of practice or exercise. But in due course the limits of the kind of information obtainable on this basis become apparent. As in the case of the original Galtonian studies on natural groups, the data from these artificially created laboratory groups are essentially descriptive. To be able to make causal inferences it is necessary to introduce a comparative perspective and to study the difference in the performance of two or more groups exposed to different conditions. Thus is born a fundamentally new entity in psychological research, namely, the *treatment group*. Starting from insignificant beginnings early in the century, this unit of study was beginning to occupy a noticeable place in the literature of the nineteenth twenties and was eventually to provide the dominant form for psychological research.

This involved a modification of the Galtonian model by extending it from natural groups to deliberately constituted experimental groups. Because these groups had been created by the manipulations of the experimenter it was assumed that any relevant differences in their attributes could be seen as the effects of these manipulations. There emerged a style of research which is a hybrid product of the Wundtian and the Galtonian model. One may speak of a neo-Galtonian model because the Galtonian component is the dominant one. For while the shift from natural to artificial groups had left the essentials of the Galtonian model intact, the shift from individual to group data had destroyed the basis of the Wundtian

model and left only the external trappings of controlled experimentation. What was lost was the core concept of psychic causality which had provided the link between the experimental manipulations and the theoretical interpretations of their effects.

As long as variations in experimental conditions could be directly related to actual psychological processes taking place in an individual human subject the question of psychological inference was not a problematic topic. This tradition of psychological research was in fact continued very effectively by the Gestalt psychologists. But with the switch to aggregate data the potential for trouble in interpreting the meaning of these summarized effects was ever present. By switching from natural to experimental groups the neo-Galtonians had opened up the possibility of demonstrating the existence of causal effects, but they had come no closer to relating these effects to psychological processes in real live human individuals. For the attributes and changes in attributes which they were able to describe were the properties of groups which these psychologists had themselves created and which had no historical existence outside their laboratories.

This entailed two sets of problems. One is the problem of statistical inference and concerns the question of generalizing from laboratory groups to groups outside. The other is the problem of psychological inference which is more subtle but also more fundamental. It concerns the difficulty of getting from the attributes of a particular aggregate to psychological theories that refer to something happening in human individuals. For the aggregate is defined by the operations of the experimenter, not by the operations of the subjects - it is in every sense a *treatment group*. As such, it differs fundamentally from historical groups which individuals of like mind may decide to form. What defines a treatment group, however, are not the characteristics of the subjects but the intentions of the experimenter.

The social context

If we wish to understand the general direction of development

that characterized American psychology during the first half of the twentieth century we need to pay very serious attention to institutional factors. These are of two kinds - firstly, the gradual emergence of institutionalized research practices internal to the discipline, and secondly, the external institutional settings on which practitioners of the discipline depended for the resources that made their research, and indeed their professional careers, possible. While a number of factors were obviously at work, I believe that we can discern a consistent trend for the discipline-internal practices to adapt themselves to the requirements of the external institutional contexts in which these practices operated. These external contexts involved mainly universities and various social agencies, among which the school system was of primary importance during the most of the period in question.

This is evident enough in the research content, which becomes increasingly taken up with questions of mental testing and of learning. But what may be of more fundamental significance, though less immediately obvious, is the effect which these intimate links of the requirements of educational institutions had on the form of psychological research practice. The neo-Galtonian mode of research was precisely adapted to providing the kind of knowledge that educational administrators needed. What they required was a rationale on which to base their decisions about the implementation of various training programmes. In the American context questions of tradition played a far smaller role and questions of efficiency a far larger role in making these decisions than they did in Europe. The psychological studies initiated in this context were generally characterized by the following features: (1) The goals of the research were determined by issues of specific and immediate social concern rather than by issues of general psychological theory; (2) The questions asked were questions of output, performance and efficiency, rather than questions involving internal psychological processes; (3) The subjects on whom this research was carried out, and to whom its results applied were assumed to be minors, either in legal fact, or in the more general sense that they were persons

without valid insight who were not free to determine their own fate but were objects of social control by those in authority.

It appears that the neo-Galtonian model of psychological research represented the routinized and quasi-formal expression of these features. The model's substitution of the treatment group for the individual as the primary object of study, as well as its concentration on the analysis of quantified performance measures, were a crystallization of the research requirements that arose in certain practical institutional contexts. Historically, the requirements of educational institutions, or rather, of their administrators, appear to have played the dominant role. Insofar as they were faced with analogous decisions, administrators of other social, agencies and of business corporations could at times find the results of this style of psychological research of interest. One should not underestimate the potential merits of this style of research from a certain point of view. It was and remains capable of yielding results of some utility in situations where its limitations are acceptable. These are primarily situations in which human individuals appear in the role of minors with curtailed insight and autonomy, where decisions hinge on quantitative measures of group performance, and where limited institutional goals disallow critical reflection on the range and definition of relevant treatment conditions.

In the period between World War I and II American psychologists succeeded in developing the basic features of a new model of research practice which was quite well suited to the task of providing administratively useful information in limited institutional contexts. They had done this by incorporating the requirements and limitations of such contexts in their paradigm of research practice so that it is not inappropriate to refer to it as a *technological* paradigm.

By 1940 experimental research was dominated by the neo-Galtonian model, which was beginning to be equated with "the scientific method" as such. This soon led to problems, because the favoured research model has been developed to help with administrative decisions, not to illuminate general theoretical issues.

The mismatch between the technological goals that had been built into prevailing conceptions of research design and the theoretical aspirations of the discipline was reflected in the adoption of certain devices that were designed to bridge the gap between theory and evidence. One such device was the test of statistical significance. This procedure made it possible to equate the evaluation of psychological theories with the making of an administrative decision about the effectiveness of group treatment programmes. Problems of *psychological* inference were therefore sidestepped and presented as problems of *statistical* inference. Theoretical issues were treated as a matter of administrative stipulation ("operational definition"). It was this research model that came to dominate ever broader areas of psychology in the period following World War II. However, this raises issues that are beyond the scope of the present analysis.

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VOLKSPSYCHOLOGIE, STATISTIK UND SOZIALREFORM

Siegfried Jaeger

Psychologisches Insitut der
Freien Universität Berlin

Summary

Around 1870 in Berlin the political scientist H. Schwabe and the pedagogue F. Bartholomäi analysed living conditions in urban centers and their psychological impact on social groups. The analyses, which were based on population statistics, were used to develop reform proposals.

The pedagogical relevance of these attempts at an empirical and theoretical legitimization of the demand for an educational reform suited to the educational needs of the people is assessed. A survey of the contents of children's minds on entering school is seen as a new approach to the analysis of the psychological presuppositions of school training based on the urban conditions of child development.

The more general aim of this attempt at an "urban psychology" was a differential psychology of urban people based on statistical data on age, social structure, religious, cultural, and political tendencies, and working and living conditions.

The example of housing is used to illustrate the conjunction of psychological analysis and social reform proposals, which were to offset the disadvantages of urban life in order to mitigate social radicalism.

The new methodology used by the authors is seen as one of the first attempts at the scientific use of statistics.

Bemühungen, von anderen Wissenschaftsdisziplinen her in praktischer Absicht einen Beitrag zur Fortbildung der Psychologie zu leisten, finden selten das Interesse von Psychologiehistorikern, wenn diese Beiträge nicht auf den Hauptentwicklungslinien der einzelwissen-

schaftlichen Psychologie liegen. Hierzu gehören sicherlich auch die in Vergessenheit geratenen Arbeiten des Pädagogen Friedrich Bartholomäi (? - 1878) und des Staatswissenschaftlers Hermann Schwabe (1830-1874). Als Mitarbeiter bzw. Leiter des Statistischen Büros der Stadt Berlin mit dem Organ *Berlin und seine Entwicklung* versuchten sie gemeinsam, im Anschluss an Herbarts Theorie und neueren natur- und sozialwissenschaftlichen Ansätzen eine Konzeption zu entwickeln, mit der die vielfältigen Probleme der rasch wachsenden Grosstadt zur Zeit der Reichsgründung auch psychologisch begreifbar und praktisch angehbar gemacht werden sollten. In diesem Rahmen begründen sie mit der Untersuchung des Vorstellungskreises von Schulkindern eine Forschungsrichtung im Grenzgebiet von Psychologie und Pädagogik, die sowohl in Europa als auch in Amerika fortgeführt wurde; entwickeln sie das Konzept einer psychologischen Statistik als Erkenntnismittel und Zugang zu einer praktischen Psychologie und versuchen, als Ansatz zu einer Volkspsychologie die psychologischen Besonderheiten der Grossstadt und ihre Bewohner herauszuarbeiten.

Ihre Programmatik, die sich bereits in Schwabes Arbeit über *Die Engländer und ihre Kohlenarbeiter* van 1862 andeutet, wird von diesem in dem Aufsatz *Betrachtungen über die Volksseele in Berlin* von 1870 formuliert. Ausgehend von Herbarts Diktum, dass die Psychologie so lange einseitig bleibt, als die den Menschen ausserhalb von Geschichte und Gesellschaft stehend betrachtet, wird eine Volkspsychologie angestrebt, die mit der Völkerpsychologie von Lazarus und Steinthal die Bestimmung als Psychologie des gesellschaftlichen Menschen oder der Menschlichen Gesellschaft gemeinsam hat, aber gesellschaftliche Gruppen zum Gegenstand nimmt, z.B. eine Grossstadt, "sobald sie von einer bestimmten Einheit und Energie des geistigen Lebens beherrscht wirdt". Die Statistik soll der Volkspsychologie dienen, 1. "indem sie versucht, die von ihr gesammelten Daten und gefundenen Resultate ... auf die einfachen, elementaren Kräfte des menschlichen Bewusstseins zurückzuführen, d.h. psychologisch zu erklären", und 2. "indem sie durch direkte, selbständige Erhebungen psychologisch interessante Momente zutage fördert" (Schwabe 1870: 126 f.).

Es können hier nur die für den Versuch einer "Volkspychologie der Grossstadt" relevanten Arbeiten vorgestellt und die Verbindung von theoretisch-methodischen Ansätzen und sozialreformerischer Motivation deutlich gemacht werden. Die pädagogisch-schulstatistischen Arbeiten stehen am Anfang der Kooperation von Schwabe und Bartholomäi.

Auf Anregung von Bartholomäi hatte der Pädagogische Verein in Berlin eine Vollerhebung über den Vorstellungskreis der Schulanfänger versucht. Diese konnte aber erst nach Unterstützung durch das von Schwabe aufgebaute Statistische Büro realisiert werden. An der Zusammenarbeit war Schwabe interessiert, da der Auftrag des Berliner Bürgermeisters von 1867, "eine Statistik des Schulwesens zu erstellen, welche die bestehenden Schulen, ihre Einrichtungen und Leistungen vergleicht mit dem Bedürfnis der städtischen Bevölkerung resp. ihrer verschiedenen Klassen oder Schichten" (vgl. Schwabe & Bartholomäi 1870, 2), ihn auf pädagogische und schulpolitische Fragen verwies. Auch hatte er bereits 1864 in einem Aufsatz *Die nachteiligen Wirkungen der Arbeitsteilung auf den Menschen* analysiert und gefordert, die durch Arbeitsteilung gefährdete Einheit der menschlichen Natur durch harmonische Ausbildung wiederherzustellen und in diesem Sinne Bemühungen um die Einrichtung von Kindergärten, Tätigkeitsschulen (vgl. Pappenheim 1868) und eines Gewerbemuseums gefördert (Schwabe 1866, 1867, 1868a und b). In der Arbeit von Schwabe und Bartholomäi *Über Inhalt und Methode einer Berliner Schulstatistik* von 1870 werden in einem theoretisch-methodischen Teil die Bedingungen der körperlichen, geistigen, sittlichen und sozialen Entwicklung des Kindes und die damit in Zusammenhang stehenden Erziehungs- und Unterrichtsverhältnisse sowie ihre Erfassbarkeit analysiert. In einem zweiten empirischen Teil wird über das vorhandene statistische Material eine Charakterisierung der Leistungsfähigkeit des Schulen sowie ihrer Probleme, den objektiven und subjektiven Bildungsbedürfnissen der Bevölkerung zu genügen, versucht und schliesslich die Auswertung und Interpretation der Erhebung über den Vorstellungskreis der Berliner Kinder beim Eintritt in die Schule geliefert.

Ihre theoretischen Überlegungen gehen aus von dem Postulat, dass jeder entwickelnde Unterricht and die kindliche Individualität anknüpfen muss. Individualität bestimmen sie als teils angeborene, teils erworbene, relativ unveränderliche geistige Form des Lebens, die charakterisierbar ist durch Intensität, Geschwindigkeit und bestimmtes Verhältnis der Menge, Stärke und Gruppierung der Vorstellungsmassen. Um sicher an dem vorhandenen Gedankenkreis anknüpfen zu können, erscheint die empirischen Erforschung der Individualität der Schüler in den unteren Klassen notwendig. Ergänzend zur Analyse der schulstatistischen Materials will daher die Erhebung zum Vorstellungskreis der Schulanfänger die durchschnittliche Individualität feststellen, soweit sie auf den Vorstellungen aus der Umgebung des grossstädtischen Kindes beruht, und durch die Berücksichtigung der schichtspezifischen Einzugsgebiete der Schulen und der Art der vorschulischen Erziehung einen Teil der Ursachen ergründen, auf denen die Verschiedenheit der Individualität bzw. Bildungsfähigkeit beruht. Nach Voruntersuchungen wurden über einen Fragebogen mit 138 Items ca. 2000 Schulanfänger durch ihre Lehrer befragt nach Vorstellungen über Naturgegenstände, Örtlichkeiten ihrer Umgebung, über Märchen und religiöse Stoffe, und ergänzend Fertigkeiten, wie Nachsprechen von Wörtern und Wiedergabe eines Tons geprüft. Die Ergebnisse zeigen, dass die Kinder über viele als vorhanden unterstellte Vorstellungen nicht verfügen, dass die Vorstellungsarmut mit der Entfernung des Erfragten zunimmt und ausgeprägte geschlechtsspezifische Differenzen vorhanden sind und dass der Kindergarten die beste Vorbereitung für die Schule ist. In Unterrichtsmethodischer Hinsicht wird daher gefordert, die quantitativen und qualitativen Differenzen und resultierenden Unterschiedlichen Veränderungsmöglichkeiten der Schüler zu berücksichtigen, die fehlenden Vorstellungen durch möglichst unmittelbare Anschauungen. z.B. Exkursionen herbeizuführen und die auf falschen Voraussetzungen beruhenden Lehrmittel zu ersetzen. In zusammenarbeit mit dem "Verein für wissenschaftliche Pädagogik" hat Bartholomäi später diese Forderungen konkretisiert (1871a, 1873) und versucht, die dazu nötigen psychologisch-theoretischen Voraussetzungen der Lehrbildung zu bestimmen (1871b, 1871f, 1872a).

Insgesamt soll die Schule nach Schwabe und Bartholomäi unter den erfahrungsbegrenzenden und gefährdenden Grossstadtverhältnissen eine Humanitäts- und Hygieneanstalt sein, welche die ungünstigen Lebens- und Entwicklungsbedingungen insbesondere der Unterschichtkinder kompensiert und Teile des Familienlebens ersetzt. Eine allgemeine Volksschule lehnen sie mit der Begründung ab, dass die Anschauungen, das sittlich-ästhetische Empfinden und die Lebensformen in den verschiedenen Ständen so sehr verschieden und infolgedessen die Kinder verschieden individualisiert sind, dass sie getrennt erzogen werden müssen.

Zwar wurden aus Materialgründen nur einige Hypothesen wirklich geprüft und die vermutete grossstadtspezifische Wahrnehmungsweise und Begriffsbildung gar nicht untersucht; dennoch ist anzuerkennen, dass hier erstmals der Versuch gemacht wirdt, durch eine theoriegeleitete statistische Analyse die Fortbildung des Schulwesens wissenschaftlich zu begründen und durch eine direkte Erhebung die dazu nötigen Voraussetzungen zu entwickeln.

In Verallgemeinerung der Überlegungen, wie die psychologische Statistik für eine praktische Psychologie nutzbar gemacht werden kann, fordert Bartholomäi (1871a) die Untersuchung des Vorstellungsvorrats in allen Alters- und Unterrichtsstufen aus allen Gesellschafts- und Berufsgruppen sowie Regionen des Landes durch einen "Verein für Psychologische Statistik", der seine Arbeit bei Erfolg auf andere Gebiete ausdehnen soll.

Spätere Arbeiten - von denen die von G.S. Hall (1882) und B. Hartmann (1885) wirkungsgeschichtlich am bedeutsamsten sind und über die E. Meumann (1911, 335-393) und K. Wilker (1911) einen Überblick geben - gehen kaum über das hier entfaltete Programm der Ermittlung vorhandener Vorstellungen und ihrer Entstehungsbedingungen hinaus. Im Gegenteil, die Frage nach der Determiniertheit des durchschnittlichen Vorstellungskreise durch unterschiedliche Umweltbedingungen wird allmählich aufgegeben zugunsten einer altersspezifischen Statuscharakterisierung des normalen bzw. typischen geistigen Inventars der Kinder, sowie neu hinzukommend ihrer Gefühls- und Phantasietätigkeit. Davon abspaltend werden

Anleitungen zur Beschreibung einzelner Kinder unter Berücksichtigung auch der körperlichen Entwicklung in Form von Personalienbüchern, Schülerfragebögen etc. erarbeitet.

Neben der psychologischen Charakterisierung des Grossstadtkindes führt Schwabe das Programm einer Volkspsychologie weiter in Analysen der psychologischen Wirkungen der Altersverhältnisse, Familiensklassen, Konfessionszugehörigkeit sowie der verschiedenen Beschäftigungen, die an einigen Interpretationen des statistischen Materials illustriert werden sollen. Die charakteristische Regsamkeit der Stadt wird nach Schwabe neben der Vielfalt der Berufe und Beschäftigungen durch die überproportionale Vertretung der 20-bis 30-jährigen erklärt, deren Streben nach Anerkennung und Erfolg die Berliner Gesellschaft mindestens ebenso prägt wie die schwache Vertretung der höheren Altersklassen. Bartholomäi (1870) hat hierzu ein mathematisches Modell zur Bestimmung der "mittleren Tragfähigkeit" der produktiven und unproduktiven Altersklassen und der ökonomischen Verlaufsschwankungen einer hypothetischen Familie entwickelt. Die Rolle der Alten, ihr besseres Verständnis für die Bedürfnisse der Kinder und ihre konservative Haltung kommen für Erziehung, Meinungsbildung und Kulturleben kaum zum Tragen mit der Folge, dass die Bevölkerung zu raschem Fortschritt und Wechsel geneigter ist. Auch die relativ grosse Zahl der Unverheirateten, bedingt durch die Schwierigkeiten, eine Familie zu erhalten, hat nach Schwabe psychologische Konsequenzen; aus der fehlenden Notwendigkeit, die Vorstellungs- und Lebenskreise zu harmonisieren, erklärt er die Tendenz zu Egoismus, geistiger Starrheit und Störungen der psychischen und physischen Gesundheit. Aber die Grossstadt bietet auch ein Korrektiv durch die Betätigungsmöglichkeiten im Vereinswesen. Auf die Schwierigkeiten, die Persönlichkeit des anderen genauer kennenzulernen, wird die hohe Scheidungsrate und die Herausbildung der persönlichen Reklame zurückgeführt. Hierzu hat Bartholomäi (1874) seine interessante Inhaltsanalyse von 1200 Heirats-, Bettel-, Borg- und Kontaktannoncen vorgelegt.

Von den die geistige Kraft und Assimilationsfähigkeit der Grossstadt bewirkenden "allgemeinen Apperzeptionsmassen" behandelt

Schwabe exemplarisch die religiösen und zeigt, dass sich je nach dem Grad der Entfremdung bzw. Bindung an die Kirche und aus der Spezifik religiöser Denkformen von Protestanten, Katholiken und Juden unterschiedliche Bildungsbedürfnisse, Formen des Sozialverhaltens und politisch-kulturellen Betätigung ergeben.

Bei der Analyse der psychologischen Auswirkung der Beschäftigung geht Schwabe allgemein davon aus, dass die grossstädtische Gesellschaft eine industrielle und fast ausschliesslich bürgerliche ist; er meint damit, dass das Konkurrenzbürgertum nicht nur nach Besitz, Vermehrung und Verbesserung der geistigen und materiellen Güter und nach Macht strebt, sondern in diesem Prozess auch den übrigen Klassen ihre bestimmte Form gibt durch den Einfluss auf die Arbeits- und Lebensbedingungen. So ist etwa der Fabrikarbeiter durch die Gebundenheit an gesundheitsschädigende Arbeitsräume, durch Abhängigkeit und das Unvermögen, seine Lage wesentlich zu verbessern, nicht nur äusserlich, sondern auch psychisch geprägt. Die Form seiner Sinnlichkeit ist verständlich, denn er fühlt sich in der Fabrik als einer unempfindlichen Welt strenger Gesetzmässigkeit so wenig als Mensch, dass er draussen in Berausung und Liebe das Gefühl einer Freiheit in den kürzesten Augenblick zusammendrängt. Seine harte Arbeit erzeugt eine Entschiedenheit und Bestimmtheit, die ohne langes Besinnen zum Handeln führt. Daher seien bei Aufständen und brennenden Tagesfragen diese Arbeiter stark vertreten.

Von Schwabes Arbeiten zur Wohnungsfrage (1868c, 1871, 1874a, 1874b) sei die über *Das Nomadentum der Berliner Bevölkerung* erwähnt, welches sich in den fast 50% der Mieter betreffenden Umzüge pro Jahr zeigt. Dies kann nicht auf ein verbessertes Wohnungsangebot zurückgeführt werden, denn Schwabe weist nach, dass es sich zu 80% um Umschichtungen in den wirtschaftlich und sozial schwächsten Gruppen in den ärmsten Bezirken handelt. In den Auswirkungen des Wohnungs- und Gemeindewechsels, dem Fehlen von Häuslichkeit sieht Schwabe ein wesentliches Moment der ethischen und sozialen Entwurzelung der Arbeiterschaft. Zur Abhilfe der Wohnungsnot und ihrer Folgen propagiert er die Gründung von Koloniesiedlungen und zeigt am Beispiel von deren ersten Anfängen, dass sich die Vorteile des

Landlebens bei entsprechend ausgebauten Verkehrsmitteln mit denen der Grossstadt verbinden lassen (vgl. 1873). Durch ein Dauerwohnrecht soll die in der Stadt zur blossen Ware gewordene Wohnung wieder häusliche Heimat werden können, und durch die günstigeren und gesünderen Wohnverhältnisse und Mischung der Bevölkerung sieht er viele soziale Missstände entfallen. Schwabe gibt noch eine allgemeinere, von Darwin inspirierte Begründung: Der Kampf um die Existenz sei überall in der Natur um so härter, je mehr Individuen auf begrenztem Raum zusammenkommen. Daher sei die Verpflanzung, die Kolonisation das einfachste Mittel, den Existenzkampf humaner zu gestalten (vgl. 1874b, 60). Dass die Wohnkolonien gerade nicht den besonders durch das Wohnungselend betroffenen zugutekommen und bald Spekulationsinteressen unterworfen sind, hat Schwabe nicht gesehen bzw. nicht mehr erlebt. Mit ihren Vorschlägen wollten Schwabe und Bartholomäi die Mängel der Grossstadt kompensieren bzw. Alternativen zu deren wildwüchsigem Wachstum aufzeigen, um so die soziale Frage zu entschärfen. Trotz aller Kritik an den sozialen Auswirkungen der kapitalistischen Produktionsweise sehen sie diese als natürliches Resultat der Entfaltung der Arbeitsteilung an. Gesellschaftstheoretisch stehen sie so der Position von Friedrich Albert Lange nahe, mit dem sie auch das Anliegen teilen, der Psychologie neue Grundlagen zu schaffen (vgl. Lange 1865, 1870). Nach Schwabes frühe Tod (1874) signalisiert die Umwandlung seiner Zeitschrift in ein Statistisches Jahrbuch der Stadt Berlin ein anderes Wissenschaftsverständnis. Der Statistiker habe sich auf die wissenschaftliche Messung und Darstellung der Tatsachen zu beschränken. Die der statistischen Methode implizite Objektivität erlaube es nicht, sich einer bestimmten sozialpolitischen Strömung anzuschliessen. Die kritische Analyse und Veränderung der sozialen Bedingungen der untersuchten Erscheinungen soll nicht mehr - wie Schwabe und Bartholomäi dies sahen - zur Verantwortung der Wissenschaftler zählen (vgl. Böckh 1878).

Nicht zuletzt weil die Gegenstände ihrer Arbeiten Inhalte verschiedener Einzelwissenschaften wurden, sind Schwabe und Bartholomäi in Vergessenheit geraten, wenn man absieht von den gele-

gentlichen Erwähnungen ihrer - regelmässig falsch zitierten - Arbeit zum vorstellungskreis in der Psychologie, den Hinweis auf Bartholomäi (1887) als Biograph Herbart's und Herausgeber von dessen pädagogischen Schriften in der Pädagogik und auf Schwabe als Förderer der statistischen Darstellungsmethoden und Autor jenes nach ihm benannten "Gesetzes" über die relativ steigenden Kosten für Wohnung bei sinkendem Einkommen in der Volkswirtschaft.

Ihr Versuch einer Volkspsychologie, der letztlich auf eine psychologische Erklärung der statistisch fassbaren sozialen und geistigen Erscheinungen gesellschaftlicher Gruppen der Grossstadt abzielt, hat keine wissenschaftlich ernstzunehmende Fortsetzung gefunden (vgl. Reich 1879, Hellpach 1941). Ihre Vorstellungen von wissenschaftlich begründeter Schul - bzw. Sozialreform werden zum Teil einerseits von den mit statistischen Material argumentierenden Vertretern der Schulreformbewegung (etwa Preyer 1887), andererseits von dem 1873 gegründeten "Verein für Sozialpolitik" fortgeführt, dessen Vorläuferorganisation, den "Zentralverein in Preussen für das Wohl der arbeitenden Klassen", Schwabe in leitender Funktion angehörte.

Bestehen bleibt auch ihr Verdienst, die bereits im ausgehenden 18. Jahrhundert artikulierte (vgl. Trapp 1780, 73) und - trotz bzw. wegen Quételets spekulativer Ausdeutung des "homme moyen" - sich erst allmählich verdichtende Forderung nach einer psychologischen und pädagogischen Nutzbarmachung der Statistik in inhaltlicher und methodischer Hinsicht (vgl. Sigismund 1856: 20f., Wundt 1862, XXVII, Lange 1870, 595, Dürre 1872, 1874 und St-1872) praktisch realisiert und dadurch wesentlich zur Durchsetzung einer quantitativ-empirischen Betrachtungsweise beigetragen zu haben, ohne selbst dem sich später durchsetzenden Empirismus zu verfallen. Sie erschlossen damit der Psychologie eine die Einzelfallbeobachtung und das Experiment ergänzenden methodischen Zugang insbesondere zu entwicklungs- und differentiellpsychologischen Fragestellungen, dessen relativer Stellenwert erst in der weiteren Entwicklung besser begreifbar wurde. Besonders bei den Volksschullehrern weckten sie die Hoffnung auf eine Verwissenschaftlichung der

Pädagogik und Effektivierung des Schulsystems als Voraussetzung ihrer gesellschaftlichen Emanzipation.

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FREUD'S THEORIES OF FEMALE SEXUALITY

Dr. Willem van Hoorn

The University of Amsterdam

"Full fathom five thy father
lies;
Of his bones are coral made;
Those are pearls that were his
eyes;
Nothing of him that doth fade
But doth suffer a sea-change
Into something rich and
strange."

Ariel's song;

The Tempest, I, 1

Summary

Freud's theories of female sexuality are related to some transformed aspects of Goethe's, Lamarck's and Haeckel's evolutionary theories. Moreover, basic ideas stemming from Romanticism, *Naturphilosophie* and Symbolism have to be taken into account to shed light on the Freudian notions of the man-woman relationship, the female-male antagonism and the eternal war between the sexes.

The immediate societal context of Freud's theorizing is formed by Jewish and women's emancipation around the turn of the century and during the 1920's. In connection with changing economic and social circumstances a new life-style emerged which formed a threat to bourgeois marriage.

In this paper it is argued that Freud considers human sexuality as animalistic in nature. This assumption has far reaching consequences for his theories of female sexuality. Upon scrutiny, one here discovers the equation of:

sexuality = animalistic = primitive = childish = woman

With this *fixe Idee* in mind, Freud wrote the *Drei Abhandlungen* (1905 d) and his papers on female sexuality and femininity (1925 j, 1931 b and 1933 a). The decisive factor, here and elsewhere, seems to be the mythical and socio-historical castration of woman. In Freud's theorizing this leads to vast differences in the structure of the female and male castration complexes, the formation of the Oedipus complexes and the divergent developments of the super-ego. Suffice it to remark that the present author regards most of Freud's writing about female sexuality as brilliant nonsense.

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A prelude to Descendenz-Theorie

Some twenty years ago, when I was an undergraduate student at the University of Leiden, I had to take a course in developmental psychology, the text of which consisted for the greater part of Heinz Werner's *Comparative Psychology of Mental Development*. The origins of this book date back to Werner's Hamburg years when he worked in William Stern's institute: *Einführung in die Entwicklungspsychologie* (1926). I will quote from the revised second English printing of 1957.

Werner's developmental psychology takes a comparative point of view and works with the principle of genetic parallelism. Werner maintains that the same formal principles of mental organization apply to the mental life of children, to so-called primitive men and to certain psychotics. As is well-known, in biology Haeckel, following Carus, has formulated the law of recapitulation - ontogeny is the short and rapid recapitulation of phylogeny. As Stephen Gould (1977) has amply made clear, corresponding laws for the mental development of human beings have been formulated by a.o. Stanley Hall, Sigmund Freud, Jean Piaget and Benjamin Spock. When it comes to comparing the individual's mental development with racial evolution, there is such an *embarras de tristesse* that I - within the available space - have to be very brief:

Stanley Hall: "It would be well for psychologists to postulate purely instinctive vestiges, which originated somewhere since the time when our remote ancestors left the sea, ceased to be amphibious, etc." (1897, p. 169).

Freud, 1937: "Keiner der angeblich überwundenen Irr- und Aberglauben der Menschheit, von dem nicht Reste heute unter uns fortleben, in den tieferen Schichten der Kulturvölker oder selbst in den obersten Schichten der Kulturgesellschaft. Was einmal zu Leben gekommen ist, weiss sich zäh zu behaupten. Manchmal könnte man zweifeln, ob die Drachen der Urzeit wirklich ausgestorben sind" (1937c, SA-Ergb. p. 369).

Piaget: "(For a child) one boat will flow because it is light, another, because it is heavy, etc. Logical coherence is entirely sacrificed in such cases to fidelity of fact. The causality which results from phenomenonism of this kind is not unlike that which is to be found in primitive races" (1960, p. 253).

Dr. Benjamin Spock: "Each child as he develops is retracing the whole history of mankind, physically and spiritually, step by step. After a few weeks in the womb a baby has gills like a fish" (1968, p. 229).

Heinz Werner, this great student of *Felix Krueger's* and *William Stern's*, seems to be wisest of them all: total rejection of any form of Haeckelian recapitulation in child development: "Returning to a consideration of the relation between child psychology and ethnopsychology, we find it quite obvious that any hypothesis of recapitulation has to be rejected" (1957, p. 26).

Freud's basic notion of female sexuality

This prelude to the pervasive influence of Haeckel's *Abstammungslehre* (transmutation theory), serves to familiarise the reader with the idea that in Freud's theories of female sexuality we have the equation of:

animal = primitive = child = woman

One example of "the dragons of primeval time which are still alive", should suffice. When Freud, in the *Drei Abhandlungen*, discusses inherited infantile polymorphous perverse sexuality, he equates the child's behavior with "das unkultivierte Durchschnittsweib, bei dem die nämliche polymorph perverse Veranlagung erhalten bleibt". And this same polymorphous, that is infantile and animalistic predisposition is exploited by the prostitute in her professional activities. In short, take some aspects of evolutionary ideas of Goethe, Lamarck and Haeckel together, and it becomes "unmöglich, in der gleichmässigen Anlage zu allen Perversionen nicht das allgemein Menschliche und Ursprüngliche zu erkennen" (1905d; SA-V-97).¹

a) *Human sexuality = animalistic*

From an overall point of view, Freud considers human sexuality as animalistic in nature: "Es sind vor allem die koprophilen Triebanteile (des Sexualtriebes), die sich als unverträglich mit unserer ästhetischen Kultur erwiesen" "Die fundamentalen Vorgänge, welche die Liebeserregung liefern, bleiben ungeändert. Das Exkrementelle ist allzu innig und untrennbar mit dem Sexuellen verwachsen, die Lage der Genitalien bleibt das bestimmende unveränderliche Moment. Und so ist auch die Liebe im Grunde heute ebenso animalisch, wie sie es von jeher war." Summarized in one short and catching phrase: "Anatomie ist das Schicksal".²

However, as we know from *Animal Farm*, although all animals are equal, some are more equal than others. This basic principle obviously applies to woman's predicament. Says Freud: The feminists' demand for equal rights does not reach far. Just how far thinking by way of analogies may influence theorizing can be exemplified by Freud's dictum that morphological differences between the sexes must result in differences in mental development between woman and man. It is with this *fixe Idee* in mind that Freud writes the *Drei Abhandlungen* (1905 d) and his papers on female sexuality and femi-

ninity (1925 j, 1931 b and 1933 a). The decisive factor, as we all know, seems to be the mythical and socio-historical castration of woman. In Freudian thought this leads to significant differences between the contents of the female and male castration complexes, of the Oedipus complexes and to the divergent formations of the super-ego. "Während der Oedipuskomplex des Knaben am Kastrationskomplex zugrunde geht, wird der des Mädchens durch den Kastrationskomplex ermöglicht und eingeleitet" (1925 j, SA-V-264)³ This crucial fact entails far reaching consequences for the differences between male and female maturity. What we are dealing with here, is the distinction between actually executed and merely threatening castration. In the case of the 5-year-old boy, the eternal wish to be reunited with the mother is given up under the paternal threat of castration. Then the Oedipus complex dissolves. From now on its libidinal energies can be redirected towards the promotion of higher cultural achievements. In the ideal male case the OC completely dissolves to be replaced by a strong superego, its loyal inheritor.⁴

In the case of the 5-year-old girl, unfortunately, her anatomy determines her social destiny. Since castration has already taken place in primeval time, there is no strong motive for the destruction of her OC. Actually, the little girl's Oedipus complex is a secondary formation which is built upon the mythical foundation of the female castration complex. (In parentheses I would like to add that since here, Freud exchanges scholarly insight and observation for mythical thought, he implicitly devalues his statement that the consequences of castration and penis-envy can be followed far into adult womanhood). And thus, all women must live with a weak super-ego. This is only one instance where Freud theoretically links women with little children.

b) *Adult female characteristics are childish*

Women are born with a specific castration complex. Soon after

birth, penis-envy sets in. The first love object is the mother, later to be replaced by the father. The first leading erotogenic zone is the (male) clitoris. The pre-oedipal period of the girl is long and eventful. The girl's Oedipus complex sets in rather late and will be half-baked because only in little boys we have *simultaneous* love for the mother and hate against the father. This complexity of factors explains why women are more prone to neuroses and why adult female characteristics are childish. Since neurosis consists of the strife of the ego against the claims of the sexual forces and since in a woman the Oedipus complex never dissolves, the original incestuous love object will stay with her all her life. This, as you will understand, will cause a great deal of trouble.

Furthermore, although Freud hesitates to say so "kann man sich aber doch der Idee nicht erwehren, dass das Niveau des sittlich Normalen für das Weib ein anderes wird. Das Ueber-Ich wird niemals so unerbittlich, so unpersönlich, so unabhängig von seinen affektiven Ursprüngen, wie wir es vom Manne fordern. Charakterzüge, die die Kritik seit jeher dem Weibe vorgehalten hat,

- dass es weniger Rechtsgefühl zeigt als der Mann
- weniger Neigung zur Unterwerfung unter die grossen Notwendigkeiten des Lebens
- sich öfter in seinen Entscheidungen von zärtlichen und feindseligen Gefühlen leiten lässt,

fänden in der oben abgeleiteten Modifikation der Ueber-Ich-bildung eine ausreichende Begründung" (1925 j: SA-V-265-266).⁵ Almost seventy years old and still a misogynist?

c) *The intellectual inferiority of so many women*

From his earliest scholarly work on, Freud has assumed the existence of an antagonism between intellectual and sexual activities. The two simply do not go together because the one serves the process of civilization, while the other only satisfies the *Lustprin-*

zip. In connection with these ideas, Freud holds the opinion that the indisputable fact of the intellectual inferiority of so many women rests upon the "zur Sexualunterdrückung erforderliche Denkhemmung" (1908 d, SA-IX-28).

Against this background the anatomical differences between the sexes and their consequences, get a special significance, which I will now go into.

First, as far as I know, Freud maintains nowhere that woman's castration automatically leads to a divergent female mental development. In his view, executed castration leads to the "Entdeckung" (discovery) of the inferiority of the clitoris, which is followed by life-long penis-envy. This should make it understandable "dass der Natur des Weibes die Masturbation ferner liege..." Why? Because the little girl's clitoral masturbation is male in nature. (Here, by the way, I would like to remark that it is not anatomy which determines social destiny, but it is Freud's persistent thinking in analogies which has led him astray; 'Analogie ist das Schicksal', so to speak)^{5,6}

Before a girl can become a woman, her clitoral sexuality has to be replaced by vaginal sexuality. Here social contacts with members of the opposite sex come in handy. Each time a little girl watches a little boy playing with his penis, she hears a voice which tells her that when it comes to the size of the sexual organ, there is no way she can beat the boy and his lustful experiences. In this manner, "die Erkenntnis" of the anatomical differences between the sexes forces the girl away from virility and masculine masturbation towards the full unfolding of womanhood.

'Discovery', 'seeing', 'hearing' and 'understanding' are all cognitive processes. Hence the conclusion is warranted that, according to Freud, it is the cognitive assimilation of the anatomical difference and its vicissitudes, which shapes woman's destiny. Thus conceived, cognition is of prime importance in psychoanalysis. For, to see is to know, and to know is to recognize (jedes Sehen ist ein Wiedersehen, jedes Kennen ist ein Wiedererkennen). To recognize is to *remember* that the pattern of human life was already drawn in

primeval time. All women and men do in their sexual lives is play their prescribed roles. It seems that prehistory is destiny, to slightly twist Napoléon's original words.

d) *The irreconcilable opposition of culture and sexuality, of thinking and womanhood.*

Freud's deepseated symbolism and romanticism and his adherence to parts of Goethe's, Lamarck's, Darwin's and Haeckel's evolutionary theories have thoroughly influenced his theory of female sexuality. Animalistic sexuality and refined culture do not go together. Primitiveness and polymorphous perverse sexuality are present-day manifestations of earlier stages of mankind. In a sense, uncultured female sexuality also belongs to an earlier stage of human history. In addition to this, one has to remember that in the mental life of human beings, the vestiges of memory are far more important than the impressions of life here and now. This factor is dependent upon the intellectual education of the individual and increases in relation to the level of personal culture. "Im Gegensatz hierzu ist der Wilde als das 'unglückselige Kind des Augenblickes' charakterisiert worden" (1905d, SA-V-144)⁷ From the equation of the primitive, the child and woman, it follows, that one can also characterize a true woman as an unhappy child of the moment. Once this equation is ascertained, we are back once again to the peculiar characteristics of the female Oedipus complex and all the unfavorable characteristics which for ages have been ascribed to women. At the very end of *Totem und Tabu*, significantly subtitled "Einige Uebereinstimmungen im Seelenleben der Wilden und der Neurotiker", Freud unequivocally states that the origins of religion, morality, society and art concur in the Oedipus complex.⁸ In its essence, the Oedipus Complex is a father complex and according to psychoanalytic theory, this father complex constitutes the core of the neuroses (1913, SA-IX-439)⁹. The killing of the *Urvater* must have left ineradicable traces in the history of mankind. Mythical tragic guilt is transmitted by a masspsyche (Massenpsyche) from one ge-

neration of sons to the next generation of fathers and so on. By the same token, baby-girls are born with the memories of castration which, once they have become mothers themselves, they transmit to their daughters as an acquired characteristic.

In short, the omnipotence of unconscious psychic reality determines the behavior of primitives, children, and women to an unexpected extent.

Conclusion: the antagonism of womanhood and culture.

In the final analysis then, psychoanalysis is imbued with the irreconcilable antagonisms of sexuality and culture, of motherhood and thinking. Why, may we ask, are womanhood and culture at odds?

The answers to this question are scattered throughout Freud's writings; from his letters to Fliess up to and including the Moses book. 'Kultur ist Erstärkung des Intellekts, Beherrschung des Trieblebens und Verinnerlichung der Aggressionsneigung'. The latter was recently translated as 'make love, not war'. Beautiful. Culture consists of the 'higher mental activities', such as science, art and ideology. In the last chapter of *Das Unbehagen in der Kultur* (1930a), Freud succinctly summarizes most of what I have been writing about in this paper: "... das Schuldgefühl als das wichtigste Problem der Kulturentwicklung hinzustellen" (SA-IX-260).¹⁰

This, in my view, seems to be an almost exclusively male matter, because the killing of the *Urvater* has made impossible human happiness for ever. It seems to be men's destiny to pay for the progress of culture by ever increasing feelings of guilt. It seems to be women's destiny to safeguard the survival of mankind.

Notes

1. Parts of proto-evolutionary theory (Lamarck: 1744-1829 and Goethe: 1749-1832) and evolutionary theory (Darwin: 1809-1882 and Haeckel: 1834-1919) are transformed into Freud's psycho-analytic evolutionary theories. Lamarck's idea of the inheritance of acquired characteristics is transformed into the concept of the inheritance of *mental* characteristics! From Goethe, Freud borrows notions about the *Urtypus*, metamorphosis and (romantic) symbolism. Haeckel's biogenetic law is overtly and covertly present. Darwinian ideas have been very influential (see C. Gildiner, 1983). Crucial Freudian notions like the killing of the *Ur-vater*, the differences between the female and male Oedipus complexes, the function of memory, the place and function of masturbation, the weaker female superego and the predisposition towards neurosis in the case of women, are all to be understood within the framework of psychoanalytic evolutionary thought.
2. 'Anatomy in destiny'. Freud loved this idea and connected it with Augustin's "Inter faeces et urinas nascimur". See Ms. K, 1-1-1896, the Dora Case, his paper on the most general form of degradation in love-life (1912 d), the dissolution of the Oedipus complex (1924 d), and 'Civilization' (1930a).
3. Complete Psychological Works (= CPW), 1925 j, vol. 19, p. 256.
4. Thus, in the case of males, a strong superego replaces the unconscious influences of the Oedipus complex. This notion of Freud's forms one of the most striking examples of biogenetic thinking, which for him "has nothing accidental because it represents the most important characteristics of man's ontogeny and phylogeny" (*Ich und Es*, SA-III-302-303). In all cultural acquisitions, men have taken the lead, however "gekreuzte Vererbung hat den Besitz auch den Frauen zugeführt" (ibid. p. 304). Here, in one sentence, Freud mixes mythology, analogistic reasoning and Lamarckism to a degree of utter incomprehensibility.
5. CPW, 1925 j, vol. 19, p. 257-258.
6. Freud's abundant use of analogy and his adherence to symbolistic forms of thought have thoroughly influenced his theories of female sexuality. Analogy, in a broad sense, comprehends any mode of reasoning that depends on the suggestion or recognition of a relationship of similarity or correspondence between two objects. Analogy includes the use of proportional relationships, of comparisons, e.g. the use of models (paradigms), and of images (icons). The use of symbols and symbolism in (Freudian) psycho-analysis, is still in need of careful study.

In the case of theorizing about woman's sexuality, Freud has taken male development as norm and standard. By way of analogy, female development is derived through comparison with the male. From an epistemological point of view then, basic Freudian notions about female sexuality such as:

- the masculinity of the clitoris
 - the vaginal orgasm
 - the second wave of repression during puberty
 - penis-envy
 - the weaker superego, etc.
- are in jeopardy due to the unwarranted use of analogy.

7. CPW, 1905 d, vol. 7, p. 242.
8. Taking aspects of Lamarck, Haeckel and Huxtings Jackson in this book together, Freud aims at a clarification of the correspondences in the mental life of primitive man and present-day neurotics. The natural and cultural history of the libido is related to the universal incest tabu, the antagonism of cultural evolution and the full development of human sexuality, the killing of the *Urvater*, and immortality. In view of the fact that anthropological research of the past 50 years has almost nowhere substantiated Freud's speculations, we may conclude that in *Totem und Tabu* we are dealing with psychoanalytic mythology and the libidinal labyrinth.
9. CPW, 1912-13, vol. 13, p. 156.
10. CPW, 1930a, vol. 21, p. 134.

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TWO DEBATES ON PSYCHOANALYSIS AND FEMINISM*

A. Fischer

J. Jansz

University of Leiden

*Introduction***

Few men have caused such an uproar in feminist's circles as Sigmund Freud. Some women consider Freud as the greatest patriarch of the 20th century, whereas others use Freud's psychoanalysis to analyse the subordinate position of women in modern society. In this paper some remarks will be made about the relationship between psychoanalysis and the feminist movement and about the positions taken by women in the psychoanalytic movement.

The roots of the debate on Freud's theory of female sexuality lay in the 'twenties when Freud's (female) colleagues elaborated and criticised Freud's theses about female psychosexual development. It all started in 1921 with an article written by Karl Abraham¹⁾. In this paper Abraham discussed the nature of the female castrationcomplex. Though there were several female and male psychoanalysts who discussed female sexuality, Karen Horney (1885-1952) and Hélène Deutsch (1884-1982) were the first and most important participants in this debate from the early twenties to the early thirties.

After the discussion in this period, we will shift our attention to the 'sixties and the 'seventies. After the suffragette's struggle in the early decades of the twentieth century, a second peak in feminist's activities can be seen in the 'sixties, the so-called 'second wave of feminism'. In our paper we shall focus on the relationship between psychoanalysis and the feminist movement

* This contribution is a compilation of two papers presented at the Heidelberg Conference (Eds.)

** This paper is part of a larger project. The first publication on this subject is: A. Fischer, W. v. Hoorn, J. Jansz: *Psychoanalyse en vrouwelijke sexualiteit*. Meppel: Boom, 1983; English translation in preparation. See also Van Hoorn's contribution in this volume.

in the USA and in France. These two countries have been selected because the reception and elaboration of Freud's theory of female sexuality has been remarkably different, there and overseas.

In our paper we propose some tentative hypotheses to explain the differences in the subsequent reactions towards Freud's theory. Throughout our discussion emphasis will be laid on societal factors to explain and understand the differences between the female theorists quoted in this paper.²⁾ Therefore, attention will be paid to Horney's and Deutsch's position in relation to the feminist movement of their days and to their positions in respect to the psychoanalytic in-group. Concerning more recent history we will focus upon the position taken in the debate between PsA and feminism by some female theorists in the feminist movement.

2. The 'twenties and the 'thirties: Horney and Deutsch

Introductory remarks - The positions of the female analysts Karen Horney and Hélène Deutsch got in the course of the 'twenties diametrically opposed to each other³⁾. Finally Deutsch remained a faithful adept of Freud's theories, while Horney criticised his male-biased and biological deterministic view of female destiny. She became one of the founders of the neo-analytic school in America. We will consider their opposing viewpoints on female sexuality in relation to their differing views on women's emancipation. For a better understanding of the emancipation question in this period, we will first describe some developments in the societal position of women and the discussion of women's emancipation in this period.

2.1. Societal position of women

Deutsch's and Horney's position in the academic world of men was quite remarkable, since it was rather exceptional for women to pursue a profession, especially a medical one.

From the beginning of this century the position of women had radically changed. The ongoing industrialisation process brought along two important changes in the lives of women: the change of

homelabor into factorial and industrial labor and the further division of labor in productive masculine tasks and reproductive feminine tasks. Especially for labourwomen the necessity to earn money became an extra burden, because they had to leave their houses to work. For middle-class women one of the consequences of this division of labor was a new feminine task: the education of children. These women of the new middle-class had a highly ambivalent attitude toward working women: on the one hand they were influenced by the protestant ethics according to which being unproductive was intolerable, on the other hand they were attracted by the ideals of the bourgeois and nobility: entertaining social contacts and enjoying public life. Many women found a solution for this ambivalence in the social reform movements: in this charity work they used their female talents to care for the weak and the poor. Though most women spent the greater part of their time as a mother and a housewife, the percentage of women working outside the home grew from the beginning of the century on. Before the First World War women engaged mainly in typical female jobs: agricultural labor, domestic jobs and teaching. During the First World War women joined the labour force in great numbers, to fill the vacancies left by their fighting men. Though many women were forced to go back to their stoves and children after 1918, a larger number of women than before the war retained their jobs. One of the factors which made this development possible was the result of the feminist struggle for more and better education for women.⁴⁾ This struggle started in the eighties and lasted a few decades. Among the demands were: the right to get access to the universities and to academic professions, and a better education for female teachers. Only in 1900 women were officially permitted to be matriculated at two universities in Germany; in 1908 women could be matriculated at all universities.⁵⁾

In the first decade of this century the women's movement in Germany and Austria was very influential and impressive. Besides the struggle for work, one of the main issues was women's suffrage. However, due to internal differences, the suffrage movement was

extremely weakened in the second decade and the fight to get the vote was merely a crusade for the reign of viruosity. Finally, after more than two decades of suffragettes' struggle, women got the vote in 1918.

When this political right was gained, the women's movement lost its influence rapidly. Only in the 'twenties a small group of radical women freed themselves of the restricting Victorian morals by stressing the beauty of women's bodies and the intelligence of women's minds. The so-called flappers started the first sexual revolution in Western Europe, but this did not result in the emancipation of women.

2.2. The women's movement and the early emancipation ideals

Ever since the foundation of the first women's movement different opinions on women's emancipation led to various divisions. The first division took place in 1895, when the *Bund Deutsche Frauenvereine* (BDF) was founded, in which no socialist women's parties were allowed. The main point of difference was the question of economical independence of women. According to the socialist women, the right to work was the only way to women's emancipation: women should be full members of society and become economically independent of men. Because it was necessary for most women in the socialist women's organization to earn money, they were mainly concerned with equal wages for women and men and the shortening of labour time.

According to the bourgeois women, the possibility for women to work and earn their own money was certainly not the soulsaving way to women's freedom. Though women should have the right to work, the essence of women's emancipation lay deeply anchored in the female characteristics of moral superiority, decency and virtuous motherhood. Most bourgeois women considered themselves the superior sex, because they were more decent, more virtuous and more capable of controlling their sexual instincts than men. These qualities were one of the consequences of the fact that only women were able to bring forth mankind, one of the most precious capacities a human

being could possess. The final aim of these bourgeois women was being respected as women, mothers and moral superior human beings. Their activities were directed at elevating the moral sense, and saving the world from vice, moral decadence and barbarity. Women were able to fulfill this great task, because they were endowed with the special quality of *motherliness*: the psychical capacity to give love and to nurture. So the main difference between man and women was not only the physical capacity of motherhood but also the psychical capacity of motherliness.

Though most women in the bourgeois women's movement agreed on the importance of motherhood, they diverged on their ideals of women's emancipation. In the first decade of this century the so-called radical women had a majority in the women's movement. They were organized in the *Bund für Mutterschutz* (League for protection of mothers) founded in 1905. This league especially defended the rights of unmarried mothers and their illegal children. This radical wing was often represented by Hélène Stöcker (1869-1943), president of the *Bund für Mutterschutz*. Hélène Stöcker and her colleagues defended a program, called the New Ethics, which contained a fierce attack on the existing relations between men and women in marriage and on the inferior position of women.⁶⁾ According to the New Ethics, marriage should be based on romantic love, not on unequal and forced relationships. Besides, women too had their own responsibility for their lives, and thus they should choose consciously for motherhood. Giving birth to children and educating them should not be an inevitable consequence of women's destiny; women had the same right as men to develop their individuality according to their own ideals. In the period from 1902 to 1909 the radical women questioned various issues, which were formerly taboo in the women's movement, like prostitution, abortion, unmarried motherhood, free sexual relationships, etc. In propagating these new sexual morals Hélène Stöcker quoted Freud⁷⁾ who had shown that human sexuality is not merely a matter of biology and offspring. However, psychoanalytic theory never became an issue in this women's movement and remained restricted to some

remarks of Hélène Stöcker on Freud's theories. As far as they knew anything about Freud's theories, especially women who belonged to the conservative wing in the women's movement, abhorred psychoanalysis, because of its emphasis on sexuality. Conservative women were convinced that the highest ideals of femininity were virtuous motherhood and moral superiority. Freud's judgment on female sexuality did not exactly agree with the ideals of these women.

According to one of the representatives of the conservative wing, Hélène Lange (1848-1930), a woman belonged exclusively to her family: she had to educate her children and bring them up as good and decent human beings. Besides this important task, her main duty in marriage was to conquer the polygamous instinct of her husband. Men had to be tamed by women in order to behave like civilised human beings. In contrast with the radical point of view, the conservative women considered motherhood as the highest ideal a woman could ever fulfil.

This conservative opinion on women's emancipation gained ground rapidly after 1909 (when the prohibition on political organization for women was abolished and great numbers of conservative women joined the women's organizations) and in the next two decades the activities of the women's movement were restricted to the organization of the adoration of motherhood. In the bourgeois women's movement emancipation had become the equation of virtuous motherhood.

2.3. Horney and Deutsch: two psychoanalytic models of women's emancipation?

Female analysts have been a minority in the psychoanalytic associations in the 'twenties and the 'thirties. Some male analysts were opposed to admitting women in the society, Freud however approved of the fact that female analysts entered the association and they sometimes even had leading positions.⁸⁾ Though Freud cannot be seen as one of the advocates of women's liberation, he must have tacitly agreed with the judgment of Karen Horney: "Und die Frage ist (...) wie weit nicht die weibliche Entwicklung, wie sie sich uns jetzt

analytisch darstellt, mit männlicher Mass gemessen ist und also dem eigentlichen Wesen der Frau nicht ganz gerecht wird."⁹⁾

Both Horney and Deutsch entered the psychoanalytic world in the same period, Horney in Berlin, Deutsch in Vienna. They both had an analytic practice, with mostly female patients; they were responsible for the training of young students of psychoanalysis and they both made studies of more theoretical psychoanalytic questions. They occupied the same societal position, both were married, though Horney divorced in 1926, and had children. But most important of all they were young and enthusiastic followers of psychoanalytic theory and practice and they both admired Freud, although Deutsch had a more personal relationship with him.¹⁰⁾ Obviously then their divergent scholarly opinions cannot have been determined by these circumstances. The dividing line between Horney and Deutsch on female sexuality - so it seems to us - can be explained in terms of their different views of women's emancipation.

Deutsch neither refers directly to the emancipation of women nor to the consequences of their societal role for their psychical and sexual life. Nevertheless, she considered herself as setting the tune for women's emancipation, because she had a medical education and got a leading place in the male psychoanalytic world. In her case women's emancipation had to be equated with the individual success of a woman who got herself a position. She, herself had such a position in the world of the Viennese bourgeoisie: "She was remembered from the early 1920's as a Helen of Troy, brilliant and beautiful, Freud's darling".¹¹⁾ However, her own career was in great contrast with her psychoanalytic theory of femininity, in which a woman's highest ideal was motherhood. In her analytic practice she treated mostly female bourgeois patients and in her freetime she often discussed problems with friends from inside the psychoanalytic circles. Like most of the psychoanalysts in that time her scope was restricted by her psychoanalytic view.

In contrast with Deutsch, Horney was more compassionate with women's problems outside the psychoanalytic bourgeois world. In

Berlin she worked as a doctor in several hospitals and finally she was one of the founders of the *Berliner Klinik für Psychoanalyse*, where people could get a psychoanalytic treatment without charge. She gave lectures for women's meetings, for instance the female workers movement, and her audience consisted often of young feminists who regarded her as a symbol of an emancipated woman. In her lectures she explored not only the theoretical problems of psychoanalysis, but she tried to apply psychoanalytic theory to the current problems of women.

Though Horney and Deutsch share a common background as psychoanalysts, there are some marked differences. The main difference is that they treated women from different backgrounds; consequently their observations and their doubts on Freud's theories must have been quite different. For instance Horney's doubt of the importance of penis envy rose because she treated women who had problems because of the restrictions of their societal roles, not because of their innate inferiority. Horney tried to apply psychoanalysis to current and common women's problems and not only to the women in bourgeois circles. In doing so she became aware of the restrictedness of some of the existing psychoanalytic concepts.

2.4. Deutsch's theory of female sexuality

When we consider Deutsch's theory of female sexuality, it is clear that she is keeping with Freud's approach. She elaborates Freud's theory with regard to specific feminine developments, like menstruation, deflowering, giving birth to a child, breastfeeding, etc. In 1925 she published a book, called *Psychoanalyse der weiblichen Sexualfunktionen*¹²⁾, which is the first extensive work on this subject. Later she amplifies her theories with several articles on woman's masochism, homosexuality and frigidity. According to Deutsch, female psychosexual development is much more difficult than the male development, because a woman is naturally inferior to a man. To reach a normal feminine attitude, the girl has to surpass the always present male tendencies in each new phase of her life. Thus there is no specific female development: the psychosexual development of a woman is always a derivation from the

male development. The girl has various difficult tasks to accomplish before she has reached normal femininity. First of all she has to give up her childish, male clitoral masturbation, because she has to discover her vagina as a pleasure organ. Intense clitoral masturbation is the result of penis envy. She must give up the longing for a penis and acknowledge the genital difference between men and woman; that is to say she has to accept the inferiority of her genitals. This acknowledgement of her castration is the first narcissistic injury in a girl's life and it will be followed by more. Menstruation, for example, is regarded by the girl as a punishment for her clitoral masturbation; again she is reminded of her castration. But she must constantly repress her wish for clitoral libido and masochistic tendencies have to predominate her active attitude. "Die analytische Erfahrungen lassen keine Zweifel übrig, dass die erste libidinöse Beziehung des kleinen Mädchens zum Vater eine masochistische ist, und der masochistische Wunsch in ihrer ersten weiblich gerichteten Phase lautet: "Ich will vom Vater kastriert werden". Meiner Ansicht nach gehört diese masochistische Wendung zum 'anatomischen Schicksal', ist biologisch und dispositionell vorgezeichnet und bildet die erste Grundlage zur endgültigen Entwicklung der Weiblichkeit (...)"¹³⁾

According to Deutsch, only the acknowledgement of her inferiority and her masochistic attitude can lead to normal female sexuality. Men and women are not sexually equal creatures and they never will be.

2.5. Horney's theory of female sexuality

Horney's point of view is formed by a critique of the classical psychoanalytic theories. This critique becomes more radical towards the end of the 'twenties. In 1923 when she wrote her first article about the female castration complex¹⁴⁾, her point of view is not very different from that of Hélène Deutsch. She accepts the notion of penis envy and explains it by the real inferiority of the girls genitals. But from 1926 on, her point of view gradually changes and

she begins to throw doubts upon the central role of penis envy in a girl's development. She states that the specific female characteristics are as important as the male qualities. She then explain penis envy as the result of the libidinal power of attraction between the sexes. Why should the girl see the penis as a superior organ, while she herself has biological capacities to be jealous of for men? Horney tries to equate the different biological capacities of men and women by stressing women's ability to procreation. This sometimes leads to a glorious exclamation with regard to women's reproductive role: "An dieser Stelle muss man als Frau erstaunt fragen: und die Mutterschaft? Und das seelige Bewusstsein, ein neues Leben in sich zu tragen? Und das unerhörte Glück in der sich steigenden Erwartungsspannung auf das Erscheinen dieser neuen Wesen?"¹⁵⁾ In addition to the equalization of female and male biological functions, she calls attention to the societal factors which form the major cause of women's inferiority, both as a social and as a sexual being.

In 1932 her view has changed more radically. She states that penis envy is attributed to women by men, because they are afraid of the female genitals.¹⁶⁾ It is very likely that the male fear of a woman = the mother and the fear of the female genitals is more deeply and more intensely repressed than the female fear of the man = the father. In the course of history men have imagined women to be dangerous vampires, evil witches or sly seducers, in order to hide their own fear of femininity. It is an attempt to objectify this fear. Men have made women feel worthless and inferior, because "minderwertigen Wesen Angst zu haben, wäre dich geradezu lächerlich."¹⁷⁾ The many problems women have, should not be explained by pointing to feminine nature, as Deutsch does, but by referring to male oppression. Women's sexual problems, like frigidity, are not caused by the biological differences between the sexes. These phenomena are determined to an important amount by culture and social organization. There is only one answer to the question why the vagina does not react in spite of its sensibility. Simply one factor can be stronger than the desire for pleasure and that is the fear.

In contrast with Deutsch, Horney acknowledges the female genitals as organs of full value which can stimulate and satisfy women's desires. Clitoral masturbation is not a male activity and vaginal sensations are present before puberty. Horney does not think that clitoral sensations after marriage are against women's nature. She is convinced that the vagina is consistently and consciously negated by men. "Das Unentdecktsein' der Vagina ist eine Verleugnung der Vagina."¹⁸⁾ It is the oppression of women that makes the girl see her own genitals as worthless. For centuries men have succeeded in humiliating and subjecting the female sex out of their own fear. Though Horney emphasizes motherhood as one of the most beautiful biological capacities of women, she is opposed to Deutsch who regards giving birth to a child as the ultimate satisfaction of women's masochistic desires.

2.6. Conclusion

One of the important goals of the early women's movement, whether conservative or radical in orientation, was to settle with the past, in which femininity has been associated with intellectual inferiority and physical weakness. Emancipation meant a high valuation of femininity as a qualified characteristic and this was the main issue of the feminist struggle. Horney and Deutsch extended these views to psychoanalytic circles: they both stressed the specificity of female sexual life and combatted the male degradation of the female sex.

However, like the conservative and the radical wings in the women's movement, Horney and Deutsch got opposed to each other in their differing views of motherhood and femininity. Deutsch considered motherhood as a precondition of femininity, while Horney considered it as an expression of femininity. Deutsch's view of woman as a reproduction slave even goes beyond Freud's conception of female sexuality: where Freud pays attention to social or cultural factors determining women's psychical life, Deutsch only appeals to women's biological destiny. As we stated above, Deutsch never talks explicitly about emancipation, but her ideal of

femininity is in agreement with the emancipation-program propagated by the conservative women's movement. Women and man are not equal sexual creatures, so they have different biological and psychical capacities and a normal woman should dedicate herself to her specific qualities.

On this point the differences between Horney and Deutsch become very clear. While Deutsch only talks about true femininity, Horney's ideal of emancipation comes close to the program of the New Ethics. She stresses the social and cultural factors in determining woman's inferior position. In her 1927 article about the perils of monogamous marriage¹⁹⁾ she states that women are not only economically dependent upon men, but during marriage they are also the possession of men. According to Horney, we live in a patriarchal society where men do not marry women out of love, but out of prestige. What is necessary then, is a radical reform of marriage, sexual mores, abortion and prostitution laws. In contrast with Deutsch, Horney questions female destiny. Women's subordinate position is not caused by her biology, but by male degradation and humiliation.

Consequently the many women's problems today are not caused by female inferiority or the failure to adjust herself to female tasks, but by the rigid demands imposed by society and by men. According to Horney, the solution of women's problems must not be sought in the adjustment of women's minds, but in a radical change of the structure of our male dominated society.

With this point of view Horney already inaugurates the anti-Freud period of the women's movement in the sixties.

3. *The 'sixties and the 'seventies: USA and France*

Introductory remarks - The debate on Freud's theory of female sexuality can, as far as the 'twenties and 'thirties are concerned, be characterized as a discussion between psychoanalytic specialists. The confrontation takes place within the psychoanalytic establishment. In the 'sixties and 'seventies we are confronted with a different situation. Now, the source of the

debate lies within the feminist movement. Freud's theory of female sexuality is interrogated by feminists and so is the psychoanalytic establishment. In France as well as in the USA a negative attitude towards Freud's theories dominates the debate between psychoanalysis and feminism in the 'sixties. In the 'seventies a more favorable attitude is evident in both countries. In trying to explain this remarkable shift, a glance at the societal backgrounds seems necessary.

3.2. USA

Some relevant backgrounds - At a social and economical level, the 'fifties may be considered as a period of growing prosperity. However, when it comes to the position of the American woman, one has to be less positive. During the Second World War many women were working in the ammunition plants. As soon as the war ended, they were forced back into their traditional roles as benevolent mothers and loving wives. This was not without consequences: "The problem lay buried, unspoken, for many years in the minds of American women. It was a strange stirring, a sense of dissatisfaction, a yearning that women suffered in the middle of the twentieth century in the United States. Each suburban wife struggled with it alone (...). All they had to do was devote their lives from earliest childhood to finding a husband and bearing children".²⁰⁾ These are the words of Betty Friedan who calls the problem "the feminine mystique".

One of the first issues taken up by the feminist movement in the early 'sixties is the protest against this kind of social isolation, against such a strict role-description for women: a normal adult woman must be heterosexual, married and a mother. In short: occupation *housewife*. The agitation is against the dependence of women on the income of their husbands; against the unequal payment for women. In October 1966 the National Organization of Women is founded. "The purpose of NOW is to take action to bring women into full participation in the mainstream of American society now, exercising all the privileges and responsibilities

thereof in truly equal partnership with men". (from NOW's Statement of purpose, 1966)²¹⁾. With the emphasis on action NOW takes 'the women's problem' to the field of political struggle. The organization is fighting for improvements in economical, political and legal spheres. Positive changes in the material realm will lead to improvement in the condition of women in society at large.

In this climate Friedan, the first president of NOW, writes about psychoanalysis. She considers Freud's theory of female sexuality as the legitimization of the subordinate position of women, as the ideological counterpart of economic oppression. In popularized psychoanalysis, in papers, magazines and in child-guidance books, images of how a grown-up American woman ought to be, were put forward. "(...) Truly feminine women do not want careers, higher education, political rights (...)", one had "to pity the neurotic, unfeminine, unhappy women who wanted to be poets or physicists or presidents".²²⁾ The career woman is suffering from a neurotic fighting for equality with men, that probably originates in her penis envy or her masculinity complex.

The terminology and the explanations stem from an American adapted version of psychoanalysis that does not have much to do with Freud's science of unconscious mental processes. From the day Freud brought the plague' to the USA (1909), psychoanalysis first spread in medical and educational circles, later it was half-heartedly built into developmental psychology and personality theory. Put in general terms, psychoanalysis became an applied science of which interpersonal relationships, pragmatism, usefulness and rationality of the ego became the catchwords. Harry Stack Sullivan's interpersonal theory of psychiatry and especially Hartmann's ego-psychology became prominent versions of American adapted psychoanalysis.

We would like to point at one example where the influence of psychoanalysis on the public image of woman can be seen. In the 'forties and the 'fifties the star of so-called *momism* is rising: many of youth's failures can be traced back to mistakes mother made in educating her children. With the support of popularized psycho-

analysis, *mom* is said to be the source of juvenile delinquency, alcohol addicted youngsters and so on. Motherhood is the sole identity for many women in this period, so women cannot but resign themselves to silence, guiltfully: "the feminine mystique".

Friedan criticized the societal role applied psychoanalysis has played in the USA. Kate Millet's orientation is somewhat different. *Sexual politics* is published in 1969, three years after the foundation of the organized feminist movement. Things have changed in these 'three golden years of American feminism'. The feminist movement has become divided on central issues as reproductive labour (do housewives deserve wages or not?) and, most importantly, sexuality. NOW's radical feminists' wing fights for equal rights for lesbians. To them, lesbianism is the central issue in the feminist struggle, in their own words: 'sisterhood is powerful'. Betty Friedan sharply criticizes this new orientation in American feminism: "The essence of the matter is that sexual politics is highly dangerous and diversionary, (...). No serious meaningful action emerges from a sexual emphasis. There is simply talk, anger, and wallowing. It is also based on a highly distorted, oversimplified view of our society, men and women, family relations, relations to children".²³⁾

Kate Millet's analysis of Freuds' concept - Millet considers Freud's theory as the reaction in the ideology: psychoanalysis legitimizes the subordinate position of women in Western society, especially in matters of sexuality. In order to be able to fight psychoanalytic practices and psychoanalysis's popularization, she comments upon Freud's theory of female sexuality rather extensively. She values Freud's case-studies, because they show the impact of a repressive Victorian morality on women. But Freud's interpretation is rejected: his concepts, like penis envy, are unscientific, creations of his male biased mind.

In Millet's opinion the freudian female personality is based upon penis envy. We will take a closer look at her analysis of this central freudian issue. The pertaining question she asks is: why

does the little girl prefer a penis to her own clitoris? Is bigger better? Why doesn't she consider her own genitals as smooth and normal in contrast with that awkward worm-like 'something' her little brother has got in front of his belly? Millet reproaches Freud for working from a male-predisposed view. Objective, scientific proof of women's penis envy cannot be found in his work. In pointing to biological instead of sociological factors, woman's destiny becomes inevitable, and so Freud saves the patriarchal order from critique. After discussing the Oedipus complex, which she traces back to the creative mind of Freud as well, she reaches the same conclusion as Betty Friedan: especially in its popularized version, Freud's theory of female sexuality has functioned as an ideological reaction against women who are fighting for emancipation.

3.3. France

Some relevant background - Speaking in general terms, the social position of women in France is rather different from their position in the USA. French women were granted the rights to vote in 1944, a quarter of a century later than in the USA. In the 'sixties less than a third of the female population in France had a job other than housewife, whereas this figure rises up to 50 percent in the USA. Women's organizations of a political origin have a strong basis among housewives. For instance the *Union des Femmes Françaises*, which is politically close to the Communist party, fights for equal rights for women in the family as well as in society, and struggles to improve the material conditions under which most French families have to live. The struggle of the Union and of the other political women's organizations, is fought under the banner of socialism. The idea is that in a classless society the position of women will more or less automatically, improve.²⁴⁾

In the 'forties and 'fifties, Simone de Beauvoir does not participate in any women's organization. But in her world famous book *Le deuxième sexe* (1949), she does emphasize the strong links between socialism and women's emancipation. While writing *The*

second sex she would not consider herself a feminist. She is merely a scholar, interested in the 'woman's problem'.²⁵⁾ A few decades later, in the 1970's, de Beauvoir has become one of the leaders of the French feminist movement.

In a way, de Beauvoir's position in 1949 as a female scholar who discusses the woman's problem in theoretical terms, is paradigmatic of the position taken by French feminists in the 'seventies. In contrast to the mainstream of American feminism, they do not start from a political struggle, but make use of scholarly or artistic-literary discourse. To put the Franco-American difference into one sentence: the French feminists' reactions towards Freud and his circle consist of intellectual debates *with*, rather than of a political struggle *against* psychoanalysis.

May 1968 - As already mentioned, women's organizations were of a socialist orientation after World War Two. In the 1968 May revolt things changed. Many women, mostly students, saw that their male comrades were as paternalistic as the enemy they both were supposed to fight against: the ruling class. Only one conclusion could be drawn: the struggle for the emancipation of women could no longer be considered as subsidiary to the struggle for a classless society. Women's struggle was their own struggle and had to be directed against *all* patriarchal structures, i.e. against the patriarchal order itself.

In this context psychoanalysis had to be considered. For the women of *Psychoanalyse et politique*, a radical feminist group founded in 1968, psychoanalysis is a necessary tool to analyse male-dominated language, the unconscious structures in which the oppression of women has its roots. Thus, the study of the female unconscious becomes important. Following Jacques Lacan and Louis Althusser, they consider psychoanalysis a subversive science.²⁶⁾ Freud's science of the unconscious shows that each individual is divided in itself. In each female person, the unconscious mental processes constitute a part of herself that cannot be controlled by

logical, rational thought. So the subject loses its ego, its center of rational thought, a loss of control over its own body and mind. By de-centering the subject, psychoanalysis breaks radically away from subject- or ego-dominated Western thought.

Psychoanalytic politics - In seeking an explanation for the attitude of french feminism towards psychoanalysis, several factors have to be taken into account. First, until the late 'sixties psychoanalysis was far less developed in France, compared to the large influence in the USA. In the 'fifties and early 'sixties there was not much of popularization of psychoanalytic thought.²⁷⁾ So, for French feminists there was hardly any reason to be critical of psychoanalytic practice. A second factor is the shift in attitude of the *Left* towards psychoanalysis. As already mentioned, political active women - one hesitates to call them feminists - were participating in the socialist struggle. Communist intellectuals were critical of Freud's work. For them, psychoanalysis was a bourgeois science, which preserves a nineteenth-century class-society. The experiences of May 1968 caused a change in attitude. When a conservative like De Gaulle, had won the elections in the fall of 1968, it became clear that the simple Marxist consciousness-raising formula had failed. It became apparent that in the fight for a better society, extrapolitical factors had to be taken into account. A scientific analysis had to deal with the ideological level as well. Many leftist intellectuals expected Lacanian psychoanalysis to be able to cope with the fundamental problems in politics. Until then, politics had concentrated upon the rational individual. In the tradition of structuralism, Lacan had shown that societal processes enter in what we call 'the individual'. In his opinion, the structures that constitute the individual are not easy to change, because they are for a large part embedded in the unconscious of the 'subject'. Thus people must first explore their unconscious motives and desires and get to know these, before a perspective of societal change can be formulated.

By the beginning of the 'seventies, the intellectual Left had a more favourable attitude towards psychoanalysis than ever before. Especially in the numerous radical feminists' groups which were founded in the early 'seventies. Their struggle was fought at a political as well as at a scientific level: psychoanalysis had to give answers that could be made useful in the struggle against the phallocratic order.

Irigaray's views - Luce Irigaray, a philosopher and a psychoanalyst, is one of the feminists who is involved in the intellectual debate with psychoanalysis. In *Speculum, de l'autre femme* (1974) she analyses, among other issues, Freud's theory of female sexuality. In her own words, she does nothing but reading Freud's texts. In reading Freud, she shows that Freud's theory of female sexuality is embedded in patriarchal structures of thought. To give one, crucial example: in patriarchal cultures, like Freud's and like our's, the distinction between the sexes is made in terms of a *dichotomy*, in which one term (the female/feminine) is subsidiary to the other (the male/masculine). Irigaray proposes to destroy this dichotomy. In her opinion psychoanalysis is needed to elaborate new ways of thinking about the distinction between the sexes, because psychoanalysis is fit to describe how feminine and masculine characteristics are constituted. In Irigaray's words the distinction between the sexes has to be put in terms of a *differentiation*: between the feminine and masculine, lots of varieties are possible.

In more general terms, Irigaray proposes to women to break with the patriarchal order, to destroy male dominated language and male dominated thought. In *Ce sexe qui n'en est pas un* (1977) she argues that in Western culture the symbol of the phallus has always been the standard. Not only in matters of sexuality, but also in language and the use of the symbols. In her own texts Irigaray does not conform herself to traditional ways of expressing. The essays in *Ce sexe (...)* are of a poetical nature, especially those where she points out the differences between male and female sexuality under the phallocratic order. In her view masculine sexuality is

abrupt, progresses by shocks and has a well defined end in the orgasm. Feminine sexuality is flowing, never ending and not centered in the genitals: "la femme a des sexes un peu partout."²⁸⁾ Irigaray argues that female sexuality has always been male dominated, the phallus has always been the standard. She proposes women to build their own joyful experiences.

3.4. *The USA revisited*

If we return to the difference between France and the USA, are we then allowed to conclude that by 1975 the reaction of feminists towards psychoanalysis was favorable in France and unfavorable in the USA? Not at all, such an answer would be too simple.

The feminist's movement - In the early 'seventies an important shift of attention can be seen in American feminism. Large scale political action is considered less important, emphasis is laid on discussing personal matters. *Women's Lib* is spreading all over the country in so-called consciousness-raising groups: exclusively female groups, discussing the effects of psychological oppression. Meanwhile NOW, and other organizations that can be labelled as *women's rights*, are still fighting at a large scale level against formal and material oppression. By 1970 *women's rights* runs into conflict with lesbian feminism. Many 'straight women' are afraid of losing the confidence of the public when *women's rights* identifies itself too much with lesbianism. One has to wait till 1973 before NOW takes position concerning homosexuality: in the future NOW will fight against all forms of sexual oppression and discrimination.

By the late 'seventies motherhood has become the central issue of American feminism. Put in general terms consciousness-raising had failed. Correct consciousness, i.e. to be aware of the fact that woman is oppressed, was reached by a happy few only. Fighting against discrimination in personal spheres turned out to be difficult. As a result the feminist's movement turns to the analysis of the roots of oppression. Several authors turn to an analysis of relations within the family. Nancy Chodorow is one of them.

Chodorow's theoretical contribution - The reproduction of mothering, psychoanalysis and the sociology of gender, written by Nancy Chodorow in 1978, is closely linked to the practices of the feminist's movement. As she says so herself,²⁹⁾ her books has its roots in the consciousness-raising group in which she participated. Women in this group, both mothers and daughters, talked about the relationship with their mothers or their daughters. In building her theory, Chodorow combines these personal experiences with insights and facts from the object-relational school in Anglo-Saxon psychoanalysis.³⁰⁾ In the last chapter of her book she analyses the phenomenon of motherhood at a societal level: she describes what effects the division of labour between the sexes has (had) upon the position of women.

The personal, psychoanalytic and societal orientations are needed to explain why it is always woman who acts as a mother. With the help of her reformulation of psychoanalytic theory, Chodorow shows how, in a traditional family, the girl is shaped into a mother. When a daughter is born, the mother recognizes herself as a little girl in this female creature. The daughter-baby has but one image of her mother: she is omnipotent. She supplies her with food, warmth and attention. Put in psychoanalytic terms the identification is total, the daughter cannot distinguish her mother from herself. Chodorow calls this relationship that lasts through the pre-oedipal period, a symbiotic one.

While growing older, the libido of the girl is directed more and more to her father. He reinforces her sex-stereotyped behavior: 'daddy's little girl'. The girl on her part, needs her father to be able to draw a line between herself and her mother. In the oedipal period the symbiotic relationship between mother and daughter is attacked, but will not be defeated. Mother and daughter will continue their symbiosis, separation between daughter and mother will always be difficult. As a consequence, women's relationships with men will always be secondary. In her marriage the girl will be a loving wife, but she will always have a desire for a third person with whom she can work out her symbiotic relationship she once had

with her mother. In a traditional family within Western culture, giving birth to a child, especially to a daughter, will fulfil this desire.

Even these very short remarks about Chodorow's theory show that she uses psychoanalytic terminology and interpretations. She values Freud for what she calls his main contributions to psychology: his emphasis on the role of sexuality in personality development and his impressive analysis of the significance of unconscious mental processes. However, Chodorow is highly critical of Freud's theory of female sexuality. In her opinion, Freud leans too heavily on his patriarchal presuppositions when he makes an analysis of the female personality. In doing so, he obviously goes beyond his clinical observations.

Within the ranks of the feminist movement, *The reproduction of mothering*, has become rather controversial. Some say that this renewed attention for motherhood is exactly what anti-feminists and conservatives would like to stress. In the world-wide social and economic crisis it may be noticed that woman is put back in her traditional role. Chodorow disagrees with this critique: she analyzes the way in which the female personality is moulded in order to become a mother. She does not recommend motherhood as woman's, or feminist's destiny!

From consciousness-raising to unconsciousness mental processes - As we have seen, nowadays a favorable attitude towards psychoanalysis dominates in intellectual feminists' circles. A rationale is given by the author of *Psychoanalysis and feminism*, Juliet Mitchell: "(...) a rejection of psychoanalysis and of Freud's work is fatal for feminism, (...) psychoanalysis is not a recommendation for a patriarchal society, but an analysis of one".³¹⁾ This statement stands in sharp contrast with the rejection of psychoanalysis by Friedan and Millet. An explanation for this important shift in the discussion between feminism and psychoanalysis can be found in the new orientation within the feminist movement. During the 'seventies, American as well as French feminism 'goes psychological'. At

a formal and material level successes are obtained, but the oppression of women continues. Apart from the need of discussing these psychological forms of oppression, there is a need of analyzing the phenomenon. Feminists' struggle cannot be restricted to the political level only. At the universities, especially in the USA, *women's studies* are elaborated. The broad stream of feminism which was one restricted to the political realm, is nowadays underpinned by a scientific orientation.

4. Conclusion

The gap between the 'sixties and the 'seventies on the one hand and the 'twenties and the 'thirties on the other is large. In the earliest period, there is no debate between psychoanalysis and the women's movement. The discussion about Freud's theory of female sexuality is restricted to psychoanalytic circles. However, in contrast with academic discussion, only one female analyst by the name of Karen Horney takes position at women's issues: In the recent past, however, one can truly speak about a debate between feminism and psychoanalysis. In the 'sixties Freud's theory of female sexuality is attacked by the emancipation movement. In the 'seventies, psychoanalytic concepts are used by feminists to analyse the subordinate position of women in a patriarchal society. So, in the 'twenties and 'thirties psychoanalysis is ignored by the women's movement, whereas in the more recent past psychoanalysis is an issue, pro or con, within the ranks of feminism.

How can this striking difference be explained? A first factor that has to be taken into account is the varying degree of popularity psychoanalysis had gained in society at large. In the Interbellum in Germany and Austria, psychoanalysis was neither generally known nor did it gain a large scale therapeutic practice. As a consequence Freud's theory of female sexuality was discussed only among psychoanalysts and among professionals of a medical or sexuological orientation. As we have seen, by 1960 the American public is confronted with popularized versions of psychoanalytic 'knowledge' day after day. Psychoanalysis is practised in the

therapeutic field, by educational psychologists and by counselors. Because of the image of the ideal woman popularized psychoanalysis is promoting, feminism takes issue with the freudian analysis of female sexuality: Freud's theory is attacked in a sharp way. But, if we return to the 'twenties and 'thirties, why, we may ask, the feminists' movements of those days did not question Freudian theory? As a matter of fact, organized feminism ignored psychoanalysis. In our opinion, an explanation could be found in the ideal of emancipation women were fighting for in the Interbellum.

An emancipated woman had to be female in every respect. 'Female characteristics' like high standards of morality, virtuosity, charitativeness and sensitivity were explicitly underlined by the bourgeois women's movement. Their striving for emancipation was worked out within the existing order of society, they did not have any revolutionary aims. The so-called 'revolutionary impact' of psychoanalysis in discussing sexual matters in the open did not fit in with their concept of emancipation. Neither was a theory that considered activity and passivity as psychological variables that cannot be fixated in the biological male and female respectively without problems. The societal position of both Horney and Deutsch was perfectly coherent with the ideals the movement was fighting for. As educated, professional women they were the emancipated women in persona.

Feminist's struggle in the recent past has more revolutionary aims. In the USA as well as in France, *women's lib* is fighting for a society that is totally different from the existing patriarchal one. Scientific knowledge is needed to analyse why so many women conform to their subordinate position. As is said nowadays by several feminist scholars, the answer might be found in an analysis of the way in which female-(and male-!) genderidentity is constructed within the family. This process takes place at a conscious and an unconscious level, consequently the concepts and techniques from Freud's 'science of the unconscious' are needed to properly analyse femininity and the oppression of the female in a patriarchal society.

Notes

- 1) Abraham (1921)
- 2) We try to elaborate our theses by making use of the contextualistic perspective on the history of psychology. This approach was initiated by W. van Hoorn, University of Amsterdam and Th. Verhave, City University of New York. See e.g. van Hoorn (1983).
- 3) We will only consider Horney's and Deutsch's positions in the early psychoanalytic debate (1921-1933).
- 4) See for an extensive overview of the history of the feminist movement in Germany: Schenk (1981).
- 5) Consequently Deutsch and Horney were among the first women who started a study of medicine.
- 6) See for the discussion in the bourgeois women's movement: Frederiksen (1981).
- 7) Stöcker (1916).
- 8) In the minutes of the Scientific Meeting on May 15, 1907, for instance, a discussion is carried on about Wittels's essay *Female Physicians*. Those present, like Freud, Adler, Graf, Wittels, Ranks, were convinced women had not the ability to be good physicians. "Concerning the medical profession in particular, he (Graf) thinks that women will never make contributions of importance equal to those of man since she lacks the great personal influence, the suggestive power, which in addition to knowledge, is indispensable for the competent physician". (p. 197). Freud also argues that women gain nothing by studying and that they cannot equal man's achievement in the sublimation of sexuality, but he would take it as gross inconsistency to exclude women in principle.
- 9) Horney (1926), p. 362.
- 10) See also: Roazen (1979).
- 11) Roazen (1979), p. 454.
- 12) Deutsch (1925).
- 13) Deutsch (1930).
- 14) Horney (1923).
- 15) Horney (1926).
- 16) Horney (1932).
- 17) Horney (1932), p. 7.
- 18) Horney (1932), p. 16.
- 19) Horney (1927).
- 20) Friedan (1963), p. 13-14.
- 21) Friedan (1976), p. 87.
- 22) Friedan (1963), p. 13.
- 23) Critique of Sexual Politics (1970) in: Friedan (1976), p. 165.
- 24) An overview of women's organizations can be found in Albistur and Armogathe (1977), p. 447-461.
- 25) L'Arc (1975).
- 26) Turkle (1979), see especially chapter 3 and the Conclusion for the debate on the subversive character of French psychoanalysis.

- 27) Moscovici (1961).
- 28) Irigaray (1977), p. 28.
- 29) Interview with Nancy Chodorow in 1980. Published in Amsberg and Steenhuis (1983), p. 77-87.
- 30) Cf. authors like: A. and M. Balint, Guntrip, Fairbairn, Winnicott and Schafer.
- 31) Mitchell (1974), p. XV.

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'MOLECULARISM' ET 'HOLISM' DANS LA PREMIÈRE
GENERATION DES BEHAVIORISTES

Carlo Ricci

Istituto Walden

Laboratorio di scienze comportamentali

Roma

"Il convient donc de parler de la masse totale des éléments-stimulus, provoquant chez l'homme une réaction à cette dernière d'une façon globale et unitaire. Les situations peuvent être les plus simples ou bien elles peuvent représenter le maximum de la complexité."

Summary

In this paper the criticism of molecularism which is usually raised against behaviorism is discussed.

We emphasize how the fast development of behaviorism has been followed by an enlargement of its field of study.

The analysis of the behaviorism of Watson, Holt, Hunter, Weiss, Lashley and Kantor leads to the conclusion of the unreliability of the thesis, according to which behaviorism is essentially a molecular form of psychology.

On the contrary, if we talk about a prevalence, it is the holistic attitude properly that has received a greater support from the behaviorists.

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Cette étude ne se propose pas seulement de démontrer qu'il est illégitime d'attribuer une attitude réductiviste au behaviorisme, mais elle veut aussi affirmer que, si l'on veut vraiment parler de la prédominance d'une attitude par rapport à une autre, alors il faut dire que la plupart des behavioristes ont suivi celle de l'holisme.

Avant d'indiquer de quelle façon et dans quelle mesure, les différents behavioristes de la première génération ont soutenu des thèses réductivistes ou holistes, il faut tout d'abord donner une définition à ces deux concepts: réductivisme et holisme.

Le terme réductivisme se rapporte à l'attitude du savant qui se distingue dans la recherche des unités fondamentales qui seront considérées comme une unité de mesure dans l'étude du comportement. Selon Meazzini (1980), l'attitude réductiviste se base sur deux aspects fondamentaux:

- "1. la détermination des unités fondamentales du comportement;
2. la découverte des modalités grâce auxquelles ces éléments s'unissent en donnant lieu à des unités toujours plus complexes et plus significatives" (Meazzini 1980, p. 94).

Selon Phillips (1980), le concept d'holisme peut avoir trois différentes significations: holisme 1, holisme 2, holisme 3. L'holisme 1 se base sur l'association de cinq thèses se rapportant aux totalités organiques, à savoir:

- "1. La méthode analytique, standardisée par les sciences bio-médicales, n'est pas adéquate lorsqu'elle s'adresse à certains cas tels que: un organisme biologique, la société ou la réalité même dans sa totalité.
2. La totalité est plus que l'ensemble des parties qui la compose.
3. La totalité détermine la nature des parties qui la compose.
4. L'on ne peut comprendre les parties si ces dernières sont considérées séparément par rapport à la totalité.
5. Les parties sont dynamiquement liées les unes avec les autres ou elles sont interdépendantes." (Phillips 1980, p. 15).

L'holisme 2: "affirme que la totalité, même après être étudiée, ne peut être expliquée en se rapportant à ses parties (...).

L'holisme 3: affirme qu'il est nécessaire d'avoir des termes qui se rapportent aux totalités ainsi qu'à leurs propriétés." (Phillips 1980, p. 37).

Notre but n'est pas celui de mettre en question si l'holisme est ou n'est pas valable, et pour approfondir ce sujet, il faut se reporter à l'essai de Phillips même (1980) ainsi qu'aux ouvrages de Bertalanffy (1971a) et Emery (1974).

Sur la base des définitions qui ont été proposées, il nous semble que l'on puisse décerner un "continuum" où d'un côté l'on retrouve l'attitude réductiviste et de l'autre l'holisme ¹. Il est intéressant de remarquer que si l'on tient compte de ce genre de définition, il en résulte que même les théoriciens de la Gestalt peuvent être classés d'une manière différente (1). Il faut maintenant chercher à classer dans le cadre de ce "continuum" les différentes auteurs qui seront examinés.

Selon Lazzeroni (1966), ce n'est pas seulement à partir du "Manifeste Behavioriste" de Watson que se sont formées des orientations totalement différentes entre elles, mais déjà en 1925, en se rapportant à une étude de A.A. Roback, l'on pouvait décerner vingt orientations différentes dans le domaine du behaviorisme. Il est évident que certains de ces derniers ont exercé un rôle prioritaire sur les développements successifs du behaviorisme ainsi que sur la psychologie expérimentale dans son ensemble, alors que d'autres n'ont pas eu une influence aussi importante. Suite au critère susmentionné, ce rapport va maintenant examiner les contributions qui ont été apportées par les savants suivants du behaviorisme: J. Broadus Watson (1878-1958), Edwin B. Holt (1873-1952), Walter S. Hunter (1889-1953), Albert P. Weiss (1879-1931), Karl S. Lashley (1890-1958), Jacob R. Kantor (1888).

En examinant chacun des auteurs susmentionnés, notre attention ne sera penchée que sur ces aspects qui se rapportent le plus au sujet faisant l'objet de cette étude.

Il est évident que la première orientation à examiner est celle de J.B. Watson, "reconnu comme le fondateur du behaviorisme", (Curi 1966, page 11). Dans l'essai de Watson: "Les aspects fondamentaux du behaviorisme" (Meazzini 1979, pages 76-96), il nous semble de décerner trois éléments représentant l'attitude réductiviste:

- (1) La réduction de tout problème en des termes plus simples.
- (2) Compte tenu d'un stimulus, toute réaction qui en découle, ne serait ce qu'une sécrétion glandulaire ou une contraction musculaire, peut être considérée comme une unité de mesure de la réponse.
- (3) Même lorsque la réponse est provoquée par une "situation stimulus", elle peut quand même être rapportée à un groupe complexe de stimuli.

L'affirmation de ces assomptions semble confirmer la présence d'une attitude réductiviste chez Watson et, malgré sa flexibilité en ce qui concerne le choix de l'unité de mesure qui pourrait se relier à un moment donné à des aspects molaires du comportement, l'on ne peut se passer de classer Watson dans le cadre de l'attitude réductiviste.

La chose qui nous intéresse le plus, compte tenu du but de cette étude, ne concerne que l'aspect théorique et la "flexibilité opérationnelle" de Watson, qui a d'ailleurs été soulignée par Meazzini (1979), n'implique guère une adhésion aux thèses holistes. Au contraire, ces dernières ont caractérisé la contribution apportée par d'autres auteurs, comme nous allons voir par la suite dans cette étude.

E.B. Holt, philosophe et expérimentaliste, a suivi la proposition behavioriste de Watson, tout en restant fidèle au "réalisme épistémologique" qui le porta à refuser la thèse de Watson selon laquelle il fallait mettre la conscience entre parenthèses; selon Holt, elle devait être analysée, ne serait-ce que comme donnée extérieure.

Dans son essai de (1931): "Animal drive and the learning process", il énonce son crédo behavioriste. C'est justement dans cet essai que l'on retrouve les aspects caractérisant l'attitude de l'auteur en nous disant s'il adhère ou moins au réductivisme:

- (1) L'irréductibilité de tout problème en unités simples ou élémentaires.
- (2) L'unité d'analyse ne peut être significative que lorsque elle se présente comme une action ayant un but.

- (3) Le behaviorisme molaire doit être considéré comme une organisation complexe, étant quelque chose de plus que l'ensemble des éléments qui en détermine sa composition et il représente une qualité "émergente".
- (4) L'organisme répond comme un tout, même lorsque ce n'est qu'une de ses parties qui interagit.

Ces quatre éléments, que l'on retrouvera entièrement chez Tolman, indiqueront d'une manière évidente que Holt accepte sans aucun doute certaines thèses holistes, c'est pourquoi il peut être classé dans l'holisme 2 dans le cadre du "continuum".

W.S. Hunter, dont le nom est lié à la proposition de remplacer le terme psychologie avec celui "d'anthroponomie", ayant une orientation fonctionnelle, a adhéré au behaviorisme en lui apportant une contribution significative du point de vue philosophique.

Hunter avait pris une position de critique à l'égard de l'attitude réductionniste (entre autres, cette dernière n'a rien à voir avec le réductivisme cf. Mahraba, 1977; Meazzini 1980), une attitude tendant à considérer une discipline comme fondamentale (Mahraba 1977), qui s'identifiait dans la position que Watson avait prise pour la physiologie.

Hunter (1952) tend à poser l'accent sur les aspects qui distinguent l'étude de la physiologie par rapport à la psychologie, en trouvant ainsi trois aspects fondamentaux:

- (1) La psychologie traite le comportement extérieur, qui peut être observé, alors que la physiologie s'intéresse des "comportement sous-cutanés".
- (2) La psychobiologie s'intéresse surtout au milieu interpersonnel et social, alors que la physiologie concerne le milieu physique.
- (3) La psychologie étudie les "fonctions extrinsèques", la physiologie les "fonctions intrinsèques".

Nous avons voulu mentionner la position que Hunter avait prise pour la distinction entre psychologie et physiologie pour y retrouver tout simplement des éléments pouvant définir l'approche que Hunter a voulu adopter à l'égard du phénomène réductiviste.

D'après la définition de la psychologie en tant qu'étude des fonctions extérieures, il s'ensuit qu'il faut refuser la proposition de Watson de choisir comme unité d'analyse les sécrétions glandulaires ou les contractions musculaires qui, selon Hunter, appartiennent à la physiologie.

En résumant la position de cet auteur, l'on peut constater trois caractéristiques fondamentales:

- (1) Bien que l'on puisse décomposer les problèmes en unités simples, ils ne peuvent pas être des réactions physiologiques (mouvements de corps, sécrétions glandulaires, etc.).
- (2) Il faut examiner les résultats du comportement pour lui donner une juste signification.
- (3) Les unités molaires sont aussi représentées en tant que "dimension sociale".

Sur la base de ce qui a été dit, Hunter peut être classé dans l'*holisme* 2 dans le cadre du "continuum".

K.L. Lashley, élève de Watson, est sans aucun doute l'un des plus grands représentants de la neuropsychologie moderne. La plus grande contribution qu'il a apportée à trait à sa recherche sur les rapports existants entre les processus neurocorticaux et l'apprentissage.

Suite à ces études, il a mis en évidence deux principes qu'il a respectivement définis le principe de *l'action de masse* et le principe de *la potentialité équitable*. Les conséquences de ce discours sont résumées de la manière suivante par Lashley même dans son étude "Les processus du cerveau et l'intelligence" de 1929:

- (1) "Le processus de l'apprentissage et l'acquisition des habitudes ne dépendent pas d'un changement structurel étant bien localisé à l'intérieur de l'écorce cérébrale. (...)
- (2) La contribution apportée par les différentes parties d'une zone spécialisée ou de l'écorce cérébrale dans son ensemble, dans le cas de fonctions non localisées, est égale du point de vue qualitatif. Il n'y a pas de signe sommatore des différentes fonctions, mais une fonction dynamique, n'étant pas spécialisée, du tissu comme un tout unique;

- (3) L'analyse de l'habitude du labyrinthe indique que sa formation intéresse des processus caractérisant le comportement intelligent. Il s'ensuit que les résultats obtenus avec le rat sont généralisés à l'égard de la fonction cérébrale dans l'intelligence. Les données sur la démence humaine indiquent une situation ressemblant à celle que l'on retrouve chez le rat après une lésion cérébrale;
- (4) Les processus d'intégration doivent être recherchés dans les rapports dynamiques entre les différentes parties du système nerveux, plutôt que dans une différenciation structurale détaillée." (Lashley 1979; page 215).

L'on ne peut donc classer Lashley que dans le cadre de l'*holisme*.

A.P. Weiss, qui souligne la nécessité d'approfondir encore plus les bases philosophiques et épistémologiques du behaviorisme (Weiss, 1924), arrive à élaborer une "théorie physique du comportement" (Curi, 1967, page 89).

Etant insatisfait du caractère incomplet des termes psychologiques et physiologiques, Weiss pense que ce problème peut être résolu en s'adressant aux sciences physiques et en réduisant les phénomènes psychologiques à l'intérieur de ces dernières. L'étude du comportement se réduit à la description des éléments suivants:

- "(1) Stimulus biophysique
- (2) Réponse biophysique
- (3) Stimulus biosocial
- (4) Réponse biosociale" (Curi 1966).

La conception selon laquelle la réalité se développe le long d'un "continuum" de l'électron aux nations et encore au-delà (Meazzini 1981), porte cet auteur à accepter le principe d'une hiérarchie entre les différents niveaux qui pourrait s'approcher à l'une des théories holistes; mais abstraction faite de cet aspect, il doit être classé parmi les réductivistes.

J.R. Kantor n'est pas seulement fameux grâce à son adhésion et à sa nouvelle formulation de la psychologie (Kantor 1973), mais aussi car il a donné naissance à l'interbehaviorisme (Kantor 1958).

Suivant Lazzeroni (1966), Kantor accepte le sens que Watson donne au concept du comportement, en tant que moyen pour affirmer l'activité psychique en de termes concrets, et il est aussi prêt à accepter le langage S-R comme l'un des éléments de l'analyse du comportement; cependant, il continue à souligner que toute activité du comportement (psychique) est toujours le produit d'une réaction de l'organisme, en entendant par organisme un "tout".

La formation d'une psychologie se rapportant ouvertement à l'organisme et étant clairement holiste, fait classé Kantor dans le cadre de l'*holisme* 1.

Conclusion:

Etant donné que le behaviorisme n'a jamais eu, dès son début, une "connotation doctrinaire bien précise" (Nagel 1966, page 480) et que des positions théoriques fortement diversifiées y ont été introduites en s'y consolidant (2), il faut donc considérer comme illégitime la critique selon laquelle le behaviorisme a été essentiellement une psychologie réductiviste (Hamlyn 1957, Bertalanffy 1971/b, Titone 1973).

D'après une analyse détaillée des premières études sur le behaviorisme, il en ressort que l'attitude réductiviste n'a pas été prédominante, mais au contraire, la psychologie behavioriste a prêté une plus grande attention aux thèses holistes, surtout du point de vue théorique.

Notes

- (1) Selon l'analyse proposée par Weimer, historien de la psychologie, l'holisme 1 pourrait être associé aux savants de "l'école de Lipsia", guidée par Felix Krueger (...), Karl Sander et Hermann Volkert, qui suivaient une psychologie génétique selon la tradition de l'évolution de l'organisme. Ils avaient défini leur conception 'la psychologie de Ganzheit' ('holiste')(Weimer 1979, page 287). Au contraire, des auteurs plus fameux tels que Wertheimer, Koffka et Köhler ont suivi une orientation différente. En effet selon Weimer, ces derniers, "même s'ils étaient considérés comme des anti-associationnistes, ils ne l'étaient vraiment qu'en partie." (Weimer 1979, page 287).
- (2) Jusqu'au point où l'on demande si le behaviorisme n'a jamais représenté un paradigme pour la psychologie expérimentale. (Warren, 1971, Briskman, 1972).

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PSYCHOLOGICAL CLASSIFICATION AND
NEW PERSPECTIVES OF SCIENTIFIC METHODS

István Magyari-Beck

Budapest

Summary

In this paper a new hypothesis concerning the classification and development of scientific methodology is presented. Emphasis is laid upon the importance and significance of psychology for both the classification and the development of the methodology under discussion. In the author's view, methodologically speaking, psychology has not only been influenced by other sciences, but psychology has exerted an influence upon other sciences as well. The methodologies of pre-positivism, positivism and post-positivism, are shortly examined. Some notions of Bergson, Lewin, Ruzsak and Gordon are discussed to point to a new direction in scientific methodology.

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In this short contribution I would like to present a new hypothesis concerning the *classification and development* of scientific methods. Furthermore I would like to show the *importance and significance* of psychology for both the *classification and the development* of the methods under discussion. By way of introduction: if I am right we could *speak not only of the influence of other sciences on psychology but of the fundamental influence of psychology on other sciences as well.*

Let us first present our classification of scientific methods. This classification will have two main characteristics. First it will be based on the *psychological notion* of scientific methods, and secondly it will also contain a *historical principle*. Well, what is the scientific method from our psychological point of view? *The scientific method seen from the psychological standpoint is "simply" the reduction of brain work to the more simple form of its acting.* To accept this definition we have to remember that the human brain is "designed" for the complex work which aims at the

survival of human beings. Among others, this complex work contains emotions, intuitions, attitudes and so on. Logical functions are only a part of the work in question.

Thus the scientist is faced with a difficult task: how to exclude his rich mental world and to work only in the service of the discovery of the new truth. Every culture has its own special "spiritual practice". Here we find one of the main characteristics of the scientific culture and the scientific profession as compared to art, for example, in which we use all of our capabilities. I just wrote that this restriction is characteristic of scientific culture... Indeed one must exclude one's rich mental world not only in "doing" science, but in "consuming" it as an outsider or learner as well. From the abovementioned characteristics follow not only the difficulties of the scientific job, but the special scientific ethic as well. This is quite comprehensible because the ethical orders appear as general answers to the problems and difficulties of behavior in any case. As a matter of fact the difficulties of scientific work are connected with the rules according to which in the sciences we have to work rather under our human level and not on or above it. A number of great scientists have pointed out that a scientist has to work in a self-restraining way, control his imagination, etc.

I think that scientific methods could be classified on the basis presented above. Namely, *they can be put in a rank order from the point of view of their closeness to the complex brain work*. The most complex methods such as *observation, conversation, etc.*, will stand on top of the scale. On the other hand, the least complex, the simplest methods such as *mathematical* one's will stand at the bottom. Between these two ends or extremes such methods as different kinds of experiments will be found. Here I must prevent a misunderstanding. The fact according to which experiments and mathematics have much more explicit rules than observation, for example, does not mean that experiments and mathematics form a more complex method than observation. Inversely, the more rules, the more simplicity of methods. But why? Because the sets of rules show

only the number of the latter which must be used in stead of the instinctive use of innumerable "rules" of the human brain. And the more simple a method is, the more complete the set of its rules can be. The set of totally complete methodological rules can be defined as an algorithm. The first mathematical characterization of scientific methods is incorporated in this classification in as much as the rank order is a kind of mathematical scale and measurement.¹

The long term development of scientific methods in the past was a gradual estrangement from the complex level. It is important to mention here that the automation of scientific work by computers does require such simplification of methods. In this respect scientific work behaves as other types of human work; like industrial jobs for example.

Perhaps the most prominent fact of our days is the discovery of the usefulness of human complexity. It means that *the direction of the development of methods changed "yesterday"*. Bergson's *intuition*² marked only the prelude to this new movement. The "natural experiment" of Lazursky³, the action research of Kurt Lewin⁴, the gnosis of Theodore Roszak⁵ - constitute the new branch of complex scientific methods. Not to speak of a number of so-called creative technics, developed by psychologists. Such a method is the synectics by Gordon⁶, for example. A number of new methods resulted from the theory of *decision-making*.⁷ I do not intend to classify these proposals here. It is enough for me that these new proposals are based on the very complex human nature. They exploit much more human capacity than traditional scientific methods. It is as if the historical line of methods decided to make a sling and returned to the original human level.

Because of this "circulation", the scientific job has come nearer to other jobs. Nowadays, from time to time a social scientist reminds us of an inquirer or a detective. Especially when he or she makes use of "participant observation". Or a social scientist may remind us of a *journalist* when he or she *interviews* people "from the field". Recently, J.L. Mangham added to the *police-model* and the *journalist-model* of the social scientist the *model of thea-*

trical director in his *methodological theory of dramatism*.⁸ Step by step we must take notice that in our days the sciences, humanities and the arts have come nearer to each other. The new line of scientific methods is also responsible for this.

At least three stages of methodological development can be separated or differentiated in the history of European science. Namely: *pre-positivism*, *positivism* itself, and *post-positivism*. The *positivism* in our approach is definable as the use of the simplest methods which at the same time are very usable in the process of verification, because these methods are founded on the most common - mostly sensory - abilities of people. This is why we shall find great differences between *prepositivism* and *post-positivism*. The *post-positivist* set of scientific methods by all means contains the rigorous methods of verification too. But in these days we do not reduce any more our *explicit* methodology prescriptions to the simplest sensory procedures of data gathering and verification. These statements are very important as a number of contemporary "scientists" lay claim to be advanced thinkers only by returning to the *pre-positivist* way of investigation.

What is the motive force of the development described in these pages? Generally speaking, *the discrepancies between the subject matters and the methods set the development of the latter in motion*. In the first stage the investigation of nature caused the simplifying of methods. On the other hand, the new line of development of scientific methods is connected with the term of "scientism". (As the reader has already noticed in this contribution, I use the term "science" in the broadest sense. I apply it not only to the natural sciences). However, if we are speaking about scientism, we mean the application of the more simple methods to the complex subject matters. That is the application of methods at the bottom of the above scale to the topic which must be approached by more complex means. This is to say that the new line of development of scientific methods has established itself when the investigation of the human sphere has become the scientific one. *Such a topic is for example, psychology, i.e. brain-work itself*. As the structure

of method must be isomorphic with the structure of its subject matter, we cannot do without complex methods in psychology. (Of course partial problems can be approached only by simple partial methods. At the beginning of our century the whole of psychology could be divided from the point of view of the complexity of methods which were in use. Behaviorism used the simplest, psychoanalysis the most complex methods. Gestaltism stood - in this respect - in between them. Today we already use a hierarchically ordered system of methods depending on problems inquired. The simpler an inquired mental function is, the simpler can the method used by us be. It is true that the discovery of *perceptual defense* put an end to the vulgar view in psychology according to which the basic levels of the human psyche are independent from the upper one's. Instead of this we must accept the thesis of Theilhard de Chardin⁹, according to which things are determined from the top level of their system.

One of the main suggestions of my contribution is, that among others, it was psychology that inaugurated the abovementioned revolution in the realm of scientific methods. But the specific behavior of psychologists - as contrasted with other scientists in the human sphere - was that at first they wanted to conform to the natural sciences. However, this was not successful. This failure reversed the direction of methodological development; this time once and for all. This revolution and not the abortive adaptation to the natural sciences is the main contribution of psychology to the development of scientific thinking. Here we can find the encouraging role of psychology in the development of the scientific methods as a whole.

Notes

1. Stevens (1951).
2. E.g. Bergson (1907).
3. Mjasziscsev (1966).
4. E.g. Lewin (1946).
5. Roszak (1974).
6. Gordon (1961).
7. Kaufmann (1968).
8. Mangham (1983).
9. Chardin (1955).

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ROOTS OF COGNITIVE SCIENCE: THE

CARTESIAN PROGRAM*

Dr. T.C. Meijering

Dept. of Philosophy

Nijmegen University

Nijmegen-The Netherlands

Dept. of Philosophy

University of California

Berkeley, California-U.S.A.

1. *Introduction*

Using the methodology of scientific research programs¹ as an effective tool for historical analysis this paper reconstructs the emergence of information-theoretical conception of perception in the history of psychology in terms of an enduring 'Cartesian' research program whose hard core² and positive heuristic³ determined in advance various crucial developments in the theory formation of empirical psychology during the 17th century, partly inspiring and partly constraining the problem-solving capacities of individual scientists working in the field.

2. *Keplerian dioptrics and Cartesian mechanicism*

Kepler's theorizing, in optics no less than in astronomy, was clearly inspired by an implicit mechanistic conception of nature⁴. The apparent success of Kepler's dioptrics, however, should not make us blind for the tremendous theoretical problems which the (Cartesian) adoption and elaboration of mechanicism as a comprehensive research program brought in its wake in the field of philosophy, epistemology and theoretical psychology. Indeed, the transition to such a novel conception of the world would require,

* the completion of this paper has been made possible in part by a grant from the Netherlands Organization for the Advancement of Pure Research (Z.W.O.)

in the first place, a radically new scientific *metaphysics* involving a fundamental critique of the traditional notion of substantial forms while replacing it with the idea that, appearances to the contrary notwithstanding, physical reality is exclusively characterized by either purely geometrically determined matter-in-motion or else by intrinsically unchangeable atoms which, colorless and odorless, without heat or sound, whirl around in an infinite empty universe. Furthermore, it would necessitate an entirely new *theory of knowledge* according to which knowledge is a reconstruction, and no longer a copy, of reality. And finally, in close connection with the foregoing, a new *theory of mind* would have to be formulated according to which the mind is an information-processing device capable of autonomous organization of sensory inputs. Especially this latter transition will concern us here.

3. *Negative heuristic of the Cartesian research program. Dualism of thought and sense. Descartes' information theory not a cognitive theory of perception*

In Descartes, in particular, the introduction of the mechanistic hypothesis and its philosophical elaboration would seem to require the development of a full-blown information-theoretical conception of the mind in order to bridge the gap, now exposed, between the mechanical input of a strictly neuro-physiological kind on the one hand and certain mental responses in the form of adequate sensations on the other. However, as we shall see, the intellectual leap to such a novel and, indeed, revolutionary conception of the human mind proved much too great to be taken at once. At any rate, for the time being this conception is blocked by the negative heuristic of the mechanistic program itself, viz., the *Cartesian dualism of sensing and thinking, of seeing and judging*. Put another way, we could say that what later on in the history of psychology used to be distinguished as sensation and perception Descartes and his followers still regarded as indistinguishably identical.

According to Descartes the mind is pure and ever-active thought or consciousness. The body, by contrast, is no more than

extended matter-in-motion. There is nothing 'in between'. The interaction between the two substances is thus immediate, there being no vehicle for interaction that could bridge the gap as it were. Consequently, there is not room for a *genuinly* information-theoretical account of post-retinal or post-terminal interpretive processes of a subconscious character.

In the following I will examine how the Cartesian doctrine of the absolute distinction between thought and sense dominated, and *constrained*, the post-Cartesian development of the theory of perception. Accordingly, perception had to be interpreted in terms of purely passive sensations without the intervention of 'ratiocination' or any quasi-intellectual processes. To what profound extent the history of the theory of perception after Descartes has struggled with this conceptual constraint built into the mechanico-representationist research program and in fact *blocking the rise of a genuinely autonomous psychology of perception* most clearly transpires from the interesting theory of perception which was developed by Malebranche (1638-1715). A detailed discussion of his views may thus serve as a representative test case of my rational reconstruction of the relevant historical episode.

4. *Malebranche and the Cartesian research program into the theory of vision*

4.1 *Ambiguities in Descartes' theory of sensory judgement. Lack of a genuine (cognitive) theory of information processing*

During the 17th and the 18th centuries the psycho-physiological problem of information processing was principally focussed upon the problem concerning the perception of distance and magnitude. As a result of ongoing research and theoretical activity particularly in this area the distinction between seeing and judging was developed and sharpened considerably in due course.

Descartes mentions four 'means' or cues of visual distance discrimination⁵. They are: (1) accommodation, or information concerning the "shape of the eye"; (2) convergence; (3) clearness and luminosity; (4) collateral information concerning actual dimen-

sions. But Descartes' theory is not so much a theory of information processing in the proper sense but at best a psycho-physiological correlation theory without a detailed insight into the nature of the information-handling processes. Clearly, Descartes is very reluctant to allow any genuinely mental activity regarding the transformation from pineal patterns to conscious sensations. All he is willing to admit are immediate and naturally determined transactions between body and mind.

Descartes' theory of judgment, however, is steeped in considerable ambiguity and even inconsistency. For on the one hand he envisages appearances (or ideas as immediately apprehended) as in no way tinged by the influence of judgement so that appearances are infallibly true *considered in themselves*⁶. Thus, in the realm of appearance what seems, is. And it is only in judgement that we may go awry. For judgement involves an *aliquidamplius* taking us beyond the realm of pure appearances, or "clear perceptions"⁷. However, one of the main themes of the *Dioptrics* is precisely that judgement also determines what objects (their size and distance) will appear to us to be⁸. Thus, it would appear, the 'pure' data of sense are also far from virginal and, consequently, they are liable to vice, the vice of error in this case. But this would seem to contradict Descartes' theory of sensory illusion as something never residing in phenomenal facts alone.

Furthermore, if "seeing distance" involved the use of "natural geometry", how is it that lower animals incapable of the art of reasoning can "see" objects at a distance?

Again, if the "reasoning" allegedly involved in perception is incorrigible and belongs to the province of natural belief rather than to that of pure thought, it would seem altogether misleading to speak of "judgment", of "natural geometry ... like that made by surveyors"⁹ and of a "grade of sensation ... clearly depend(ing) upon the understanding alone"¹⁰

These terminological confusions, however, result from Descartes' attempt to tackle the momentous problem - indeed, he was the first one to do so in an informed way - of how we can come to

know the independently real world *indirectly*, that is, by means of appearances that are distorted relative to the cerebral patterns from which they have been derived. As a result of the union of body and mind our immediate apprehensions of sensory information not only differ from the peripheral images in the organs of sense but also from the cerebral images in the pineal gland, the seat of the *sensus communis* where all available information concerning the external world comes together. And yet sense experience is essential to our knowledge of the world. An adequate theory of knowledge, therefore, must account for the representative character of the data of our immediate awareness. And this will require a detailed theory of the processing of sensory information. Descartes only laid the groundwork for such a theory. As we will see, it was left to Malebranche to develop the theory considerably and to disentangle the various confusions we have noted above.

4.2 *Malebranche's theory of visual distance discrimination and of apparent magnitude*

Descartes only provided a theory of information correlation, not a theory of information processing. Now Malebranche, in sharp contrast to Descartes, appears to be acutely aware of the intolerable lack of a detailed information processing theory in the Cartesian theory of perception. He offers approximately the same list of means ("moyens"), or cues, of visual distance discrimination as Descartes had given. He explicitly adds a further cue, viz., interposition, or the number and kind of intervening objects. But Malebranche does not call visual distance discriminations based upon these 'means' the result of 'seeing' but of 'judgement'. However, while he sometimes uses the expression "natural judgement", he also speaks of "compound sensations"¹¹. This terminological ambiguity, which we will shortly examine, will prove to be highly significant.

However, deferring for the moment the problem of the exact nature of the judgements allegedly involved in visual distance discrimination we must first summarize Malebranche's even more interesting observations concerning the visual perception of magni-

tude. Even apparent magnitude, he claims, is estimated or inferred. This, according to him, explains the moon illusion. Thus, using the cue of interposition we arrive at differential judgments of visual distance for the horizon moon as compared to the moon in mid-heavens. Consequently, the horizon moon appears to be much larger.¹²

4.3. *Regis contra Malebranche's information theory of perception.*
Corroborated empirical excess content of the Cartesian program according to Malebranche

No doubt this explanation of the moon illusion according to which a mental estimate of distance should affect the actual perception of magnitude is both shocking from an epistemological point of view and revolutionary from an information-theoretical point of view. In fact, Malebranche acknowledged as much.¹³

Not surprisingly, Regis objected in his *Système de Philosophie*¹⁴ that an astronomer's knowledge of the moon's actual distance and magnitude by no means alters his perception of its apparent magnitude. Consequently (he argues) the psychological cause adduced by Malebranche is ineffective. Instead one should trace the moon illusion to the varying size of the retinal image projected by the moon in its various positions due to differential refraction.

This theory was not original with Regis. Malebranche already observed in the *Recherche* that this was the common view shared by "un très-grand nombre de philosophes"¹⁵. However, in that same passage Malebranche had also effectively refuted the opposing view by arguing that refraction does not affect the size of the retinal image but only the apparent elevation of celestial bodies above the horizon.

But in his *Réponse à M. Regis* Malebranche seizes the occasion for a positive defense of his own theory. He especially elaborates, and even sharpens, his view that all visual sensations are effected by judgments of distance. The apparent magnitude even of objects in our immediate environment requires the operation of complex post-retinal information-handling processes. A dwarf, two paces from us,

certainly appears much smaller than a giant, three times as large, who is at six paces from us, *even though the retinal images projected by them are equal in size*. Since, therefore, the inequality in the apparent magnitudes cannot be traced to the size of the retinal images, it must arise from the perceived inequality in the apparent distances¹⁶.

Malebranche goes on to show that in contrast to the opponent's research strategy the Cartesian research program can claim what would, in modern terms, certainly qualify as 'corroborated empirical excess content'¹⁷. He describes an experiment which optically isolates luminous objects in the field of view. When a glass blackened with soot is thus used to observe the horizon sun or moon the latter appear to be no different in size than when viewed in the meridian¹⁸. These novel effects are precisely anticipated by the Cartesian information-theoretical account as developed by Malebranche. They thus help to qualify that research program as a progressive one¹⁹.

4.4 Tensions between the positive and the negative heuristic of the Cartesian research program. The negative heuristic at work in Malebranche's theorizing.

Even more important in the present context, however, is Malebranche's reply to Regis' objection that an astronomer's knowledge of actual distances does not effect his apprehension of apparent magnitudes. For this reply touches upon the question, essential to our present discussion, as to the nature of the judgements allegedly involved in ordinary perception. As against Regis Malebranche emphasizes the important point that in order to exercise any influence upon our perceptions of magnitude *distance must be actually perceived by, or at least impressed upon*²⁰, the senses²¹. Now the reason for this is the absolute distinction between sensible as opposed to merely intellectual knowledge with respect to their origin (or their psychological aspect) as well as their validity (or their epistemological aspect). For sensible effects are produced in virtue of the laws of the union of body and mind. But

intellectual knowledge is gained through free activity of the mind alone.

Clearly, underlying this distinction drawn by Malebranche is the Cartesian dualism of sense and thought. And this epistemological dualism, in its turn, we have recognized as nothing but the twin version of the ontological dualism of body and mind. As an offshoot of Cartesian mechanicism it barred Descartes, as we have pointed out, from developing an adequate theory of genuine information processing. Instead, he was constrained to formulate no more than an unsophisticated and unsatisfactory theory of information correlation. To invoke subconscious and even quasi-intellectual operations transforming internal representations in accordance with complex optical principles was simply forbidden by the negative heuristic of Cartesian mechanicism inasmuch as the latter implied a radical dualism of body and mind. And yet at the same time an information-theoretical solution precisely along these lines seemed to be strongly suggested by the positive heuristic of that very same program.

Thus Descartes, in elaborating upon 'natural geometry' Keplerian style, apparently did assume subconscious calculations determining visual distance by utilizing the cue of convergence of the optical axes. Yet he had no easy way of accounting for the processes involved. He finally attributed them to an "action de la pensée"²² which he described - inconsistently, it seems, and certainly in conflict with his later teaching in the *Meditations* and the *Principles* of the mind as essentially active and conscious of all its activities - as "implicitly [containing] a reckoning like that made by surveyors"²³ and yet involving no more than "a simple act of imagination"²⁴. If this is already obscure in itself, it is not at all clear how (as Descartes goes on to explain) natural geometry "by means of a single eye if we make it change its place"²⁵, i.e., by means of sequential rather than simultaneous impressions, could possibly constitute a simple act of imagination.

Seen in this light the terminological novelty introduced by Malebranche is highly significant. For whereas Descartes dealt with natural geometry as constituting a "simple" act of the imagination,

Malebranche speaks of natural judgments being "*compound*". This terminological change reflects, I believe, a progressive shift in the positive heuristic of the developing representationist research program towards a more detailed *psychological* theory of post-terminal information-handling processes.

As a result, the internal tension built into the mechanico-representationist research program is much more conspicuous in Malebranche than it was in Descartes. For on the one hand Malebranche was keenly aware of the hiatus in Descartes' theory of vision and of the need to attribute to the mind (or to *some* mind, at any rate) an organizing role with respect to the data of sense. While on the other hand he could not free himself from the dualist doctrines defining the Cartesian conception of science. Consequently, all quasi-intellectual processes which he himself had just identified and analysed with great care are projected back into the sphere of the purely sensible. Thus all *perception*, although apparently intellectually organized and being at any rate very complex, are yet to be regarded, after all, as no more than mere passive *sensations*, 'compound' sensations to be sure, but nonetheless passively produced.

4.5 *Rational reconstruction of Malebranche's occasionalism. Divine intervention and the computer analogy.*

The impossible tension the ambiguity discussed above naturally generated in his theory Malebranche has sought to resolve by a *deus-ex-machina* theory which in the history of psychology met with so much scorn and derision that it even eclipsed the extraordinary advanced character of Malebranche's theory of perception. However, the history of ideas deserves a less narrow-minded historiography. Malebranche's universally denounced so-called 'occasionalism' should not be regarded as a scientific aberration. Nor should it be laid to rest in the bric-à-brac collection of theological absurdities sprung from the heated imagination of deranged popes and priests who had gone off their rocker since the scientific revolution. On the contrary, it should, and it can, be reconstructed as a

rational development of a research program being confronted with its own conceptual limitations in that it provokes substantial questions (concerning the interaction of body and mind) which in principle it cannot answer *within* the mechanistic framework of science as conceived by it. In other words, Malebranche's occasionalism should be regarded as a first and serious, if obsolete, attempt to provide an adequate solution for the exceedingly complex information-theoretical problem of the organizing 'ratiomorphic' activities apparently operative in the production of even primitive perceptual knowledge. This problem was the immediate result of the mechanistic strategy being applied to the field of perception. And it led to a thorough theoretical aporia at two related philosophical frontiers.

First, the psycho-physiological frontier concerning the relation of two essentially different entities, viz., body and mind: how is it possible at all for any transactions to occur between levels of reality so utterly different? Moreover, who, or what, controls these apparent interactions? For, surely, these interactions prove to be so incredibly complex that a simple correlation theory as in Descartes clearly does not suffice to explain these psycho-physiological events. On the other hand, the more we learn to appreciate the literally mind-boggling complexity of the information-handling processes, the more implausible becomes a purely intellectual solution of the mind-body problem. This virtually unsolvable theoretical dilemma finally drives Malebranche to embrace the doctrine embodied in his occasionalism: God himself takes care of the apparent interactions of a seemingly intellectual character between body and mind. Hence Malebranche's dictum that ultimately we "see all things in God".

From a modern point of view this looks like a pretty bizarre theory. However, we should heed the fact that the terminology used by Malebranche in his description of the complex information processing he had analysed is not so much inspired by theological concerns but rather constitutes, as we will see, a striking anticipation of the modern *computer* analogy. And this analogy, as we

know, has proved to be extraordinarily fruitful in the history of cognitive psychology²⁶.

The second aporia brought about by the application of the machanicist research program to the field of perception concerned the epistemological problem of the objective basis of human knowledge. In Malebranche's remarkable theory of judgements ("jugements des sens"²⁷) this problem comes about in a sequence of steps (here omitted) leading to skepticism with regard to the senses²⁸.

Thus, even though Malebranche seems to realize more acutely than Descartes that visual perception in general involves more than mere seeing, he ultimately refuses, just like Descartes, to develop a realistic theory of information-handling processes capable of utilizing subconscious retinal and non-retinal cues for the development of complex but adaptive perceptual hypotheses. For the 'natural judgments' he invokes are the very opposite of finely tuned cognitive strategies designed to minimize error and to maximize perceptual reliability. Rather, they occur automatically, as instinctive and instantaneous reflexes, according to fixed and immutable laws governing the union of body and mind. They apparently involve "des raisonnements instantanés" which vary with every movement of our eyes, but in truth they are operations "tous formés par un acte éternel"²⁹. Their surprisingly fortunate results can be explained only by reference to omnipotent Providence, not to mortal intelligence³⁰. However, since God always acts in accordance with the same laws, illusions are bound to occur whenever the data are poor or the inferences not suitable to the logic of a particular situation. Consequently, perceptual knowledge, though based on interpreted experience, is both irredeemably fallible and in principle unrevisable even in the light of conflicting experience. In its construction the *finite mind* can play no part, neither through conscious activity nor through blind instinct. This pithy statement happily summarizes the double aporia generated by the mechanistic research program as well as the respective solutions suggested by Malebranche.

Notice, however, how in Malebranche's theory of divine intervention the computer analogy is strikingly manifest:

I believe I should warn again that it is not our soul which forms judgments of distance, agnitude, etc., ... but God does so, in accordance with the laws of the union of mind and body ... However, God makes these judgments in us and for us in exactly the same way as we would form them ourselves if only we had perfect knowledge of optics and geometry (such as God has); and if we also knew perfectly everything that actually occurs in our eyes and brain; and if our soul could act accordingly in itself, giving itself its sensations. That's why I attribute to the soul the capacity to form these judgments and reasonings and to generate in itself corresponding sensations which, in point of fact, can only be the effect of an infinite intellect and power. Thus, the very moment we open our eyes God alone can instruct us instantaneously about the magnitude, the shape, the movement and the colors of the objects surrounding us³¹.

The information-theoretical problem inherent in the mind-body relationship and rightly exposed by Malebranche he thus resolved by an impossible compromise. For on the one hand perceptions are quite "natural", existing in us, finite minds, simply as "compound sensations" in whose formation we take no part. In so far, then, visual perception (whether compound or not) must be considered as pure *seeing*, as in Descartes. But on the other hand, with respect to God, the Almighty Computer, perceptions must be regarded as genuine *judgements*. For God *interprets* to us our bodily states and accordingly forms the sensations we have. The psycho-physiological gap exposed by mechanicism but which it cannot close, is now bridged by the fiction of a divine computer.

That a primitive computer model is clearly intended also transpires from the fact that God does not act arbitrarily but strictly obeys, like a programmed machine, the fixed laws governing the union of body and mind³². This is why distance can only cause an increase in apparent magnitude when registered on the sensory apparatus. Intellectual knowledge, such as an astronomer's knowledge of the real distance of celestial bodies, does not affect the senses and hence cannot exert any influence upon our sensations.

Thus, despite Malebranche's apparently 'activist' notion of perception as involving interpretations imposed upon arbitrary intelligible signs - an idea which could come to full fruition in Berkeley's theory of perceptions but which, inchoately, begins its life in Malebranche's pages - the ultimate effect of Malebranche's theory, as in Descartes', is nevertheless to equate perception again with pure sensibility and with passive receptivity of ideas directly triggered by physiological impressions, and to regard perceptual judgment as a mere mode of sensation depending on God's incessant and beneficent interventions. The revolutionary impact of the theory that all perceptual knowledge is based upon *interpreted* experience is thus dulled by the corollary thesis that if there is more to seeing than meets the eye, it is God's Providence alone which, by an eternal act of his benevolence, takes care of these theoretical elements. To us, passive percipients, they are, however, indistinguishable parts of the given-in-experience and thus not modifiable in the light of new experience.

5. Conclusion

Descartes' mechanistic research program comprised a negative heuristic according to which reality was subdivided into exactly two substantial domains, the world of mind and the world of matter. Mind he defined purely intellectually, matter purely mechanically. Consequently, any scientific account of corporeal states and interactions had to be formulated in purely mechanical and mathematical terms. This even held for physiology and ethology. To appeal to any other ordering principles such as proclaimed by the Aristotelian pseudo-animistic doctrine of souls would be a methodological sin incompatible with the Cartesian program.

Now to bridge the gap between being and thinking, or between body and mind, inevitably called for an information theory of perception. The pattern of neurophysiological stimuli on the walls of the pineal gland is qualitatively entirely unlike the mechanical properties of the external world nor does it bear any qualitative resemblance to the phenomenal and geometrical aspects of the world

as perceived. Consequently, that pattern, far from being a qualitatively adequate representation of the real world, can only be regarded as a mere symbolic representation which, though arbitrary, is nevertheless *rich enough from an information-theoretical point of view* to account for the diversity of the perceived qualities of the phenomenal world (i.e., of the world-as-given-in-immediate-perception). This is the *positive heuristic* of the Cartesian program, which, though refined and developed, still clearly inspires the dynamics of Malebranche's enquiries as well. For even though Malebranche required divine agency to deal with the extraordinary complexity of the information-handling processes, yet inasmuch as God only acts on the occasion of physiological impressions one still ought to abide (such is his implicit view) by the Cartesian heuristic of tracing all phenomenal variety to known physiological variety³³.

However, this positive heuristic in its turn is conceptually constrained by a consequence of the negative heuristic of the mechanist research program, viz., the *epistemological dualism of sensing and thinking* which parallels the ontological dualism of body and mind. This is why the ingenious information processing so keenly analysed by Malebranche cannot, according to him, be attributed to operations of the individual mind but must be interpreted as consisting of supra-individual processes. However, if we were to replace the fictitious God-computer by subconscious automatic processes, this would suggest a highly advanced model of the mind comparable to simulation models familiar in contemporary artificial intelligence research. But, needless to say, this theoretical vista is still a far cry from the conceptual world of Malebranche.

Thus, because Malebranche identifies the cognitive with the active and the conscious his program (and that of the Cartesians in general) leaves no room for the development of an autonomous empirical psychology of perception in addition to the mechanical physics and the rational psychology of Descartes. Consequently, Malebranche is very vague concerning such pertinent questions as to

what extent the individual has to be aware of the media ("moyens") of distance and magnitude. Again, it is hard to see what role can be played by individual experience in the development of mature perception. But the most important consequence of his epistemological dualism is that all the very diverse elements in perception are reduced to the same level as the purely phenomenal qualities such as color or pain. In other words, in spite of the promising theoretical break-through in the realization of complex intellectual contributions being made to ordinary experience, perception in Malebranche is ultimately collapsed back into the realm of pure sensation.

Only in the course of time a gradual sharpening was to take place of the distinction between sensation (or the immediately given in experience) and perception itself. However, it should be clear by now why such a step would not be an easy one to take. For in the first place it would imply the abandonment of the Cartesian dualism of thought and sense. Secondly, it would involve a considerable broadening of the conception of the mind by the gradual, if laborious, recognition of purely cognitive processes being operative at a subconscious level. And finally, as a result of this latter development, it would lead to an extension of the domain of cognitive psychology and to its establishment as an *empirical* science.

In due time the given-in-visual-perception was strictly confined to the retinal impression. Molyneux already initiated this development by observing that the retinal 'image' of distance is a mere point so that distance itself is "invisible"³⁴. In Thomas Reid, one century after Malebranche, this leads to the more sophisticated conclusion that at any rate there is no *retinal* basis for visual distance discrimination so that, at least, distance is not *immediately* given in vision and thus does not properly belong to the things we 'see'. We will remember that Malebranche, by contrast, also included the decoding results produced by natural judgements as belonging in the last resort to the 'given' in perception.

Complementary to this sharpening of the distinction between seeing and judging, or between sensation and perception, more research was being devoted to, and further theoretical development was achieved of, those 'deeper' information-handling processes to be distinguished from the surface processing defining no more than the 'raw data' of vision. As stated above, this development would lead, slowly but surely, to a recognition of subconscious cognitive processes shaping the phenomenal world. At the same time, however, this would also toll the knell of the Cartesian foundations of the mechanist program of philosophical psychology. It was not until the mid-19th century that this step could finally be taken. And it was Helmholtz, in particular, who boldly initiated the new research program into the theory of perception, thereby ushering in a new era in the history of psychology.

Notes

1. Designed and developed at the London School of Economics this methodological conception originally derives from the work of Imre Lakatos; see esp. his 'Falsification and the Methodology of Scientific Research Programmes' and his 'History of Science and its Rational Reconstructions' in his (1978: 8-101; 102-38).
2. Lakatos (1978: 48-9).
3. Ibid., p. 49-52.
4. Meyering (1981), Straker (1971), and Crombie (1967).
5. *Dioptrics*, Discourse VI, AG 249-52.
6. *Entretien avec Burman*, Adam's edition, p. 24; AT V, p. 152.
7. Cf. *Principles*, I, 45-6. HR, I, 237.
8. *Reply to Objections*, VI, HR, II, 252.
9. *Dioptrics*, Discourse VI, AG 250.
10. *Reply to Objections*, VI, HR 252.
11. "Sensation composée". Cf. RV Liv. I, Chap. VII, 4, p. 17.
12. RV p. 22.
13. RV I, XIV, I, p. 33.
14. *Regis* (1690).
15. RV p. 23.
16. *Réponse à M. Regis*, I, 3, OM XVII-1: 264-5.
17. Lakatos (1978: 31 ff.).
18. *Réponse à M. Regis*, I, 6, OM XVII-1: 266.
19. Lakatos (1978: 33-4).
20. This qualification is in the spirit of Malebranche's overall theory since he allows that visual distance discrimination utilizes cues which we do not consciously perceive (e.g., the data involved in accommodation). Thus the meaning of Malebranche's principles is not the same as that of Berkeley's "evident" principle that no idea not itself perceived can be

the means of perceiving any other idea. On the contrary, Malebranche does not mean to draw a distinction here between conscious and unconscious material but rather between intellectual and sensible material. And the latter is defined not in terms of what is available in consciousness (to be determined by introspection) but rather in terms of what is physiologically present to the mind (to be determined by studying physiological activity).

21. *Réponse à M. Regis*, I, 4, OM XVII-1:265.
22. *Dioptrics*, Discourse VI, AG 250.
23. Ibid.
24. Ibid. Emphasis added.
25. Ibid.
26. Leibniz, himself a computer scientist of the first hour, further developed the computer analogy and, moreover, related it to a revolutionary theory of the subconscious. Cf. Meyering (1981: 296-8); also cf. L.H. Whyte (1960).
27. RV I, VII, p. 17.
28. Cf. RV I, VII; and RV IX.
29. RV IX, p. 24.
30. Ibid.
31. Ibid. Cf. also *Dernier Eclaircissement*, 26, OM III, p. 327; and *Réponse à M. Regis*, I, 10, OM XVII-1, p. 269.
32. Ibid.
33. Ibid.
34. Molyneux (1692: 113).

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Abbreviations

- AG: *Descartes, Philosophical Writing*, E. Anscombe and P.T. Geach (eds.), New York, 1971.
- AT: *Oeuvres de Descartes*, C. Adams and P. Tannery (eds.), 12 vols., Paris, 1897-1910.
- HR: *The Philosophical Works of Descartes*, 2 vols., E.D. Haldane and G.R.T. Ross (eds.), New York, 1973.
- OM: *Oeuvres de Malebranche*, vol. XVII, P. Costabel, A. Cuvillier and A. Robinet (eds.), Paris, 1960; vol III, G. Rodis-Lewis (ed.), Paris, 1964.
- RV: *Recherche de la Vérité*: in: *Oeuvres Complètes de Malebranche*, publié par MM. de Genoude & de Lourdoueix, Tome Premier, Paris, 1837.

COGNITIVE PSYCHOLOGY AND THE HISTORY OF SCIENCE:

A NEW LOOK AT MICHAEL FARADAY

by

Ryan D. Tweney

Department of Psychology

Bowling Green State University

Bowling Green, Ohio 43403

U.S.A.

In recent years, a creative tension has existed among historians of science concerning the relative importance of internalist and externalist history. Proponents of the latter have rightly argued that no historical account of science can be considered complete which ignores the social and cultural context of the investigator. Among historians of psychology, in particular, there has been much concern with the necessity of avoiding "presentism" and the biasing effects of its frequent ancillary, a focus on "Great Men".¹

Healthy as this critique has been, it has generated its own excesses. It is now common to find a new reductionism in some studies of science, an attempt to reduce science to "nothing but" a reflection of cultural and social forces. Within some traditions of the sociology of science, the tendency has proceeded to the point that purely relativist accounts of science are now frequent. In books like Latour and Woolgar's *Laboratory Life* (1979) there is no room for rational inquiry, for discovery, for creativity on the part of individual scientists. Instead, the social unit of analysis is the only unit of analysis:

"We argue that scientists ... are routinely confronted by a seething mass of alternative interpretation. Despite participants' well-ordered reconstruction and rationalization, actual scientific practice entails the confrontation and negotiation of utter confusion."
(Latour and Woolgar, p. 36)

For Latour and Woolgar, understanding such a process is a matter for sociology, since the only relevant cognitions on the part of

scientist-participants involves the application of group norms to rationalize the confusion. Such a perspective can reveal a good deal about the nature of science; I do not wish to argue that it is incorrect, only that it is incomplete.

Recent decades have seen a series of accounts by cognitive psychologists of the heuristics used in complex problem-solving environments with important implications for an understanding of science. Such heuristics have been the focus of extensive laboratory research, and we are now in a position to make several strong claims about their nature.² In particular, of the heuristics relevant for an understanding of science, all seem to share the following characteristics:

- (1) They are subject to "executive control" (via a process not now understood), and hence may or *may* not be applied in a given situation. Further, their use is modifiable as a function of circumstance, training, etc.
- (2) All appear to be accommodations to human limitations on information processing capacity.
- (3) Most reveal a strong bias to confirm an expectation.
- (4) All are sensitive to the nature of the mental representation of the problem at hand.
- (5) All appear to be adaptive in at least some task environments.

The implications for the history and sociology of science appear clear: we do not need to attribute *all* aspects of scientific thought to the interplay of latent social and cultural forces. While such forces undoubtedly exist, there may be a good deal we can discover by considering the manifest cognitive forces which are part of the problemsolving capabilities of the working scientist. The historical understanding of science may thus be able to derive benefits from a cognitive psychology of science, just as it now benefits from the sociology of science. The present paper presents a case study which hopefully reveals the point. I have sought to apply insights derived from cognitive psychology to an understanding of Michael Faraday's research program. I believe the results force consideration of Faraday's research from perspectives

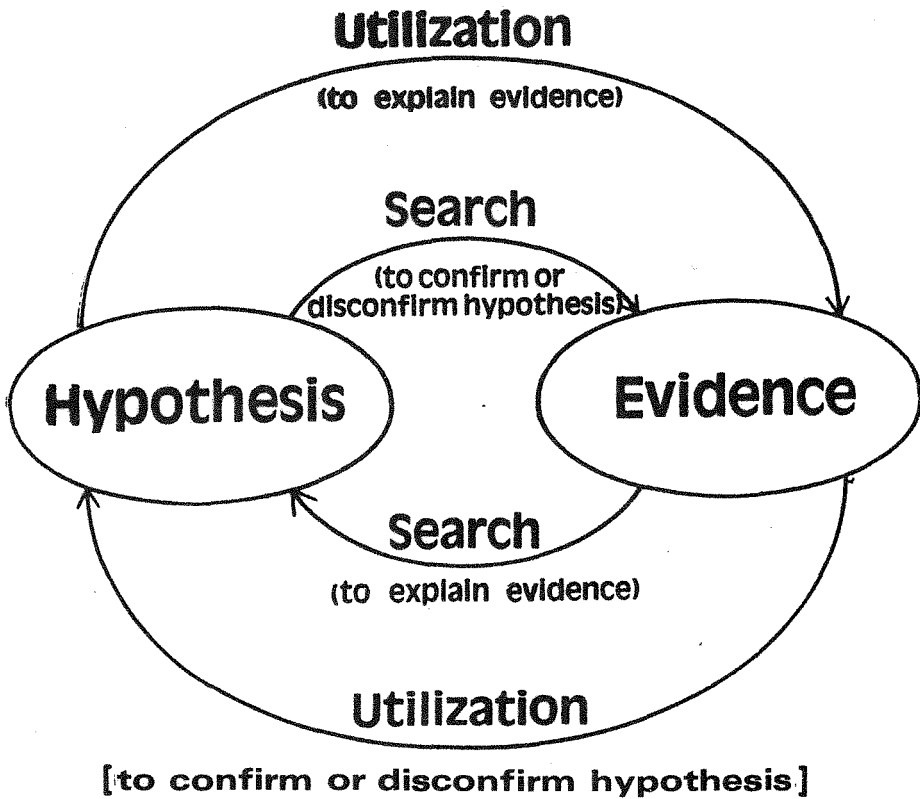
which have not previously played a role in the very extensive literature on Faraday.³

For the kind of analysis I am proposing, Faraday is an ideal subject for several reasons. First, his work is characterized by continuing interplay of theoretical and experimental work. Since, as I argue below, it is the relation between hypothesis and evidence that most readily lends itself to a cognitive approach, Faraday is exactly the kind of scientist whose work should be illuminated by consideration from the new perspective. Second, Faraday was an inveterate note-taker, recording nearly all of his work in extensive, well-organized notebooks (some of which have been published, Faraday, 1932-36). The notebooks are, at times, nearly as thorough as a "think-aloud" protocol, of the sort studied by Simon (cf. Newell & Simon, 1972). Third, Faraday is a figure of commanding importance in the history of science, one whose methods have been held up as models for generations of young scientists (e.g., by Tyndall, 1868). While no case study can ever be the basis for generalizing to all scientists, it is at least reassuring to begin with someone whose style is broadly influential.

The perspective used to approach the analytic problem of analyzing Faraday's work is shown schematically in Figure 1. "Hypothesis" and "Evidence" are here both taken to be cognitive (rather than ontological entities). The difference between them resides only in the fact that evidence is believed by the scientist to represent something "out there", whereas a hypothesis may or may not be "out there". The arrows stand for mental processes rather than mental representations. While they can have many diverse characteristics, two broad classes are of special concern: search operations, in which new mental representations are constructed, and utilization operations, in which existing mental representations are coordinated (e.g., by showing that one can be logically derived from another, is consistent with another, resembles another by similarity, etc.).

Arrows are directional in the diagram to capture the fact that scientific activity at this level of analysis is generally motiva-

Figure 1



ted in the sense that one or another mental representation is taken as primary. Thus, I may possess a *hypothesis* and search for evidence to confirm or disconfirm it, or I may have certain evidence and seek to utilize it to confirm or disconfirm a long-standing hypothesis. Exactly what occurs by way of cognitive activity when a given arrow is under consideration is not fixed. Many heuristics are possible in most cases, and the choice of one over another depends upon a variety of factors.⁴ Similarly, the nature of a representation is dependent upon many factors - whether the scientists prefers visual metaphors, mathematical expressions, and so on.

The diagram summarized a framework for the analyses I am proposing. It is not, as it now stands, itself a testable theory. But conceiving of the scientific enterprise in this fashion allows one to raise the appropriate questions, and it is a framework that corresponds nicely to current work in cognitive psychology. As such, it itself possesses heuristic value.

To test the feasibility of the proposed analysis, 134 experiments carried out by Farady between August 29, 1831 and November 3, 1831 were analyzed. The block was defined by choosing all experiments carried out as part of Farady's successful discovery of electromagnetic induction (up to the date of his first report of the finding to the Royal Society). The series is primarily directed at establishing the existence and parameters of a set of interrelated empirical relationships.

The series begins with a crucial experiment carried out on August 29, 1831. The first entry in his notebook for that day described a device fabricated for the day's investigation. It consisted of an iron ring six inches in diameter and about 7/8" thick. One-half of the ring was wound with a copper coil insulated with cloth strips. The other half of the ring was covered with a similar coil. He connected the leads from the first coil to a battery. The leads from the second coil he passed over a freely suspended magnetic needle, set far enough away to be unaffected by the electromagnetic forces set up by the battery in the iron ring.

The magnetic needle served as a sensitive indicator of the presence of weak electric currents in the second coil. When the battery leads were connected, the magnetic needle was momentarily deflected. When the leads were disconnected, the needle was momentarily deflected again but in the opposite direction. When no current flowed in the first coil or when a steady current flowed, there were no effects in the second coil. Only on making and breaking the circuit could the effects be observed.

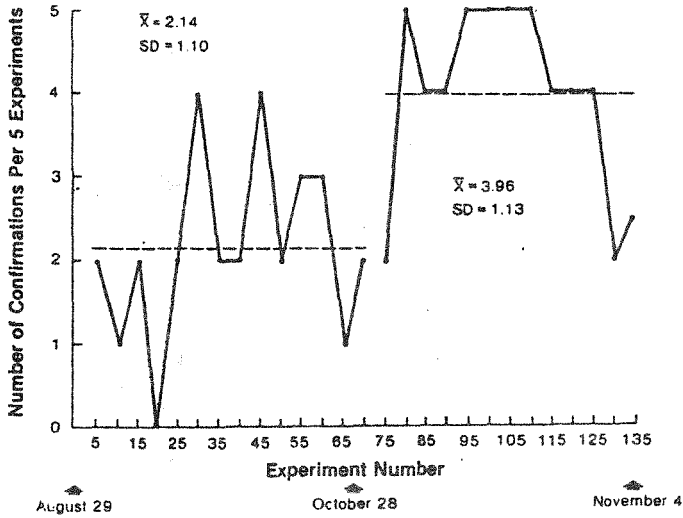
Others, including Faraday himself, had previously sought similar effects, but no one expected them be momentary. If a magnetic field could induce an electric current, everyone had thought the effects would be continuous with steady currents. In fact, currents are induced only by *changing* magnetic fields.

For the purpose of analysis, I regarded an "experiment" as defined by a manipulative verb (or a cluster of manipulative verbs). Thus, when Faraday spoke of "placing a needle in Coil N", and observing the result, the incident was taken as one experiment. There was very little uncertainty in classifying experiments in this fashion.

Deciding what hypothesis Faraday was testing in any given experiment was more difficult. Sometimes, in about 30% of the cases, Faraday explicitly stated what he was after. For some of the rest (about 30% again), the experiment was so closely tied to previous experiments that there was no problem in assuming that the same hypothesis was being tested. That leaves about 40% of the experiments for which some fairly subjective judgments were necessary. I worked backward from the results for these, supplementing the inferences, where possible, by reference to Faraday's final published results (Faraday, 1839-1855). One problem with the procedure, aside from its subjectivity, is that it results in a great multiplicity of hypotheses. For the 134 experiments in the series, 65 hypotheses were discriminated.

Once all of the hypotheses were elaborated, it was a simple matter to determine whether a given experimental outcome confirmed or disconfirmed each hypothesis. Figure 2 presents the results, blocked for convenience into groups of five experiments. Note that

Figure 2



there were relatively few confirmatory outcomes until after October 28, when Faraday gained access to a very large and powerful electromagnet belonging to the Royal Society. There is evidence for confirmation bias in both halves of the series, and especially in Faraday's repeated attempts to produce an induced current using ordinary magnets (he had little difficulty in producing such currents when electromagnets were used). In spite of six failures to obtain the sought-after effects (Experiments 10, 12, 17, 18, 19, 22), Faraday persisted until one confirmatory result was obtained (Experiment 23). At this point, he dropped the problem until after 20 other experiments directed at other hypotheses had been carried out.

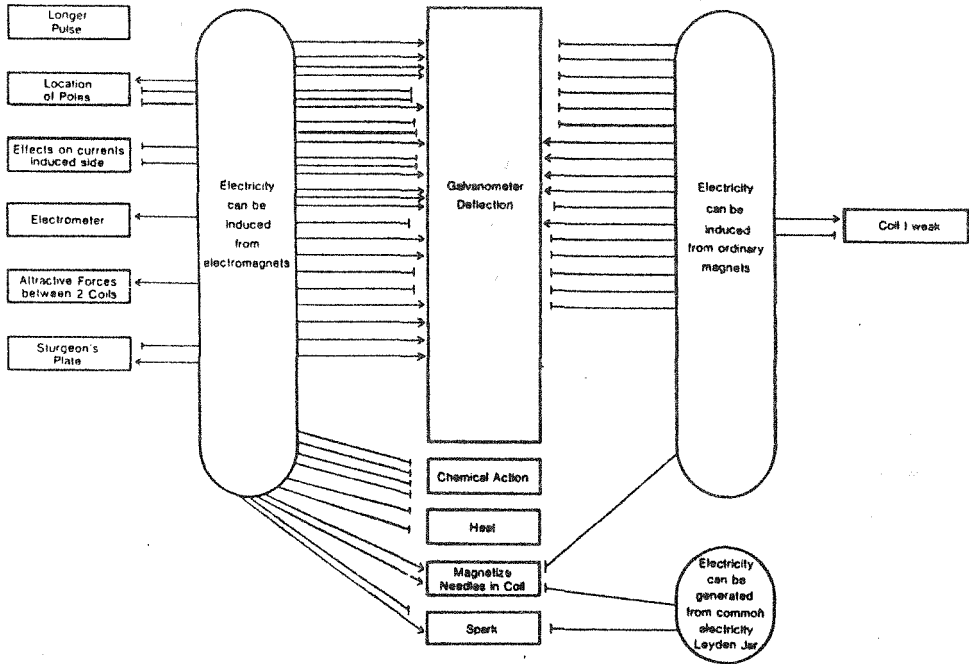
The above analysis is, of course, limited by the subjectivity of the classification of hypotheses. After completing it, I tried to reduce the multiplicity of obtained hypotheses by searching for common themes among the 65 hypotheses. The search proved rewarding,

as it resulted in exactly three major Hypotheses (capital H, to distinguish them from the 65 "lower case" hypotheses). When the 134 experiments were related to these three, the entire series turned out to be surprisingly simple in structure, as Figure 3 indicates. Square boxes in the figure represent observable entities, (i.e., evidence), with the most important ones arranged in the central column. Boxes with rounded edges represent the Hypotheses, while each line represents one experiment (the figure shows only those experiments up to October 28, 1831). Each experiment either confirmed (→) or disconfirmed (→) Faraday's expectation.

It can be seen that of the three Hypotheses one was relatively unimportant, having been tested (and disconfirmed) only twice. Of the two major Hypotheses, Faraday had much more success with the one on the left, that electricity can be induced from electromagnets. The two Hypotheses, are, of course, closely related. We now know (and Faraday knew in later years) that they are really two aspects of one phenomenon--that electric currents can be induced by *changing* magnetic fields. Thus, when an electromagnet is turned on or off, there is a changing field, just as there is a changing field when a permanent magnet is moved through space. This integration of the two Hypotheses relies upon the theoretical concept of a field, which was only latent in Faraday's mind in 1831.⁵ There had been many attempts to unite electricity and magnetism as different manifestations of a unitary force, especially since Oersted's discovery of electromagnetism in 1819-20. Faraday shared this belief, but he was also careful to keep the two major Hypotheses distinct in his laboratory work, as the present analysis indicated. Thus, from an experimental point of view, Faraday was exploring two closely related, *but not identical*, propositions. Until he showed their empirical commonality, he continued to regard them as distinct.

It seems apparent in retrospect that working with two related Hypotheses was a major facilitator of Faraday's success with the 1831 experiments. His strategy meant, in effect, that he had a relatively confirmable Hypothesis (on the left) to fall back on

Figure 3



when he encountered difficulty in confirming the other Hypothesis (on the right). Nor was the easier Hypothesis simply a time-filler: the results on the left provided him with clues to what might work on the right. With respect to the two Hypotheses, then, there is evidence for confirmation bias. Consider also the sequence of six disconfirmations of the right-hand Hypothesis that occurred initially, experiments 10, 12, 17, 18, 19 and 22. When Faraday finally confirmed the Hypothesis using two bar magnets (experiment 23), he dropped further attempts and turned to other problems, treating the right-hand hypothesis as if it were proved. His next experiment derived from the right-hand Hypothesis was the 43rd in the series, and was carried out exactly 30 days after the 23rd.

The framework given in Figure 1 maps nicely onto the preceding analysis. We can describe Faraday's research on the problem of

induction as constituting a search for evidence to confirm or disconfirm two *hypotheses* (i.e., the second arrow from the top). His use of two hypotheses, perhaps the most singular aspect of his approach, can be regarded as a cognitive heuristic of some generality; it resembles Platt's (1964) "Strong Inference", and has also been observed in at least one laboratory setting (Tweney, et al., 1980). It is strikingly similar to Gruber's (1974) "Network of Enterprise" notion.

The analysis is limited in scope, so limited that we would be unjustified in generalizing it even to other parts of Faraday's research, much less any other scientist's. Even so, it makes the proposed level of analysis appear quite promising. In particular, were we to look at any one of the 134 experiments in isolation, we would be, empirically, at a loss to account for its nature. Most would appear to be simply illogical - Faraday persistently sought confirmatory outcomes and ignored disconfirmatory ones. In context, however, such behavior appears far more justifiable, because we can see that he was using one easily confirmable hypothesis to provide support (psychologically) for his efforts on a second, harder hypothesis.

One implication, perhaps the most important one for the role of internalist history, stems directly from the above. The analysis suggests that Faraday's behavior possesses an underlying rationality that emerges only when the context of his research is taken into account at *the proper level of analysis*. Certainly the cultural and social context of his research must be considered to fully understand the place of his work in the history of science. Equally certain, however, is the fact that there is a cognitive context that must also be included. To assume otherwise, to assume that nothing cognitive is of interest because it is merely derivative, is to commit an error fully as serious as the error of presentism.

Notes

1. On presentism, see Butterfield (1931) and Stocking (1965). A focus on "Great Men" is characteristic in Watson (1978), and in Boring (1929/1950), in spite of each writer's attempt to avoid such an orientation. The present paper, in concentrating on only one, undeniably great, scientist, might seem to betray a similar emphasis. However, as will be apparent, Faraday is only a starting point, chosen for analytic convenience. The same techniques should be applied to other figures, both great and not-so-great.
2. Reviews of the relevant research can be found in Einhorn & Hogarth (1981), Kahneman, Slovic, & Tversky (1982), Nisbett & Ross (1980), and Tweeney, Doherty, and Mynatt (1981).
3. The Faraday literature is vast - cf. Jeffreys (1960). Good starting points are Tyndall (1868), Williams (1965), and Agassi (1971). The last named book has been the focus of substantial controversy - cf. Williams (1975), Agassi (1978), and Bradie (1976).
4. The implication is that such processes must be understood as participating in probabilistic relationships, rather than exceptionless laws (Tweeney & Doherty, 1983).
5. The development of the field concept has been described by Williams (1965) and Gooding (1981).

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TRENDS IN AMERICAN PSYCHOLOGICAL RESEARCH: 1900 - 1940*

Paolo Legrenzi
University of Trieste

Summary

During the first 40 years of this century there was an enormous investment in scientific research in the USA. Of all fields of research, in human as well as the natural sciences, it was psychology that had in this period the greatest percentage increase, both in terms of personnel and facilities. This fact appears still more impressive and worthy of analysing if we consider the much slower rate at which psychology developed in Europe and, even more surprisingly, in the socialist republics.

In this paper we shall first make use of some quantitative parameters to offer a sketch of the birth and early decades of American psychology. This sketch, although it does not go into detail, should refute several facile reductionist interpretations of the birth and development of the discipline in the USA. We shall then pass on to a closer study of this development, in order to clarify the interweaving of basic research with applications, and describe certain tendencies in research that have remained constant while the "schools" have been evolving. We shall describe some of the very diverse ways in which psychology became subordinated to the needs of capitalistic development in American society. Entire sectors can be found in which research assumed, or assumes today, almost exclusively "concealing" roles, elaborating theories or proposing conceptions of human nature that have rightly been described as "ideologies of convenience". Examples are the use of tests and much of the psychology of individual differences, the various forms of diagnosis of mental "abnormality" not based on any theory of the normal mind, the adaptation by American psychologists of Lewin's social psychology of human relations.

* Editors note: In the text send to the editors the indication of footnotes was missing. Since we were not able to get in touch with Prof. Legrenzi in time the editors decided to have printed the references but not the notes.

The Birth of Psychology in de U.S.A.

Psychology was introduced into the U.S.A. almost totally as a product imported from Germany. In fact, only in James do we find a founder capable of *blending* in an original way a set of ideas acquired in Europe with an epistemological-methodological system of reference greatly influenced by the American philosophical climate. It is true that Stanley Hall was typically American in his philosophical outlook, but he made no importance theoretical contribution to psychology except to give enthusiastic support to the experimental method, at the same time assuming the role of tireless organizer and administrator. If we consider two other pioneers, Cattell and Titchener, we can see how their initial work consisted essentially of introducing into American what they have learned from Wundt in his laboratory. Even by 1890, in the sector of human sciences, the number of Americans studying in their country was still less than those attending European universities. Twenty years later we find a completely different picture. We will not speak here on the "scientific conquests" about which American psychology has been able to boast since then; we will examine instead its influence in the area of university formation and research, bearing in mind that there were as yet no external research centres: these laboratories, linked directly or indirectly to the Defence Ministry or to the multinational corporations, became widespread at a later date. If we consider for example the grants to the various sectors of the fifteen most important universities, we find in 1910 approximately equal figures (about \$ 40,000) for the natural sciences (physics taking up half the sum) and for the human sciences (psychology already receiving the same sum als philosophy, namely \$ 6,000, and the remainder going to economics, political science, literature, and the arts). It is also interesting to note that the numer of enrolments in psychology courses was very low in relation to the size of the grant and, further, the fact that the salaries of psychology professors were among the highest (this reflects the fact that administrations in Anglo-Saxon universities hired a relatively small number of professors and lecturers).

These facts seem to indicate that the overall development of psychology should be interpreted not as an answer to demand from below due to the crisis in the humanities (as happened decades later in the Latin cultural area), but rather as a process directed from above. Psychological research flourished, and the reversal of the previous situation of German predominance is shown by the yearly figures regarding the number of articles published in each language (see Table 1, which gives figures up to the year 1927 for the *Psychological Index* and *Psychological Abstracts*).

The aggregate figures in Table 1 show how the predominance of the English language in psychology publications, and therefore the affirmation and development of psychology in the Anglo-Saxon countries, predates the use of psychology for practical applications.

Incidentally, still relating to Table 1, we are reminded how in the category "other languages" (bottom right hand figure) Italian was the chief language from 1894 up to 1924, after which its appearance gradually diminished to practically nothing.

Another of the many possible indications of this development (one could for example examine the increase in the number of laboratories) is the way the various psychological associations (the most important of which is the American Psychological Association - A.P.A.) grew stronger and took shape. An analysis of the transformation which the A.P.A. underwent for half a century in its composition, types of professionalism, etc., would merit a close study. Here it is sufficient to note the rate of increase in the number of enrolments (higher than any other professional corporation): from 200 enrolled in 1900 the figures rise to 20,000 in 1960 and 46,000 in 1973. What is interesting is not so much the constant linear increase but the wish to constitute a distinctive group based on a distinctive expertise: so much so that the A.P.A. was founded in 1892 and is the first professional association of psychologists. Quite early, in fact, the majority of publications tended to appear in journals that specialized in psychology or even in sub-sectors of psychology (see Fig. 2). On the other hand, the sectors that might produce practical applications were not initial-

Figure 1

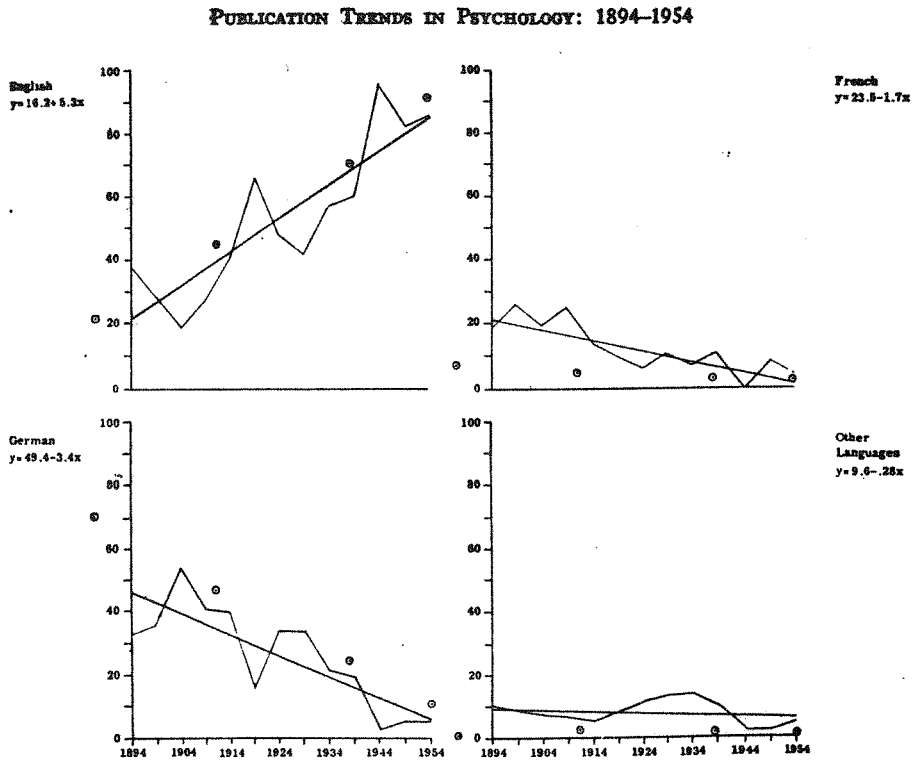


FIG. 1 Percentage distribution of entries by language. (Ordinate scale: 1 mm = 1%. The slope coefficient, b , in the secular trend equations is significant at the 1% level in all cases except Other Languages.)

ly the psychologists' most marked object of interest, and became important (or at least relatively important) only at a later period in the first quarter of this century (cf. fig. 3 - still Louttit's data). In the period 1940-1960 an important change occurred in the composition of the A.P.A. During the period we are considering in this article (1900-40) there was a preponderance of teachers and university researchers (often with external activities but still connected to the universities, but from 1940-60 the distinction changed considerably, until at the beginning of the 1960's it was:

Practice	= 30%
Teaching	= 20%
Research	= 20%
Management	= 15%
Test and others	= 15%

The evolution of American psychology from the period we have been considering up to the '60s can also be analysed by considering how psychologists make their living. While during the period examined here a huge majority of the members of the A.P.A. is made up of college professors who often give professional advice externally or run individual professional practices as therapists, in 1962 a representative sample of America's psychologists shows that fewer than 5% are free professionals, that fewer than 40% are in any way connected to universities, whereas as many as 35% work for local or federal agencies, and the rest for industry.

Function and Characteristics of Research Psychology

We have already mentioned how one of the main influences upon the birth of American psychology was so-called "practical interests". For example, in "History of Scientific and Philosophic Thought" edited by Geymonat, France Meotti writes (p. 374): "It is undeniable that from this point of view, at least in the first phase, the dominant psychological interest in the United States were more of a practical than a theoretical nature. There was a surge of interest in theoretical psychological problems with the spread in

Figure 2.

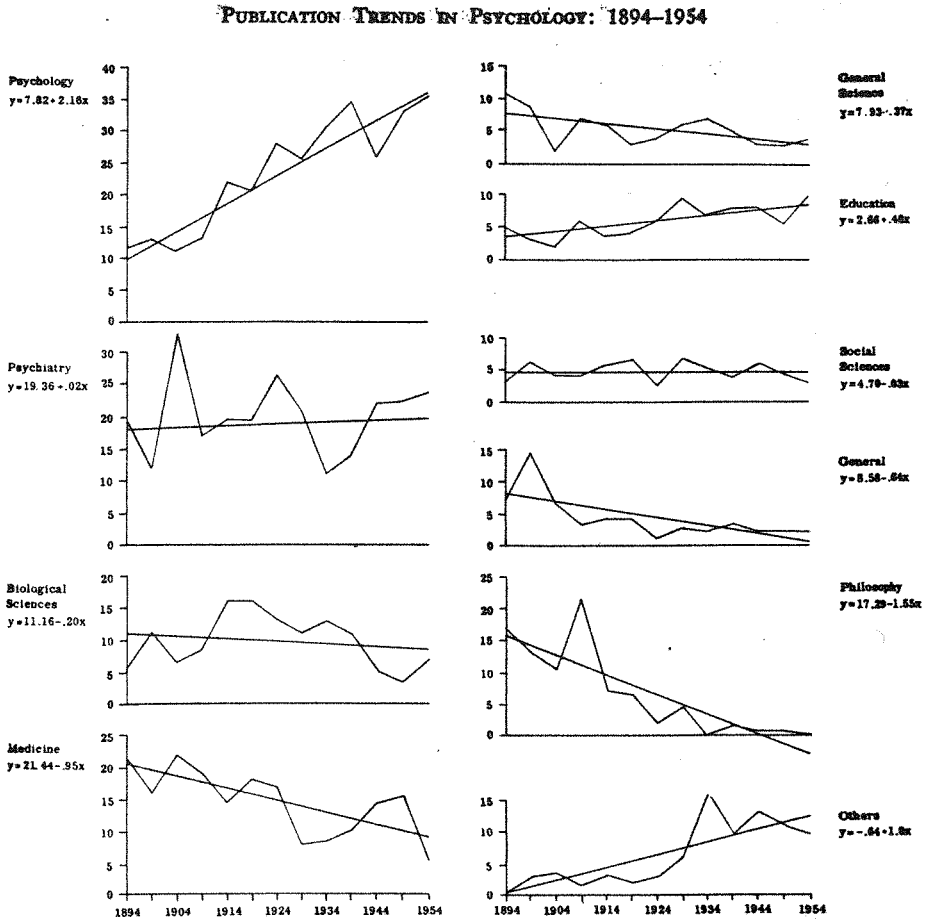


FIG. 2. Percentage distribution of journal article entries by subject fields of journals. (Ordinate scale: 1 mm = 1%. The slope coefficient, b , in the secular trend equations is significantly different from zero at the 1% level for Psychology, Medicine, Education, General, Philosophy, and Others, and at the 5% level for General Science.)

the United States of Gestalt psychology, which caused quite strong polemics especially as it aspired to set itself up as a general but also philosophical type of vision". Neither of these interpretations is adequate. Given the premise that the distinction between basic scientific psychology and applied psychology can only be seen - and in this we follow Holzkamp - as a differentiation between research based on long-term planning and research directed at a contemporary practical problem, it can be shown that much of American research up to the first world war was of the first type. This statement is borne out not only by Louttit's data (cf the last distribution of the lower right in Fig. 3) but also by the results

Figure 3. **PUBLICATION TRENDS IN PSYCHOLOGY: 1894-1954**

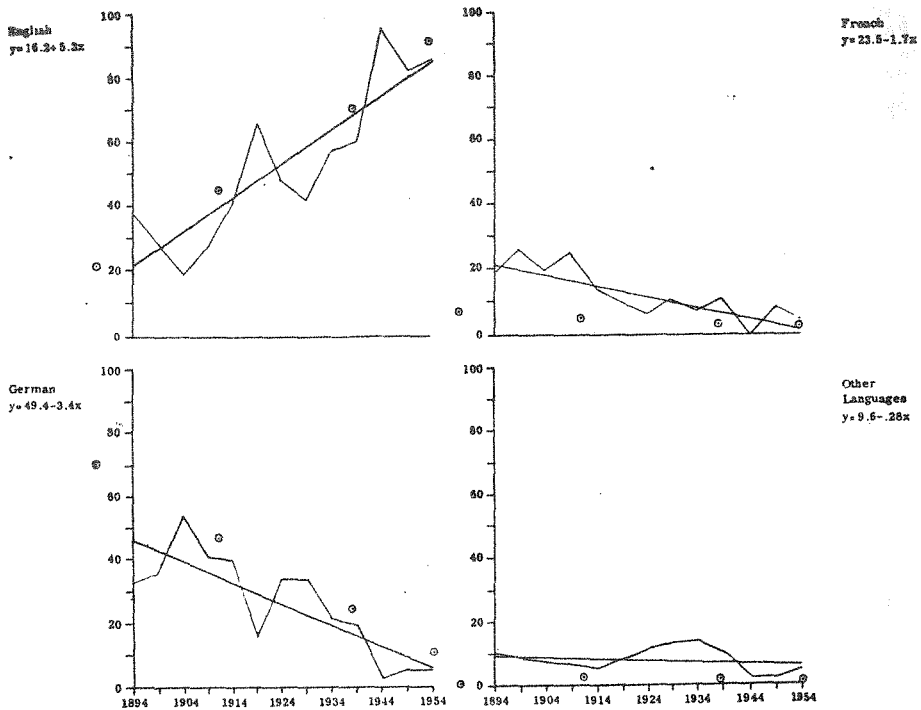


FIG. 3. Percentage distribution of entries by language. (Ordinate scale: 1 mm = 1%. The slope coefficient, b , in the secular trend equations is significant at the 1% level in all cases except Other Languages.)

The relatively low number of papers on applied psychology at the beginning of this century.

of one of the few American works that does not have exclusively rhetorical and celebratory aims, magnifying the prodigious quantitative development of the discipline (more people, more money, more research, etc.). I am referring to an analysis conducted in 1940 by Bruner and Allport. Bruner and Allport catalogued and then subdivided into a series of *not-mutually exclusive* categories, the articles that appeared in the following 14 journals which were chosen as being the most important by a sample group of A.P.A. members:

American Journal of Psychology (1887); Psychological Review (1888); Pedagogical Seminary (1891); Psychological Bulletin (1904); Journal of Abnormal and Social Psychology (1906); Journal of Educational Psychology (1910); Journal of Comparative Psychology (formerly Journal of Animal Behavior) (1911); Journal of Experimental Psychology (1916); Journal of Applied Psychology (1917); Journal of General Psychology (1928); Journal of Social Psychology (1930); Character and Personality (1932); Journal of Psychology (1935) and Psychometrika (1936).

Now applied psychology (even though understood here in the widest sense possible, and not in the narrow sense adopted by Louttit (cf. Fig. 3) comprised at the beginning of the 1900's no more than a quarter of what was published in these magazines. Only in 1918 must we add to the 30% of research made with applied aims 20% of research on the use of tests and 10% concerning the examination of clinical cases.

Just as the first phase of American psychology does not seem to have been dominated by immediately practical interests, neither is it true that the Gestalt approach stirred up deep, far-reaching debates. The data of Bruner and Allport confirm what has been shown by other researchers, namely that Gestalt passed relatively unnoticed right in the very school that was determined to fit individual contributions into a systematic and complex theoretical frame of reference (the acceptance of several specific discoveries in the field of perception and the development of several Lewinian themes actually bear witness on the contrary to a bitty and distorted use of Gestalt if viewed as a global proposal). In fact

the works that Bruner and Allport catalogued as "belonging to schools, their rejection, relative debates" take up no more than 2% of the space in 1908 and in 1918, rising to 10% in 1928 (it should be taken into account that the debate then involved behaviourism too) and falling to 6% in 1938.

This orientation contrasts with the one adopted by those who validate their conclusions with reports of a phenomenological, descriptive, but non-quantifiable nature. While the latter orientation falls from 26% of the articles in 1910 to 5% in 1930 in the *Journal of Abnormal and Social Psychology* (and with analogous percentages in the other two magazines), the numerical-empirical approach passes from 1% in 1910 to 37% in 1930, and finally to 63% in 1950.

These trends in the psychiatry and clinical sector also bear witness to the consequences for psychological research of what we have called the "functional-pragmatic" climate of American culture. To this climate should be attributed not only certain distinctive features concerning methodology and research content but also the rise of theoretical tendencies (or "schools") such as behaviourism. A symptom of this climate is the progressive elimination of research into mental processes and (still following Bruner & Allport's data) the growing use of animals as experimental subjects (from 3% at the beginning of 1900's we pass to 15% in 1938); or the progressive abandoning of notions like that of unconscious mental processes (from 15% in 1908 to 5% in 1938), and vice-versa for the general employment of statistical methods. These, together with the other method and content-oriented aspects previously mentioned seem to appear in that same common tendency.

On the more general scale that invests the significance of this research, it would seem too narrow to translate what has been called a largely functional-pragmatic climate into an exclusively instrumental function of psychology dictated by practical needs (still on the data level let us remember that Bruner & Allport's category "applied psychology" falls from the previously-mentioned 30% in 1918 to 18% in 1938, despite and increase in the number of

journals in the meantime). To confirm this interpretation of the birth and development of American psychological research we can cite other characteristics which, although less striking and less easily quantifiable, are perhaps more interesting on a theoretical plane: namely, the increasing use of animals in experiments and, most important of all, the new role taken on by the subject in experimental research. This is a deep change, which constitutes an index of the Americanisation of German psychology being the lowest common denominator of all the investigations conducted with the experimental method (and it is well-known that this method spread like wildfire throughout the most disparate fields of research).

In short we agree with Holzkamp (1974, p. 51) that in American psychology the experimental subject is treated as "a member of human race". American psychologists no longer resort to detailed and controlled phenomenological observation, or to the introspective self-analysis of a few subjects or even of one subject alone. They examine larger samples, representative of a given population, and bear in mind "the variability of common man's behavior". Finally, since the psychological qualifications of the experimental subject are no longer relevant, the personal identity of the subject is not specified: he simply retains the character of an element in a statistical distribution. Mention has already been made of the introduction of statistical methodology into research. Up to 1900 it only appears in 2% of the articles examined by Bruner & Allport; in 1908 it had risen to 6%, in 1918 to 27%, and in 1928 to 46%. This is only a methodological change: it implies a philosophy of research: a) in the first place it enormously increases the asymmetry between experimenter and subject, the latter taking part in the experiment as an "organism" and not a "person"; b) the utilization of the subject as an "anonymous organism" gives incentive to the use of artificial experimental conditions and is the basis of the excessive psychological reductionism that characterizes a good part of the research in cognitive processes and social behavior between the two world wars (32); c) thus we end up by studying men not in the multiple and disparate conditions in

which they effectively live their lives, but rather producing in the experiment artificial conditions in which men are regarded as "experimental subjects". As we shall see, this tendency will be emphasized and definitely sanctioned as "methodological rigour" with the advent of behaviourism.

We shall not consider here the ideological implications of these methodological assumptions. Summing up, we can say that there have been two kinds of results: 1) the analysis of goal-directed behaviour, which was a characteristic of German psychology at the beginning of the 1900s, gradually being substituted by the various manifestations of the stimulus-response mechanism; 2) all the phenomena that do not fit into this scheme being gradually eliminated from psychology. The idea of man which emerges from such a psychology, i.e. deprived of an effective capacity to acquire knowledge, and hence the power to act, performs instead a "concealing" role that is eminently ideological. It thus belongs to the functional-pragmatic climate mentioned before, whose socio-economic concomitances we are not going to examine here, and accomplishes in a much more integrated way than in European countries the role of substitution than in the countries like Italy occurred at a later period, and only after the failure of traditional disciplines to conceal contradictions that were much deeper and more obviously outside the world of culture.

Research and its social use

The methodological and theoretical choices of research content examined in the preceding paragraph are being stressed not only to describe the characteristics of American psychological research in the period being considered here, but also to try to analyse the relationship between research and its social use. The bases of this relationship were established in quite a simple and ingenuous way in the first decade of this century when Munsterberg, transplanting the German tradition, started applied psychology in the United States. Then academic psychology, not yet too bound by methodological ties, sought to achieve an immediate decanting of its dis-

coveries into concrete reality: industry, medicine, mental hygiene, testimony and other aspects of the world of justice, schooling and all the other forms of interpersonal relationships. But gradually as university psychology developed characteristics than rendered it more and more difficult to relate to everyday life, there arose a schism, which is only recently heating in the U.S.A., between so-called basic scientific research and the application of psychology to concrete everyday problems. Must research conducted in an academic sphere therefore come to be considered a form of "scholasticism" disconnected from any social use whatsoever?

We do not believe that even this radical interpretation is correct since we have tried to show, even though briefly and schematically, how "mainstream behaviourism" has performed an ideological concealing role at a higher level, sanctioning and communicating to workers a certain idea of Man, spreading an "ad hoc" image of psychology among those outside the field, legitimizing in this way a certain kind of social use. This role of "ideological concealment" is immediately evident in the elaboration of those instruments typical of this stream of applied psychology, such as tests, a stream possessing just those characteristics which are very weak as regards their contributions to knowledge. In this field of applied psychology should also be included the enormous mass of values created exclusively to satisfy short-term exigencies and therefore lacking in any theoretical depth even though often valid on the methodological level.

Let us consider for example the specific but imposing case of the psychological department directed by Lee at the Ford Company. This department, already formed in 1912 by a hundred or so "counsellors", was nothing more than a mechanism for controlling the behaviour of the employees inside and outside the works (in fact the counsellors were originally called "investigators"). This control had no scientific foundation, nor did it pretend to have any. In practice it amounted merely to a time-and-motion-study conducted in order to devise salary incentive schemes. And it was on the basis of the possibility of introducing such an incentive

that the revolutionary "five dollar a day" adopted by Ford in 1914 was founded. Although the aim, even explicit, of such a rationalization and standardization of the production process, the so-called "scientific organization of enterprise", was not at all "scientific", it cannot be denied that in a short time from the employer's point of view "visible" and successful results were achieved. In the first place increased profits: as Venturini recalls, not even Friedman questions the fact that the organization profited in an immediate sense: "A system of increasing the efficiency of the machinery and labour, attempting to achieve raise their output immediately to its maximum level, and inclining to put economic success before the physical and mental well-being of the worker, corresponded to the needs of a certain stage of world capitalism. Furthermore, in a short time, there were other short-term benefits resulting in the chance to reclassify wage scales and adopt an automatic promotion plan: absenteeism, though measured in a rough way, dropped at the Ford Company from 10% in 1913 to 1% in 1915".

In the medium term, however, the absence of basic scientific objectives revealed itself in the complete absence of any kind of long-term planning, and therefore of measures to prevent this "scientific organization" producing negative effects (from the point of view of production) such as a decline in the health and capacity of the workers. If the immediate effect was thus positive, it is also true that potentially such a rationalization of technique went against a biological rationalization of production. "In the United States and in England (where the industrial Fatigue Research Board was founded) the First World War", recalls Venturini, "gave a vigorous impulse to research in the conditions relating to performance, length of working hours, breaks, instability of factory workers, and injuries, in view of a biological rationalization of production. Such a rationalization was placed beside, or to be more exact, even if not always consciously, against the one championed by Taylor and his followers ..." for which reason one can never insist enough on keeping apart these two

kinds of approach to problems involving a human factor apart. On the other hand, not everyone, even from that moment, had succumbed to the "scientific" fascination of Taylorism, which boasted among its supporters scientists and thinkers of the calibre of Le Chatelier and Lenin: indeed precisely those psychologists that had an experimental background, like Münsterberg himself, since they were anxious to spread psychology in every one of its possible versions and applications, recognized "the irremediable psychological amateurishness characterizing Taylor's plans". Münsterberg's affirmation of principle that "psychological experiment must be placed at the service of commerce and industry" can be regarded as the constitutional card of all the "serious" work done in the successive decades by industrial psychology.

If this schematic historical-critical analysis of the main tendencies of American research in psychology claimed to cover even briefly the period indicated in the title, it would have to examine what happened between the two world wars. Limits of space do not allow us even briefly to touch upon the complex but globally unitary phenomenon constituted by behaviourism, to which is linked a whole series of collateral matters briefly listed here: 1) the gradual loss of faith in the uncritical use of tests; 2) the setting up of more and more psychology counseling agencies (in the field of publicity, marketing, human relations, etc.) completely outside the universities and becoming increasingly remote from them; 3) the beginning of a tendency that became relevant after the second world war, namely the appearance of research, though basic, in universities which, as time passed, were not destined to retain their monopoly in it; 4) more and more massive intervention of local bodies and government agencies in the sector of psychological assistance. Here too we see at first a close collaboration with the university, and later, more direct and autonomous forms of control.

Even a superficial examination of these points would require more space than we have at our disposal. We would like to conclude with a personal observation consisting of a hypothesis that all the above-mentioned points can be redefined in a wider perspective.

Such a perspective can be recognized in a tendency, typical of the period between the two world wars, to separate and render autonomous sectors previously intermingled and above all in a widening gap between basic research and "applied psychology". If this difference can be identified as a relationship between short- en long-term planning, we think it was this period that produced the seeds of that crisis in behaviourism - on which a good part of the research from 1920-1940 is based - which in various forms characterised the next 20 years. It is not a question merely of the inadequacy of the various versions of behaviourism on the scientific level. One aspect of the crisis is the statement that the foundation, hypotheses, and presuppositions that lay behind long-term planning did not coincide with the needs and the constraints that would exist 20 years later when people needed to apply much more than the rejection of behaviourism, so much so that we can define it as a realisation, at the moment in which its practical effects appeared, that the planning had been set up wrongly many years previously. Furthermore if attention is turned from research based on behaviourism to the enormous amount of work performed with direct practical goals, this revision will consist of an awareness that it is impossible to reconstruct a psychological science starting from contributions worked out little on a short term basis, considering the complete lack not only of theoretical background but also of scientific power in the sphere of so-called "practical research directed to current problems".

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THE 1895 DEBATE ON THE ORIGINS OF CROWD PSYCHOLOGY (1)

Jaap van Ginneken
University of Leiden

Question: Who was the first modern social scientist to write an extensive treatise on crowd psychology? The answer most histories of social psychology give is that it was the Frenchman Gustave LE BON (1841-1831, whose *Psychologie des foules* was published in 1895.

(2) There can be no doubt that this work was indeed the most influential early study. In fact, it has been called "one of the bestselling scientific books in history". Within three-quarters of a century, there were 45 reprints of the French edition, and 16 translations into foreign languages, including English. Le Bon himself has been described as "the supreme scientific vulgarizer of his generation". The French editions of his 40 works sold almost half a million copies. (3) His famous study drew heavily on earlier treatises of other writers, however, and I hope to give a number of hitherto unknown details on this question.

Most histories of social psychology remain silent of the fact that Le Bon's primacy was immediately contested. At the end of his introduction to the first edition he had claimed that the field was still "bien vierge". But at the end of his introduction to the second edition, he was forced to scale this down to "très inexploité". (4) The reason was that an Italian author had meanwhile accused him of having stolen most of the book. The Italian wrote that it was "una pirateria che è il non plus ultra del genere", and even specified in a review that:

Il 1° capitolo del vostro 1° libro è totalmente copiato come pensiero, Spesso letteralmente copiato anche nella forma. Nelle pagine 12 e 15 voi riassumete l'Introduzione del mio volume: nelle pagine 17, 18, 19, 20, 21, 25, 26, 28, 30, 38, 39, 40, 45, 46, 47 voi copiate le idee che io ho esposto nel mio 1° capitolo.

He also said that Le Bon put the same emphasis on the role of "meneurs" and their "prestige", quoted exactly the same examples from the voluminous work of Hippolyte Taine, etc. (5) The Italian author even announced that he would lodge a formal complaint for copyright violations with the "Société des Auteurs" in both Paris and Milan. Le Bon, however, shrugged off Sighele's claims as preposterous: "Son opuscul est une de ces honorables thèses de débutant fourmillant de citations, mais ne contenant pas une seule vue personnelle". (6)

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The Italian author in question was Scipio SIGHELE (1868-1913). He was at that time just acquiring a reputation as an advocate of the restitution of his native Trentino from Austria to Italy. Today he is mainly remembered for his role in this "irredentist" movement, and as a subsequent spokesman for the "democratic" wing within the short-lived Associazione Nazionalista. (7) Sighele was a young criminologist from the "positivist anthropological" school of Cesare Lombroso, and had been a favourite pupil of his more sociologically oriented associate Enrico Ferri.

It was the latter who published the whole *Polemica sulla Psicologia des Foules* in his scientific journal *La Scuola Positiva*, and who in passing reminded his scholarly readers that he himself had already outlined the field of collective psychology in his early works. Around 1890, when the juridical world in Italy was fiercely debating the long-overdue revision of penal law, Ferri stimulated his students to write monographs on different kinds of extenuating circumstances. It was Sighele who picked up the subject of crimes committed under the influence of others. He was to write a long series of books on the theme of "complicity", ranging from the criminal couple to the criminal association.

The first of these books was *La Folla delinquente*, published in 1891. (8) It differed from Le Bon's work in two ways. On the one hand, it was a rather exhaustive review of all previous pieces on the crowd, and it recognized their respective merits. On the other hand, the novice writer showed obvious trouble in integrating

these heterogeneous parts into a forceful whole, and in sketching a "grand theory". The book, furthermore, mainly focused on the criminological consequences of crowd behavior, although dealing extensively with its psychological causes as well.

It is highly improbable that Le Bon was unaware of the existence of this book. In the first place, the book had already been translated and published in French in 1892. It had in fact, been published by Félix Alcan, who was also to bring out Le Bon's book. Secondly, it had been reviewed that same year in prestigious periodicals such as the *Revue des Deux Mondes*, and had even been reviewed positively by Charles Richet in his *Revue Scientifique*, the same journal which was later to carry two large pre-publication excerpts from Le Bon's book.(9)

Sighele's treatise had also been reviewed by Tarde in the *Revue Philosophique* of Le Bon's friend Ribot. And it was referred to in Tarde's articles on the same subject, *Les Crimes des foules* (in the *Archives d'Anthropologie Criminelle* in 1892) and on *Foules et sectes du point de vue criminel* (in the *Revue des Deux Mondes* in 1893).

Tarde's family still holds much of the correspondence he received during those years. In it I have found a letter from Le Bon (dated July 2, 1894) which confirms that "J'ai lu votre article que je trouve fort remarquable". It should also be noted that Tarde is the only one of these authors whom Le Bon mentioned in his book. This indicates at the same time that he must have been aware of the existence of Sighele's treatise well before he wrote his *Psychologie des Foules*. The chances that he hadn't read it are very slim indeed.

The preceding already pointed to another founding father, who was also briefly accused by Scipio Sighele of having "borrowed" many of his ideas without duly crediting him: Gabriel TARDE (1843-1904). (10) On the one hand, Tarde had already published several pages on crowd behavior in his criminological work *La Philosophie Pénale*. On the other hand, he had pointed to alternative explanations in his social psychological classic, *Les Lois de l'Imitation*

(both published in 1890). Sighele even seems to have asked Tarde's advice before writing his own treatise. In one letter I have found (dated October 17, 1890), the former said to the latter, "Mon premier devoir est de vous remercier pour avoir bien voulu répondre à mes pauvres notes bibliographiques, qui ne méritaient pas cet honneur".

There is, however, more to it than this. In order to understand this, we have to go into the immediate backgrounds of Tarde's first article on *Les crimes des foules*. It had originally been read as a paper at the International Congress for Criminal Anthropology. It would have been more logical for Sighele instead of Tarde to have read such a paper, since his complete review of crowd literature was at that time beginning to be widely debated. The Italians, however, had decided to boycott the August 1892 Third Congress. The reason was that at the Second Congress in Paris in 1889, the French school inspired by Lacassagne had supposedly "unfairly" attacked the concept of the "born criminal" which Lombroso had advocated at the First Congress in Rome in 1885. Thus, it fell upon Tarde to lecture on the topic of the crowd. (11)

Just before he left (on July 29, 1892), he wrote to Sighele:

J'ai beaucoup à vous remercier de m'avoir envoyé la traduction française de votre *folla delinquente*, et aussi d'y avoir parlé de moi en termes si sympathiques. J'ai à m'excuser en même temps de vous avoir pillé en vous empruntant le sujet de mon rapport au Congrès de Bruxelles ... Dans mon rapport écrit je n'ai pu parler qu'incidemment de votre brochure, mais au Congrès j'aurai à en parler plus nettement et longuement sans doute.

However, someone (probably Ferri, who was in Brussels as the only Italian representative, and who had had several arguments with Tarde in these years), reported that the *Folla delinquente* had not been mentioned at all. (12)

Sighele therefore immediately protested to Tarde, and received the following reply (dated September 14, 1892): "Dans mon rapport

vous êtes cité comme vous deviez l'être. Sans doute j'ai été bref sur votre compte ... Je me réservai d'être moins concis dans mon rapport verbal; mais comme j'ai parlé d'abondance ... j'ai été entraîné en des digressions qui m'ont fait perdre de vue votre Folla delinquente. (13)

This frank admission closed the incident for the time being, and in the course of 1894 Sighele and Ferri even exchanged some fraternal letters with Tarde on the subject of crowd psychology, which were subsequently printed in the second (1895) edition of *La Folla Delinquente*. Yet, Sighele could not keep himself from bringing up the incident again in his polemic with Le Bon. Tarde expressed his regret in a letter dated Nov. 18, 1895: "...je ne puis m'expliquer ce brusque revirement que par des influences occultes" (probably a reference to Ferri). He concluded: "Mais j'ai hâte de mettre fin à ce débat que je n'ai point provoqué et qui, en se prolongeant, ne laisserait pas d'être quelque peu ridicule". (14)

After the entire polemic had been published by Ferri, Sighele thereupon wrote an appeasing letter to Tarde, dated Feb. 27, 1896, saying, "Je crois, qu'entre gentilhommes, lorsqu'on s'est expliqué, tout doit être fini, et qu'on doit rester plus amis qu'auparavant". Tarde's answer was apparently in a similar vein, and even gave the impression that he supported the accusations against Le Bon. This is apparent from another unpublished letter by Sighele, dated March 5, 1896, which says:

"Votre dernière lettre est si aimable que je ne sais comment vous en remercier. Il est très confortant pour moi de voir que vous - au moins - ne m'avez pas abandonné! Je n'ai pas besoin de vous dire que tout ce que vous m'avez écrit à propos de M. Le Bon restera entre vous et moi ..."

It should be noted meanwhile, that Sighele and Tarde were lawyers interested in social psychology, whereas Le Bon was a physician endeavoring to link individual psychopathology to social questions. He tried to apply the principles of "dynamic" psychology

and the concept of personality "layers" to the crowd. These were in the process of being developed by his friend Ribot's Paris school (which grew out of the famous debate on hypnotic suggestion between Bernheim's Nancy school and Charcot's Salpêtrière school). (15) The question therefore remains whether - apart from the treatises by Sighele and Tarde (and the earlier fragments on which they based themselves) - there were other attempts at formulating a crowd psychology along these lines before Le Bon's. The answer is yes. And the background of the author has hardly been described in the relevant literature.

A rather oblique reference to this elusive author can be found in Tarde's first paper on *Les Crimes des foules*:

La foule n'est jamais un être 'frontal': à peine est elle 'occipétale'; elle est plutôt 'spinale', comme le dit le Dr. Fournial, d'après le Dr. Lacassagne. Cependant elle se compose d'êtres en majorité 'frontaux' ou 'occipétaux'. Le Dr. Fournial observe avec beaucoup de justesse qu'un foule composée d'adultes a ordinairement quelque chose d'enfantin, de puéril dans ses actes, dans ses colères, par exemple, et ses méchancetés gratuites. (17)

Tarde was a close friend of Lacassagne, who was one of the most prominent French criminologists of his generation. (18) He taught legal medicine in Lyons, and one of his favourite concepts was this notion of brain-parts corresponding to personality layers. It even seems that, in his lectures he himself had applied this idea to crowd psychology, although I have not been able to find it developed in his writing. But just as Ferri had inspired Sighele, so Lacassagne inspired Fournial to elaborate the question somewhat further.

Henry Fournial was born in Trans-de-Provence (Var.) In mid-1889 he was drafted into the army but was permitted to continue his studies at the "Ecole de Médecine militaire" in Lyons. (19) His thesis was accepted on January 27, 1892 and was just like Le Bon's book, called *Psychologie des Foules*. the complete title was *Essai*

sur la Psychologie des Foules; considérations médico-judiciaires sur les responsabilités collectives par le Dr. Henri Fournial. It opened with a chapter on "La Foule et ses facultés" and on "Modificateurs physiques, chimiques, individuels et sociologiques". It continued with a chapter on "Imitation sociale" (contagion, suggestion) et "Maladive" (Folies épidémiques/névroses et Foules délictueuses/ crimes). It concluded with a chapter on "La responsabilité des foules".

All this was very similar to Sighele's approach. And although Fournial in his introduction properly indicated that both Tarde's earlier pages and Sighele's book had been his main sources, Sighele still felt somewhat abused. This was reinforced by the feeling that some French tended to look down upon the Italians. (20) But it was apparently only after Le Bon's acknowledged "theft" that Sighele accused Fournial of acknowledged "plagiarism". In the same letter in which he "made up" with Tarde (February 27, 1896), he reported that he had already sent three letters of protest to Fournial's publisher Storck, "pour avoir ce qui m'est dû", but never received an answer. On March 3rd he wrote that he had just sent a fourth letter, "et si - dans une semaine -je n'aurai pas eu de réponse; j'écirai a M. Lacassagne".

I found out a possible reason why Storck hadn't answered any of Sighele's earlier letters. The publisher was probably simply unable to contact the author who had in fact been with the expeditionary force which had been sent to conquer the island of Madagascar for France. As an army doctor he had plenty of other things to do than worry about his academic credentials. Already in early 1895, no less than 60% of the troops had been hospitalized from tropical diseases. By early 1896, more than 30% of the 15.000 men had died, and Fournial himself returned to France convalescent. (21)

But he didn't stay long. In the early summer he was attached to the Foreign Legion, and in late summer he was sent to Africa, where he was later to join the famous expedition of Foureau with two other expeditions which where to start from Senegal and Congo

respectively. Once again, 20% were killed, and Fournial fell ill himself as well. (22)

All this explains why he did not bother to claim a stake in the early development of crowd psychology. I, in fact, found a promotion report in his personal file in the military archives in Paris which, under the heading "scientific work", carries the mention: "none".

Yet, the same question crops again here: did Le Bon know Fournial's work? The "external" evidence is thin, although coincidentally both were physicians involved in colonial explorations. (23) Furthermore, we have seen that Fournial's study was quoted in Tarde's papers, which Le Bon knew. The "internal" evidence is less thin, though. I already said that the central idea of Le Bon's work was that the influence of the multitude on the individual is comparable to that of a stronger on a weaker person. This supposedly manifests itself in the disintegration of the higher psychic levels, and the upsurge of the lower physic levels. The eighties had produced an abundant literature on this question. There had already been fragmentary attempts to apply it to the crowd, and Sighele had already quoted most of these. (24)

But Fournial clearly tried to develop this line of thought further, for example, in the second part of his second chapter, "Du prestige dans ses rapports avec l'hypnotisme et le somnambulisme provoqué". What Le Bon did was to continue in this same direction. See, for example, the conclusions of Fournial's first chapter on "La foule et ces facultés":

... il y a là un être tout spécial, capable au suprême degré de sentir et d'agir; mais dépourvu d'intelligence ... Le raisonnement lui est chose entièrement étranger, c'est un être, spinal et ganglionnaire semblable à ces animaux décapités, chez lesquels on constate une exagération des reflexes.

Now let us compare the titles in Le Bon's first chapter on the "Caractéristiques générales des foules": "La foule est toujours

dominée par l'inconscient -Disparaition de la vie cérébrale et prédominance de la vie medullaire - Abaissement de l'intelligence et transformation complète des sentiments".

Le Bon claims that this leads to an "orientation fixe des idées". Fournial speaks of a "généralisation exagérée des idées forces". Le Bon says that this leads to an "unité mentale", Fournial speaks of an "unanimité des esprits". Le Bon takes his main examples not only from Napoleon's rule and the French Revolution, but also from the history of Christianity and eastern religions. Fournial does the same. True: many of these had already been quoted in previous fragments, and their reinterpretation closely corresponded to the "spirit of the times". But some of these coincidences seem all to fortuitous. As a matter of fact, it is even highly probable that Le Bon appropriated Fournial's title *Psychologie des Foules*. Opposite the title page of the *Lois Psychologiques* its sequel is announced under the social darwinistic title *Luttes sociales et guerrières des peuples*. So it was only when writing was already in progress that he decided to adopt the title under which the book was to become known. It therefore seems highly probable that Le Bon based his work not only on the well-known Tarde and lesser-known Sighele, but also on the obscure Fournial.

There is one last detail which I would like to point out, even though it exceeds the scope of this paper. By the time Le Bon's ideas were widely used by the Franch Army, and his claim to be the true inventor of crowd theory had become widely accepted (contrary to his claims to be the true inventor of Nietzsche's theory of eternal return and even Einstein's theory of relativity (25) and whereas Sighele had become a well-known politician, Fournial was the first of the founding fathers to be faced with the consequences of these theories in actual practice.

In 1911 colonial troops disembarked at Casablanca to impose Frenche rule. After they had persuaded the Sultan to "accept a protectorate", a French mission was to leave Fez in the spring of 1912. But their departure was delayed by a "violent thunderstorm". Then suddenly

Le 17 avril 1912 vers midi, une bande de soldats chérifiens violemment surexcités se précipitent en armes hors de la kasbah des Chérarda proférant des menaces de mort à l'adresse des Français. D'autres soldats, des gens du peuple, des prisonniers qui s'étaient échappés des prisons, convaincus que l'heure de la guerre sainte, si secrètement et si longtemps désirée, a enfin sonné, se joignent à cette troupe d'énergumènes. (26)

Another source continues:

La fusillade éclate en ville. Au cris des mutins, répondent de partout les clameurs des "imams" et le "youyous" des femmes. Les massacres commencent. Les officiers et sous-officiers français sont assassinés dans la rue ou chez eux ... L'hôpital devient aussitôt le noyau et le réduit de la résistance ... Pendant quatre heures, il avait fallu tenir! C'est là que nous retrouvons l'énergique médecin-major Fournial.

Dès que l'émeute a commencé, il y fait immédiatement mettre l'hôpital Auvert en état de défense. Il y a pris délibérément le commandement de tout ce qu'il a pu trouver: les musiciens de la fanfare du 1er tirailleurs, les infirmiers, les convalescents et même les malades capables de se tenir debout, qu'il a aussitôt armés. Il a reparti sa troupe, assuré la garde des portes, des murs; par son sang-froid et son ardeur, il a su électriser tout ce monde et lui communiquer sa flamme. (27)

He succeeded in holding out until reinforcements arrived, although he was wounded a few months later, supposedly by a "hallucinating" Algerian. He was cited in parliament for his bravery, decorated and promoted to chief physician for the entire Fez region, where a street was even named after him some years later. Towards the end of the First World War he was made medical inspector of the eastern armies, reportedly imposed a "sanitary dictator-

ship" on a large part of the Balkans in order to prevent epidemics from spreading, and was even promoted to general. He died in 1932, and was buried in Le Muy (near Fréjus); a long forgotten fourth founding father of the "Roman" (or Latin) school of early crowd psychologists ... (28)

(With thanks to the descendants of Tarde and Sighele).

Notes

- (1) This paper is part of a longer series of studies on the origins of crowd psychology. Thus, a number of related questions are dealt with elsewhere.
- (2) The current French reprint by the Presses Universitaires de France (Paris) falsely claims to be identical to the first edition. It is in fact a later, slightly edited, version. Curiously enough, the American reprint (The Crowd, Compass/Viking, New York) is closer to the original, except that it has been censored, especially where Le Bon speaks of Christ and Christianity in the same way in which he speaks of other prophets and religions.
- (3) According to the excellent monograph by Robert Nye (1975, p. 3). It was preceded by a study of Le Bon's influence on Sorel (1973), and was followed by a study of Le Bon's influence on Pareto, Mosca and Michels (1977). Le Bon's influence on Freud is a central theme in Serge Moscovici (1982). See also notes 4, 14 and 15.
- (4) In the third edition he came back to the original claim, "il n'existait aucun ouvrage d'ensemble". The primacy question has also been debated by Susanna Barrow in (1981 esp. Ch. 6). This paper contains some important complementary information, however.
- (5) Sighele (1895, p. 171-173).
- (6) Ferri, E. et al. (1895, p. 370-373).
- (7) The best informed contemporary specialist on Sighele is Maria Garbari. See for example her articles (1974, p. 391-426 and

523-561) and her book (1977). Another recent monograph by Enrico Landolfi (1981) is far less serious.

- (8) There is some confusion in the literature as to the title and date. As was usual in those days, subsequent editions were continually enlarged with new material. Finally the book became so voluminous that it had to be divided into two parts, "I delitti della folla" (1901, 1910) and "L'intelligenza della folla" (1903, 1922). This also involved a change in emphasis, probably related to the evolution of Sighele's political convictions.
- (9) Richet subsequently refused to print Sighele's high-handed letter claiming priority. The latter was especially inflamed by this because three years earlier he had done the former a favour by writing a preface to the Italian translation of his science fiction book "Dans cent ans".
- (10) The most complete monograph is the one by Jean Milet (1970). A more specialized and recent study is an article by Ian Lubek (1981).
- (11) Sighele was to hold his talk on Les crimes des foules at the Fifth Congress in Amsterdam in 1901. At that time, he occasionally lectured on the crowd at the Université Nouvelle in Brussels (an unofficial and progressive break-away from the Université Libre). Mrs. Garbari has led me to a series of partly unpublished manuscripts on these lectures.
- (12) According to Sighele in Ferri et al. (1895) p. 372 and contrary to what is claimed on page 157 of the otherwise excellent study by Susanna Barrows.
- (13) Quoted by Sighele in Ferri, E. et al. (1895, p. 372).
- (14) Sighele in Ferri, E. et al. (1895, p. 370).
- (15) Jaap van Ginneken (1982). See also: Yvon Thiec (1981).
- (16) Nye (1975, p. 76) mentions Fournial's book once in passing but gives no further details. Barrows (1981) is the only relevant study dealing somewhat more extensively with his book. But she concedes (p. 131) that she was not able to find any information on his further career - that is, on the particulars which follow here.

- (17) Tarde (1892).
- (18) See for example Jean Patinel (1961). See also: Henri Souchon (1974).
- (19) Necrologies in *Le Matin*, Sept. 5; and *La France militaire*, Sept. 6, 1932.
- (20) Sighele was a francophile, like most of his colleagues. However, there had been a number of incidents, like the lynching of a number of Italian immigrants in Aigues Mortes in 1892. Italy was involved in increasing economic and political conflicts with France, and was realigning itself with Germany, and in fact even with Austria.
- (21) See for example, Gén. Reibell (1935).
- (22) See for example Cdt. Reibell (1903) and (1914). Fournial's role was also quoted by Med. Gén. Jean Des Cilleuls in articles in *Médecine Militaire Française* (feb. 1956, p. 50-52) and *Histoire de la Médecine* (Juin 1956, p. 50-53).
- (23) Le Bon had organized some scientific travels of his own to prospective colonies and written a number of voluminous works about them. He had sought subventions from private and public backers similar to the ones who financed the expeditions in which Fournial had participated. He spoke out in favour of a further extension of the empire, and against assimilation of aborigines. He summarized these views in his first bestseller, "*Lois psychologiques de l'évolution des peuples*" (1894). Fournial too engaged in some scientific research during his expeditions. He collected specimens of plants and animals, and made sketches of the local population. He later lost most of his notes, however, during fights in Morocco. But four of Fournial's drawings can be found in Reibell (1929-30).
- (24) In France, for example, by Charcot's colleagues Règnard and Luys, and in Italy by Sergi.
- (25) Nye (1975, Chapter 6 and p. 88/114 and 156/157 respectively).
- (26) *La Caducée, Colonies/Guerre/Marine*, August 3, 1912 (see also Oct. 5).

- (27) La France militaire, 14 Sept. 1932.
- (28) Some of the second generation authors on crowd psychology did notice Sighele's claim however. See for example R. Park (1972, Chapter 1); R. Park and E. Burgess (1970, p. 384-5); S. Freud (1921).

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THE FIRST PH.D. PROGRAMS IN EXPERIMENTAL PSYCHOLOGY
AT LEIPZIG UNIVERSITY, GERMANY, AND
THE JOHNS HOPKINS UNIVERSITY, U.S.A.

Peter J. Behrens
The Pennsylvania State University
Allentown Campus
Fogelsville, Pennsylvania, U.S.A.

Summary

German higher education of the late 19th century was characterized by two pedagogical principles: *Lehrfreiheit* and *Lernfreiheit*. These principles were present at Leipzig University in 1879 when Wilhelm Wundt directed the first doctoral study in experimental psychology. Prior to this Wundt had supervised philosophical theses. The experimental work by Wundt's first student, Max Friedrich, tested a prediction from Wundt's theory of apperception; that apperception, or recognition time, should be longer for complex visual stimuli than for simple visual stimuli. Friedrich made mathematical calculations based upon the familiar subtraction method and supported Wundt's theory. Friedrich's use of mathematical analyses was consistent with Wundt's conception of the "new" psychology based upon mathematics and natural science.

Graduate education was very different in the United States in 1880, because American education was based on the system of english colleges. Graduate education in experimental psychology after the German model of university began only with the establishment of The Johns Hopkins University in Baltimore under G. Stanley Hall in 1882. Hall had studied at Leipzig and with Wundt prior to his appointment at Hopkins. One of his students, Joseph Jastrow, began research at Hopkins in 1884 and received his Ph.D. in 1886. Jastrow, therefore, was the first American doctorate in experimental psychology. The Hopkins program was similar to the Leipzig program in intent, if not in content. The program encompassed *Lehrfreiheit* and *Lernfreiheit* in independent research, seminars, and disseration requirements. Differences between Hopkins and

Leipzig remained, however, primarily in terms of social and cultural differences between American and European life.

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Although Wilhelm Wundt's experimental psychology at Leipzig University has been documented time and again (e.g., Danziger, 1980), psychologists in general and historians of psychology in particular are less well-acquainted with the nature and content of the Ph.D. program associated with experimental psychology at Leipzig under Wundt. Documentation has been limited to listings of Wundt's doctoral students or personal reminiscences by his students (Tinker, 1932; Bringmann & Bringmann, 1980; Sokal, 1980). The present paper will address the Leipzig Ph.D. at the time of Wilhelm Wundt and compare the Leipzig degree to the first American Ph.D. program in experimental psychology at The Johns Hopkins University in Baltimore. At the same time, some comparisons will be made between the first students in experimental psychology at each institution.

An understanding of the Leipzig Ph.D. begins with an understanding of the German system of higher education during the last half of the 19th century, which was to provide an academic environment in which professors and students could pursue scholarly and research interests in virtually complete freedom. Leipzig University afforded one of the best of these environments, having been founded by rebellious and freedom-minded professors and students in 1409. By the middle of the 19th century it was recognized as one of the finest universities in Europe (Hart, 1874; Eulenburg, 1909). It was also distinctive in size, with an enrollment of about 3,000 students and 150 faculty in four divisions (Theology, Law, Medicine, and Philosophy). Only Berlin was larger of the German universities. Within Philosophy alone there were more than 20 specialties at Leipzig, including Psychology. As a result, Leipzig could boast many notables: Masius in Education, Ludwig in Physiology, Fechner in Psychology.

In 1883 most of Leipzig's students were graduates of German *Gymnasias*, or academic secondary schools. The primary goal of the *gymnasia* was to prepare students for university education through a vigorous study of Latin, Greek, mathematics, natural science, and German language, history, and culture. Students then entered the University at about age 19. An increasing percentage of university students, however, were graduates of *Realgymnasias*, or technical secondary schools, which were established in the 1850's to give a more liberal education. This consisted of modern languages (French and English), some classical language study (Latin), and more mathematics, physics, chemistry, and drawing than offered in the *Gymnasia* (Russell, 1910). Table 1 compares the typical "academic" and "technical" curricula in Prussia at the end of the 19th century.

Table 1
A Comparison of Late 19th Century Prussian
Gymnasium and *Realgymnasium*¹ Curricula
For Nine Grade Levels

Hours Per Week of Instruction²

Course	<i>Gymnasium</i>	<i>Realgymnasium</i>
Greek	36	...
Latin	62	43
French	19	31
German	20	28
Geography/History	26	28
Mathematics	34	42
English	...	18
Natural Science	10	18
Arts	<u>16</u>	<u>24</u>
Nine Grade Level Total	223	232

¹ Also known als *Realschule* in some German states

² Adopted from Russell (1910)

Furthermore, about one-third of Leipzig's students in 1883 were sons of professionals: physicians, teachers, clergy, and lawyers. But this, too, was changing, so that by 1890, only about 25% of university students were sons of university graduates. This reflected a broadening scope of university education toward the technical and scientific education to the trades, civil service, and nonprofessional classes. An additional factor was the low cost of higher education. Leipzig and the other German universities were state-supported and heavily subsidized.

Completion of the secondary school examination (*Reifeprüfung*) permitted university enrollment. Once matriculated, the student could expect to spend three or four years in public and private lectures, seminars, and independent reading before taking examinations for the baccalaureate degree. At Leipzig another three semesters at minimum were required before the doctoral examinations.

German doctoral programs were similar to baccalaureate degrees. Both were based upon the two outstanding features of German university life, *Lehrfreiheit* and *Lernfreiheit*. These were emphasis on academic freedom in teaching and pursuit of scholarly interests, respectively. Student activities, therefore, were also self-directed at both the baccalaureate and doctoral levels. Students could select the courses they wished to hear, follow an individual course of study, and even attend more than one university in pursuit of their academic goals. Examinations for the degrees were taken when the student felt sufficiently prepared. The atmosphere at Leipzig and elsewhere in Germany was aptly described in the following way:

The University is a law unto itself, each professor is a law unto himself, each student revolves on his own axis and at his own rate of speed. (Hart, 1874).

Specifically for the Ph.D. at Leipzig, a number of requirements had to be met. A curriculum had to be arranged by the student which consisted of attendance at public and private lectures. This constituted matriculation. The cost of a course was 16,50 marks in 1883. The distinctive component of the program was, of course, the dissertation. It had to be an original work, accepted by a committee from the appropriate Faculty, and conducted under the direction of a major adviser. Once completed, the dissertation had then to be presented orally to the committee and the Dean of the Faculty. It was then graded on a four-point scale: I = *egregia*; IIa = *admodum laudabilis*; II = *laudabilis*; and III = *idonea*. The categories translate as highest distinction, highly praiseworthy, praiseworthy, and acceptable, respectively. In addition, a doctoral fee, or *Gebühren* of 250 marks was required. Finally a student *vita* was required, which consisted of an autobiography of the candidate, a description of the course of study undertaken, a statement attesting to the dissertation's originality, and request for exceptions, if necessary, from university regulations.

The Leipzig Ph.D. in experimental psychology did not begin when Wundt joined the Philosophical Faculty as Professor of Philosophy in 1875. The ten dissertations he supervised between 1875 and 1879 were not experimental, and only two even addressed psychological problems (Tinker, 1932). Wundt, like other Leipzig professors, supervised a variety of dissertation topics in the division of Philosophy, such as metaphysics, mathematics, and ethics.

Wundt's laboratory, also, was not used for research in the early years of his appointment. His laboratory was a small classroom for demonstrations located in the center of the University in the *Konvikt* building. He drew large numbers of students to his public and private lectures, but his use of this space was in conjunction with his lectures, not for experimental research, either personally or as the director of student research.

A third consideration in understanding the slow start of the

Leipzig program was financial. Wundt did not have budget support from the University to purchase equipment for research in the early years (Bringmann, Bringmann, & Ungerer, 1980).

In the Fall of 1879, however, experimental research began at Leipzig under Wundt. The activities involved Wundt and three graduate students: G. Stanley Hall, Ernst Tischer, and Max Friedrich. Hall already had his doctorate, awarded from Harvard in Physiology (1878) and was in Leipzig to study the "new" psychology; Tischer would eventually receive his Ph.D under Wundt in 1882. Friedrich's research occupied the four men, and led to his degree in 1881. Therefore, Wundt's experimental psychology at Leipzig, and, more importantly for the present paper, the Ph.D. in experimental psychology at Leipzig, were conceived in 1879 and born in 1881. We shall now focus on the events of the seventeen-month gestation period.

Max Friedrich was a 23-year old student from Reudnitz, near Leipzig, when he began his research under Wundt in the Fall of 1879. His study had alternated between Berlin and Leipzig between 1876 and 1879, and he had taken two courses with Wundt: "*Geschichte der neueren Philosophie*" in the Summer Semester of 1878, and "*Elemente der mathematischen Logik*" in the Summer Semester of 1879. During the Winter Semester 1879/1880 Friedrich began experimental research and enrolled in five courses, including Wundt's "*Psychologische Gesellschaft*". Table 2 presents a complete list of the public and private lectures Friedrich attended at Leipzig.

The experiments were conducted between December 1879 and March 1880. Friedrich, Wundt, Tischer, and Hall alternated as experimenters and subjects in a research problem which investigated predictions from Wundt's theory of apperception -- that reaction time should be shorter for simple visual stimuli than for complex visual stimuli.

Table 2
Max Friedrich's Curriculum at
Leipzig University Between 1876 and 1880¹

Winter Semester 1876/77

"Analytical Mechanics"
"Introduction to Mathematical Physics"
"Mathematical Theory of Light"
"Evolution of Spinoza's Monism"
"Physics Practicum"
"Comparative Description and Critique of the Most Important
Systems of Modern Psychology"
"Differential and Integral Calculus"
"Atomic Construction of Molecules"

Summer Semester 1877

"Theory of Finite Integrals"
"Spherical Astronomy and the Use of Astronomical Instruments"
"Analytical Geometry"
"Dynamics of Differential Equations"
"Sensory Illusions"

Winter Semester 1878/79

"Calculus of Variations"
"Selected Topics on the Theory of Spherical Functions"
"Laboratory Physics"
"Descartes, Berkeley, and Kant"

Summer Semester 1878

"Theory of Elliptical Functions"
"Theory of Elliptoid Attraction"
"Partial Differential Equations of the First Order"
"History of Modern Philosophy"

Summer Semester 1879

"Physics I"
"Theory of the Origin of the Species"
"Elements of Mathematical Logic"²
"Advanced Geometry"
"Mathematics-Physics Practicum"

Winter Semester 1879/80

"Problems in Electrostatics"
"Physics II"
"Elliptical Coordinates"
"Theory of Comets"
"Psychological Society"²

¹ from Leipzig University Verzeichniss (1876-1880)

² Courses conducted by Wilhelm Wundt

Wundt's theory of apperception held that a reaction involved several mental processes, including apperception, or recognition. Because apperception time could not be measured directly, two experimental arrangements were required: one which included apperception and one which did not. A reaction to a flash of light (simple reaction) satisfied the latter, while reactions to colors (simple images) or numbers (complex images), presented successively, satisfied the conditions for an apperceptive process. Subtraction of reaction time to the flash from reaction time to the colors or numbers yielded apperception time. This is the classical subtraction method which Wundt derived from F.C. Donders, the Dutch physiologist, and which Friedrich employed in his experiments. A more detailed treatment of Friedrich's experiments is given elsewhere (Behrens, 1980).

Perhaps the most significant portion of Friedrich's dissertation was the mathematical analyses he performed on the results. One calculation was the mean variations in reaction time (*mittlere Variation*) for the different conditions. A second calculation was average errors in reaction times for a special series of stimuli (*mittlere Fehler*). These calculations were departures from standard calculations of average errors in reaction time and represented Wundt's bias that mean variations were the preferable measurements for apperception with complex images (Behrens, 1980).

Following the completion of the experimental portion of his dissertation, Friedrich remained in Leipzig employed as a teacher and student examiner at the prestigious *St. Thomä Gymnasium*. During this time he probably completed the mathematical analyses of his data and wrote the dissertation, because he remained a matriculated student at the University for three semesters, through May 1881, by payment of small continuation fees. His finished dissertation was presented early in 1881 to his committee, which included Wundt as the major adviser and six other members of the *Philosophische Fakultät*: Drobisch of Philosophy, Roscher and Blomeyer of Political

Economy, Voigt of History, and Masius and Van Noorden of Education. In addition, Heinze, past Dean, and Firkel, contemporary Dean of the *Philosophische Fakultät* took part in the evaluation process as prescribed by regulations. Friedrich's work was awarded a grade of "*admodum laudabilis*" (IIa) by Wundt, Heinze, and Fricke, the University *Prochancellor*.

One interesting irregularity was a written request by Friedrich for dispensation from the oral examination, dated 8 February 1881. That the oral examination was an important part of the normal Ph.D. examination was stated in the regulations of the Philosophical Faculty, Article 12, which read, in part:

... The purpose of this examination is primarily to ascertain to what extent the particular ideas formulated in the dissertation are themselves encompassed by the candidate's knowledge in the specialty, as well as chiefly contained in the program of studies.

The time between acceptance of the work and the oral examination shall not exceed one year.

(*Ordnungen*, 1905)

Friedrich may have asked for dispensation because more than one year would elapse between acceptance of the research and the oral examination, or he may have been unavailable for personal appearance within the one-year requirement. He gave no reason for his request, unfortunately.

Friedrich was awarded the doctoral degree in May of 1881, and the complete dissertation was published in the first issue of Wundt's journal *Philosophische Studien* in 1883 (Friedrich, 1883).

A final consideration of Friedrich's work concerns the handwritten comments by Fricke and Wundt. The *Prochancellor* raised the

fact that Friedrich was not a *Gymnasium* graduate, but a graduate of the *Realgymnasium* at Eisenach. Fricke's comment here represents the fact that the type of secondary school education was a sensitive issue for the University administration. *Realgymnasias* were viewed with some suspicion. But Fricke defended Friedrich by saying that the type of secondary education "should not stand in the way" of the doctoral degree, because he had performed satisfactorily for the semesters he studied at Leipzig (Friedrich, 1881).

Wundt's comment described Friedrich's research in general terms, but supported in particular Friedrich's mathematical treatment of the data. Wundt remarked that he agreed with Friedrich's questioning the "possible use of the method of least squares for the calculation of psychophysical results" (Friedrich, 1881). Wundt made these comments and concurred with the dispensation from the oral examination on 14 February 1881.

In the narrow context of experimental psychology Friedrich may have represented the type of student Wundt envisioned for the "new" psychology at Leipzig. Friedrich's education and interests were highly compatible with Wundtian psychology founded on mathematics and natural science. Friedrich wrote in his personal *vitaë*: "In the summer of 1876 I went to Leipzig in order to dedicate myself wholly to the study of pure science" (Friedrich, 1881), and the majority of lectures he attended at Leipzig were in this area (see Table 2). This impressive commitment to the study of natural science and mathematics could not have been missed by Wundt, who, it may be recalled, supervised philosophical dissertations prior to Friedrich's.

In a larger context Friedrich appears to reflect the practical-scientific product of *Realgymnasias* of late nineteenth century Germany. The "high industrial phase" of European higher education was characterized by students whose pursuits were in mathematical, technical, and scientific studies, associated with the demands of a

greatly expanding economic model and social order. Friedrich's dissertation fits this practical-scientific model to the extent that it was largely an applied mathematical problem. Wundt even wrote that Friedrich's work was a "clear and orderly presentation" without "accompanying additional theoretical discussions" (Friedrich, 1881).

We may now turn to a description of graduate education and experimental psychology at The Johns Hopkins University in Baltimore to point out major similarities and differences to Leipzig.

At the time of its opening in 1878 no institution of higher education in the United States could offer the type of graduate training found at Leipzig or, for that matter, any major university in Europe. Although the first Ph.D. in the United States was awarded by Yale in 1861, soon to be followed by Harvard and then the University of Pennsylvania, these Ph.D.'s typically required only some additional coursework and an additional examination. In addition, there was no standardization in advanced degree requirements. President Hadley of Yale, for example, remarked as late as 1904 that there "is not uniformity in the use of the term 'university' in America" (John, 1934). The Johns Hopkins University began a "new order" for graduate education in general and experimental psychology in particular in the United States which was unique, but with roots in the European university model.

The "new order" at Hopkins was the effort of G. Stanley Hall supported by President Gilman. The best components of the European model of higher education were adopted at Hopkins, in addition to regulations which were somewhat stricter for the Ph.D. than at Leipzig. The regulations included at least three years of study in a department in one major and two minor subjects; a thesis conducted under the direction of a major adviser and the Board of University Studies; oral and written examinations in the major and minor areas; written application for the doctoral degree at least one

year prior to the final examination; and certain fees for tuition (\$ 80 per year) and graduation (\$ 10).

G. Stanley Hall's Ph.D. from Harvard in 1878 is recognized by some as the first one in psychology (Harper, 1949). But Hall went to Leipzig to study the "new" psychology of an experimental character under Wundt. Hall's dissertation, therefore, does not qualify as the first one in *experimental* psychology in the United States, just as Harvard can not claim the first Ph.D. *program* in experimental psychology. The recipient, from Hopkins, of the first Ph.D. in experimental psychology was Joseph Jastrow in the year 1886.

Jastrow, an 1882 graduate of the University of Pennsylvania, was one of six "advanced" students which Hall had at Hopkins in 1884. At Hopkins Jastrow served as a graduate assistant and began research at the suggestion of Charles S. Peirce. One study on sensation was published in the *Proceedings of the National Academy of Sciences* in 1884. His Ph.D. thesis of 1886 was conducted in collaboration with Hall and titled "The Perception of Space by Disparate Senses". It was also published in *Mind* the same year. Other notable graduate students at Hopkins during this period were John Dewey, Edmund Sanford, and J. McKean Cattell.

We may finally propose some points of comparison between the Leipzig and Hopkins programs in experimental psychology. The first similarity, and most obvious, concerns research and facilities needed to establish and support the new science. Neither Wundt nor Hall began his laboratory in a grand fashion; both confronted financial and space limitations; but both had a dedication to the "new" psychology which fostered a strong research orientation and potential. In a word, the principle of *Lehrfreiheit* was working in each case of development.

A second similarity is the seminar. The success of graduate

psychology hinged on the performance of capable students in a supportive intellectual environment. Wundt, many years later, acknowledged Hall and Friedrich as important contributors to the success of his *Institut* (Meischner & Eschler, 1979). Jastrow, also, was extremely impressed by Hall and his "encyclopedic sweep in the subjects of his interest" (Murchison, 1930). Such mutual intellectual stimulation reflects well the second principle of university life mentioned earlier, *Lernfreiheit*.

Beyond these similarities there are two ways the Hopkins program differed significantly from the Leipzig program. The first consideration is the 19th century organization of higher education in the United States based on the English system of colleges to train clergy and teachers. The result, unfortunately, was religious sectarianism, geographic isolationism, and narrowly regulated study (Storr, 1953). Experimental psychology, therefore, had to await the "new order" of higher education with The Johns Hopkins University (1878) and later Clark University (1888). These institutions were organized specifically for graduate education and were privately endowed. Financial support of this type provided for flexibility and rapid implementation of programs. Thus, by 1888, James Bryce could remark that the new universities of the eastern United States were beginning to rival the ancient universities of Europe (Clapp, 1950). It is therefore no coincidence that the development of experimental psychology in the United States was closely associated with the emergence of American graduate education.

The cultural differences associated with American and European higher education were present in the personalities of Hall and Wundt, and reflected in the programs they led. Hall's personal qualities of independent thinking and aggressive planning were persuasive for President Gilman to support his new graduate program and laboratory. At Leipzig Wundt "evolved" a laboratory over a number of years, apparently in no hurry to implement experimental psychology. Then, too, Hall encouraged Jastrow in more than one research direction at Hopkins, while Wundt and Fricke wrote specifi-

cally of the unorthodoxy surrounding Friedrich and his work (Friedrich, 1881). The dissertations themselves reflected the extent to which the Hopkins program was "revolutionary" and the Leipzig program "evolutionary" in character. Jastrow expanded psychophysical theory to an original research question; Friedrich tested a specific prediction from Wundtian theory.

Hugo Münsterberg remarked in 1901 that the typical American university was still an extension of the college. This observation, while probably containing some truth, suggests also that Münsterberg's conception of the university was different from that which had emerged in the United States. Münsterberg, a transplanted psychologist, apparently did not see that the European model of university education could not survive that trans-Atlantic journey intact and unaltered. The Johns Hopkins University, for one, embraced the essential components of *Lehrfreiheit* and *Lernfreiheit* in its graduate programs. It could be argued, however, that the unique blend of European and American traditions provided for better expression of these principles under Hall at Hopkins than under Wundt at Leipzig. The paradox is that for some these principles could be a narrow vision for experimental psychology; whereas for others these principles could foster a wide horizon for the very same science.

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ZUR KONSTITUIERUNG DES PSYCHOLOGISCH-
WISSENSCHAFTLICHEN JUGENDBEGRIFFES

Herbert Janig
(Klagenfurt/Österreich)

Summary

The psychogenetic structures of the relationship between parents and adolescents, social factors, and the development and differentiation of scientific psychology have created a "climate" in which the psychological concept of youth could be constituted. The main characteristics of this concept are explained. This concept stresses the developmental aspect, describes "normal" and only male adolescents, defines the sexual development as a crisis and constitutes a negative opinion about youth and adolescence. It essentially influenced the scientific discussion and some common opinions about youth up to the present.

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Meine Beschäftigung mit der gegenwärtigen Jugendpsychologie - ihren theoretischen Konzepten, Themen, ihren Aufgaben und Funktionen innerhalb der Psychologie - die öffentliche Diskussion um die "Jugendprobleme" und die Konkrete Beratungstätigkeit führten mich zu der Frage, wie der psychologische Jugendbegriff entstanden ist und welches wissenschaftlich-psychologische Bild er von "der" Jugend vorgezeichnet hat.

Erst die "antipositivistische Wende" innerhalb der Wissenschaftstheorie macht es möglich, das Beziehungsgefüge zwischen Wissenschaft und Gesellschaft und deren wechselseitige Bedingungen neu zu sehen (Kuhn, 1979). So besteht die gesellschaftliche Aufgabe von Wissenschaft darin, "jene für den gesellschaftlichen Lernprozess erforderlichen Erkenntnisse hervorzubringen, die in den spontanen Erkenntnisakten der Menschen nicht gewonnen werden können" (Bayertz, 1980: 140). Auch die intensive Beschäftigung mit Jugendlichen innerhalb der wissenschaftlichen Psychologie, wie sie nach der Jahrhundertwende vor allem in der deutschsprachigen Psychologie

zu beobachten war, kann man nicht unabhängig von den Veränderungen in der Gesellschaft, in der die Wissenschaftler eingebettet waren, sehen.

Neben wissenschaftsinternen Differenzierungsbestrebungen und den damit verbundenen Selbstbehauptungstendenzen und Positionskämpfen gab es starke soziale Veränderungen für viele Jugendliche und Erwachsene und bemerkenswerte Umstände, die Psychogenese der Beziehungen zwischen Eltern und Jugendlichen betreffend. Diese Ergebnisse sind, wenngleich auch nicht direkt voneinander abhängig, so doch aufeinander bezogen. Der während der "klassischen" Periode der Jugendpsychologie in der Zeit zwischen den beiden Weltkriegen von ihren Vertretern (z.B. Charlotte Bühler, Eduard Spranger, William Stern, Otto Tumlirz, Theodor Ziehen) konstituierte psychologisch-wissenschaftliche Jugendbegriff hat wesentlichen und nachhaltigen Einfluss auf die darauffolgende psychologische Darstellung der Jugend, die Themenauswahl in der Forschung, die Pädagogisierung der Jugend und das gesellschaftlich anerkannte Bild von ihr genommen. Diese Autoren haben einen Jugendbegriff vorbereitet und beschrieben, der bis heute die psychologische und ausserpsychologische Diskussion über die Jugend beeinflusst.

Im folgenden möchte ich Beispiele für die sozialen Veränderungen, die psychogenetischen Merkmale der Eltern-Jugendlichen-Beziehung und die zunehmende Differenzierung innerhalb der Psychologie anführen. Anschliessend sollen die Merkmale des aus diesem Umfeld entstandenen Jugendbegriffe charakterisiert werden.

Vorerst einige Bemerkungen zur Psychogenese der Eltern-Kind- bzw. der Eltern-Jugendlichen-Beziehungen. Wenngleich in der psychohistorischen Untersuchung der Jugend noch entscheidende Defizite bestehen (Mitterauer, 1982), so können doch einige Merkmale zur psychogenetischen Charakterisierung des Verhältnisses der Erwachsenen- generation gegenüber der Jugendgeneration festgehalten werden. Fasst man die bei Ariès (1979) und deMause (1980) angeführten

überlegungen zusammen und berücksichtigt die selbstverständlich vorhandenen regionalen und auf ökonomische, politische und kulturelle Einflüsse zurückzuführenden Variationen sowie die Tatsache der unsicheren Quellenlage und deren Interpretation, so lassen sich die folgenden Charakteristika entwerfen, die die Situation der Jugendlichen und ihrer Eltern in der Zeit vor dem Ende des vergangenen Jahrhunderts kennzeichnen.

Die Eltern-Kind-Beziehungen sind im Verlauf der letzten Jahrhunderte im mittel- und westeuropäischen Raum durch massive Abwehr gekennzeichnet, die sich - psychoanalytisch gesehen - in Projektion, Reaktionsbildung, Verdrängung, Abspaltung, Verleugnung und Intellektualisierung ausdrückt. In dem Maße, in dem das Kind als ein eigenständiges, von den Eltern getrenntes, aber mit ihnen verbundenes und weitgehend von ihnen abhängiges Lebewesen erkannt wurde, konnten die Kinder auch in der Familie belassen, mussten sie nicht weggegeben werden. Die unmittelbare Auseinandersetzung mit den Kindern schuf eine Betroffenheit, in deren Folge sich die Ansicht, dass Kinder einer Erziehung bedürfen, ja ohne sie nicht lebensfähig seien, immer mehr durchsetzte. Das Bewusstwerden der Schwäche und der Unschuld der Kinder führte zu vermehrter organisierter Erziehung, zur Einrichtung von Schulen, die anfangs noch keine altersspezifische Trennung kannten und - im Gegensatz zur Gegenwart - eine rein "männliche Angelegenheit" waren (sowohl die Schüler als auch die Lehrer betreffend).

Kinder zu haben war nun nicht mehr ein Problem des einzelnen oder der engeren sozialen Gemeinschaft, in der man sich befand, das man in seiner je eigenen triebgesteuerten Weise lösen durfte, sondern war ein gesellschaftliches, kulturspezifisches Problem geworden. In immer stärkerem und einheitlicherem Maße fanden die Gesellschaften Mittel und Wege, mit der von ihr empfundenen Bedrohung durch die nachfolgende Generation fertig zu werden. Im Zusammenhang mit der Beschulung hatten mehrere Entwicklungen eingesetzt, die für den von uns heute noch anerkannten Jugendbegriff von Bedeutung sind.

Man erwartete, durch die Erziehung in der Familie, aber vielmehr noch durch die in der Schule, die Heranbildung eines bestimmten Typs von Erwachsenen zu fördern: das Ideal des gebildeten, perfekten und von "niedrigen" Bedürfnisse freien Menschen. Diese Entwicklung ging konform mit restriktiveren Schamgrenzen und massiver Verdrängung sexueller Bedürfnisse, die sich in tiefgreifenden Veränderungen der zwischenmenschlichen Beziehungen und Verhaltensweisen ausdrückte (vgl. Elias, 1981). Zur Erreichung dieses Zieles waren Unterwerfung unter absoluten Gehorsam, Strenge, Einschränkung und Abspaltung gefühlsmässiger Bindungen gerade recht. Rigorose Disziplinierungsmassnahmen hatten aber auch eine Verfestigung und Vertiefung von Standesunterschieden zur Folge: Ehemals kooperatives Zusammenleben wurde durch hierarchische Strukturierung und Alters-trennung abgelöst.

Ebenso haben die familiären Funktionen Veränderungen erfahren, die für die Charakterisierung der Rolle der Jugendlichen bedeutsam sind. Obschon es durchaus problematisch ist, von einem heute gültigen Familienbegriff ausgehend, Veränderungen der Familienstruktur und -funktion in der Vergangenheit beschreiben zu wollen (Rosenbaum, 1982), und allein die Verwendung schichtspezifischer Familienbegriffe schon eine gewisse Klärung des Sachverhaltes erleichtern helfen würde (Mitterauer, 1982), lassen sich dennoch Wandlungstendenzen in der Struktur und Funktion der Familie gegen Ende des vergangenen Jahrhunderts beschreiben, die für die Bestimmung des Jugendbegriffs von entscheidender Bedeutung sind. Sieder nennt zwei Komponenten, die für die Entstehung einer modernen Jugendsituation in den von Produktionsfunktionen weitgehend entlasteten Familien - des städtischen Bereichs - wesentlich sind: "Die Entstehung der Lohnarbeit und des Dienstleistungssektors als Grundlage für die Arbeitsmöglichkeiten 'ausser Haus' sowie die zunehmende Bedeutung des Schulwesens, das den jugendlichen Menschen immer länger in familialer Abhängigkeit hielt. Ersteres bewirkte für den Jugendlichen eine geringere Abhängigkeit in sozialer und wirtschaftlicher Hinsicht, letzteres verlängerte immer mehr die Jugendphase im

Elternhaus" (Sieder, 1976; Vgl. auch Sieder, 1977). Auf diese für die Familie des mittleren bis ausgehenden 19. Jahrhunderts charakteristische reziproke Beziehung zwischen dem Funktionsverlust im Hinblick auf die wirtschaftliche Produktion sowie der Einrichtung verschiedener Formen institutionalisierter Erziehung und dem Schwund ihrer Erziehungs-, Ausbildungs- und Sozialisationsfunktionen weist auch Weber-Kellerman hin (1981).

In der Position des Vaters innerhalb der Familie und im Vater-Sohn-Verhältnis dieser Zeit spiegelt sich diese durch äussere Umstände bedingte Funktionsänderung bei gleichzeitigem Nachhinken der psycho-sozialen Muster in der innerfamiliären Kommunikation wider: Die Väter nun bürgerlicher Familien hatten bei der Auflösung der Grossfamilie und dem Schrumpfen des Hauses auf den Kleinhaushalt der Zweigenerationsfamilie den grössten Teil ihrer Funktionen eingebüsst und ihre "Hausvaterstellung" verloren. Insbesondere scheint es so zu sein, dass die Väter der kleinen bürgerlichen Familien nach Modellen lebten, die für den Familienvater in einem patriarchalisch-hierarchischen Familiensystem vielleicht sinnvoll waren, in einer bürgerlichen Zweigenerationsfamilie unangebracht, der eigenen Familientradition widersprechend und zu einem Ideal hochstilisiert die Väter und damit die gesamte Familie überforderten und somit pseudopatriarchalischen Charakter gewonnen hatten. Die Beibehaltung der Rolle des autoritären, alles bestimmenden "Übervaters" war verbunden mit überzogenen Vorstellungen von der eigenen Person, oftmals massiver Gewalt, Machtstreben innerhalb der Familie und Unterschätzung dieser, genährt aus der Unsicherheit der eigenen Person und der Angst, sich immer neu als Mann innerhalb eines bestimmten Verhaltensmusters bewegen und bestätigen zu müssen. Diese Situation hat zu mittlerweile allgemein bekannten erzieherischen Übergriffen gegenüber Kindern und Jugendlichen geführt und in der Jugendbewegung den Protest einer ganzen Jugendgeneration gegen die überkommenen Lebensformen der Erwachsenenengesellschaft und ihrer Repräsentanten innerhalb der Familie herausgefordert (vgl. Muchow, 1959; Schatzman, 1978; Szemkus, 1974; vgl. auch Weber-Kellermann, 1981).

Diese Wandlungerscheinungen treffen in erster Linie für die bürgerliche Familie zu. Arbeiterfamilien fanden andere Lebensbedingungen vor; denn keine andere soziale Gruppe war in ihrer Lebensführung so unmittelbar an die industriellen Arbeitsverhältnisse gebunden. Das familiäre Leben war im wesentlichen durch lange Arbeitszeit, schlechte Wohnverhältnisse, unzureichende und ungesunde Ernährung und geringe soziale Aufstiegsmöglichkeiten gekennzeichnet: "Eine aktive Sozialisation der Kinder übernahmen schlecht und recht andere Institutionen wie Krippe, Kinderbewahranstalt, Hort und Schule" (Weber-Kellermann, 1981: 139).

Von den sozialen Veränderungen sollen drei näher beschrieben werden: das schon erwähnte gesteigerte Bedürfnis nach Beschulung der Jugendlichen, das Entstehen eines jugendlichen Arbeiterproletariats und die Jugendbewegung zu Anfang dieses Jahrhunderts. In zunehmendem Masse war es einer immer breiter werdenden Mittelschicht vor allem in den Städten seit Mitte des 19. Jahrhundert möglich, ihren Kindern eine höhere Schulbildung zu bieten. Man war von der Erkenntnis geleitet, dass höhere Bildung für die (männlichen) Jugendlichen Grundlage für ökonomische Sicherheit und die Erhaltung und Vermehrung von Besitz notwendig seien (vgl. Gillis, 1980; Ziehe, 1981). Diese Familien waren die ersten, die nach dem Adel eine geringe Kinderzahl hatten, denn die Versorgung der Eltern schien gesichert und die höhere Ausbildung kostete viel Geld.

Das Interesse und die Sorge, die bislang kleineren Kinder gegolten hatte, "schien jetzt auch auf die ältere Jugend ausgedehnt worden zu sein und zwar nicht nur, weil man sich mehr 'Gefühl' leistete, sondern auch aufgrund der Erkenntnis, dass die Investition in eine lange, tere Ausbildung umsichtig geplant und gewissenhaft betreut sein wollte und dass man sie nicht einfach dem Zufall überlassen durfte" (Gillis, 1980: 109). So ergab es sich auch, dass die neue Sorge und das Pflichtbewusstsein, das man seinen jugendlichen Kindern gegenüber zu haben glaubte, zu einer verlängerten Abhängigkeit dieser zu ihren Eltern führte. Zwischen der Verschulung und der neu aufkommenden Sorge um die Probleme und Bedürfnisse Jugend-

licher und deren Beschäftigung mit diesen besteht ein enger Zusammenhang (vgl. auch Sieder, 1976).

Dies betrifft in dieser Form eine zahlenmässig nur kleine, vom Einfluss auf die gesellschaftlichen Vorgänge her gesehen aber wesentliche Schicht, die im wilhelminischen Preussen etwa aus Angehörigen des Bildungsbürgertums und Angehörigen freier Berufe bestand. Wesentliche Kennzeichen zur Typisierung dieser Schicht sind die Selbstreproduktion, das hohe - dem humanistischen bzw. klassischen Bildungsideal entsprechende - Bildungsniveau, starkes *ingroup*-Verhalten, gesellschaftliches Prestige, Elitedenken und Zugehörigkeit zum Protestantismus. Als Randgruppen dieses Bildungsbürgertums können auch Katholiken, gebildete Adelige, die Boheme und geistige und politische Führer der Arbeiterbewegung gezählt werden (Vondung, 1976).

Im wesentlichen war es nur dieser Bevölkerungsgruppe möglich, ihren Kindern eine höhere Schulbildung zu vermitteln. Und hier erhielten vorwiegend auch nur die männlichen Jugendlichen diese Ausbildung, die streng geplant und überwacht wurde. Mädchen hielt man hingegen eher in der Familie, um sie dort für ihre Rolle als Hausfrau und Mutter optimal vorbereiten zu können. Die verlängerte Schulbildung für immer mehr Jugendliche bewirkte auch eine verlängerte psychische, soziale und ökonomische Abhängigkeit dieser Jugendlichen von ihren Eltern.

Neben der immer breiter werdenden Schicht von Jugendlichen, der höhere Bildung zugänglich geworden war, und neben der grossen Anzahl der Jugendlichen, die nach wie vor in der bäuerlichen Agrarkultur verankert war, hat sich im Zusammenhang mit der zunehmenden Industrialisierung, die auf die Städte beschränkt geblieben ist, ein zahlenmässig starkes jugendliches Arbeiterproletariat gebildet; Kinder- und Jugendarbeit mit langen Arbeitszeiten und vorwiegend maschinenabhängiger Arbeit war keine Seltenheit (vgl. Kuczynski, 1982).

Gillis betont, dass es im England des ausgehenden 19. Jahrhunderts neben einer zwar schmalen, aber gefestigten Mittelschicht eine untere Mittelschicht gab, die es vorzog, ihre Kinder lieber in unterbezahlte *White-collar*-Berufe zu drängen, als sie ein Handwerk erlernen zu lassen, wo sie zwar nicht das Ansehen, aber doch das Aussehen von Gentlemen erwerben konnten, wie dies ein Zeitgenosse formulierte. Die Arbeiterklasse war im hochindustrialisierten und relativ reichen England besonders ausgeprägt, wodurch auch die Klasseneigenheiten und -gegensätze besonders deutlich hervortraten. In den oberen Schichten der die breite Mehrheit der städtischen Bevölkerung ausmachenden Arbeiterklasse mass man einer gediegenen Ausbildung für die Kinder und Jugendlichen grössere Bedeutung bei: "Erhebungen aus der Zeit vor 1914 zeigten, dass gelernte Arbeiter eher als ungelernte bereit waren, ihre Kinder lange zur Schule gehen zu lassen, und dass sie auf eine Ausbildung nach dem Schulabschluss Wert legten, sei es in den Abendschulen, sei es durch eine Lehre" (Gillis, 1980: 129).

Zugleich gab es im England des ausgehenden 19. Jahrhunderts viele ungelernte Jugendliche, die als Hilfsarbeiter beschäftigt waren. Es war eine immer grösser werdende Zahl von 14- bis 18-jährigen, die solche Arbeiten annehmen musste und keine Lehre oder höhere Schulbildung genoss. Schulausbildung oder Lehrausbildung erforderte finanzielle Aufwendungen, die sich die Familien solcher Jugendlicher nicht leisten konnten: Sie waren kinderreich und die Väter selbst meist ungelernte Arbeiter. Die stagnierenden Möglichkeiten für besser bezahlte und angenehmere Berufe und die Tatsache abwärts gerichteter Mobilität waren auch die Gründe für die Angst des Arbeiters vor den Jugendjahren.

Die hier für England geschilderten Verhältnisse haben wohl auch mit kleinen Modifikationen für den deutschsprachigen Raum gütigkeit. Die Notwendigkeit materieller Existenzsicherung durch handwerkliche oder industrielle Arbeitstätigkeit war in einigen Fällen mit einem - freilich bescheidenen -sozialen Aufstieg verbunden, ver-

hinderte aber gleichzeitig den Zugang zur bürgerlichen Bildung und damit weiteren sozialen Aufstieg und Anerkennung sowie verbesserte ökonomische Lebensbedingungen (vgl. Popp, 1910).

Die bürgerliche und die proletarische Jugendbewegung des beginnenden 20. Jahrhunderts wird von vielen Autoren als Ausdruck des Widerstandes gegen eine Erwachsenengesellschaft angesehen, deren Werte und Normen sich den geänderten Bedingungen nicht angepasst hatten (vgl. Giesecke, 1981; Hoffmann, 1930). Je mehr die Jugendlichen unter einem Entwürdigenden disziplinären Druck seitens Schule und Elternhaus standen, je weniger sie sich für "voll genommen" fühlten, desto rascher suchten sie dieser Situation zu entkommen. Sie wollten entweder rasch erwachsen werden, um selbst in den Genuss des Erwachsenenstatus zu kommen, oder flohen in die Jugendbewegung. Dort hofften viele Jugendliche den entsprechenden Spielraum für eine eigenständige Persönlichkeitsentfaltung zu finden (Muchow, 1959).

Obwohl die Jugendbewegung zahlenmässig nur einen sehr kleinen Teil der Jugendlichen erfasste, hat sie mit ihren vielfältigen Ausdrucksformen und der breiten gesellschaftlichen Basis, auf der sie gegründet war, erstmals so etwas wie "Jugendkultur" geschaffen (vgl. Rüegg, 1974). Diese hat breiteste Kreise der Öffentlichkeit auf die Existenz Jugendlicher und die von ihr artikulierten Probleme aufmerksam gemacht und das gesellschaftliche Bewusstsein von der Jugend angeregt, was sich schliesslich in einer massiven Abwehrhaltung, gekennzeichnet durch die neu entstehende Jugendpsychologie, die verstärkte Pädagogisierung der Jugendlichen, durch die einseitig negativierende Fassung des Jugendbegriffs und die Ablehnung der Jugendbewegung ausgedrückt hat.

Mit Gillis (1980) kann man annehmen, dass Jugend im Verlauf des vergangenen Jahrhunderts, von einer als selbstverständlich erachteten gesellschaftlichen Gruppierung, immer mehr zu einem, die individuelle und soziale Realität belastenden Phänomen wurde. Das hat

andere, neue Formen des Umgangs mit Jugendlichen erforderlich gemacht. Die vermehrte intellektuelle Beschäftigung mit der Jugend kann als Versuch gesehen werden, durch die Schaffung eines rationalen Ordnungsrahmens mit diesem bedrohlichen Aspekt fertig zu werden. Wissenschaftliche Tätigkeit selbst produziert aber erst wissenschaftliche Tatsachen (vgl. Fleck, 1980; Kuhn, 1970). Wenn man im Sinn von Bayertz (1980) den Produktionsbegriff nicht nur als materielle Erzeugung versteht, sondern in einem weiteren Sinn, so sind auch Beobachtungsdaten, wie sie etwa der Psychologie zur Verfügung stehen, als Produkte zu sehen. Sie werden durch aktive Wahrnehmungstätigkeit aus ihrem Zusammenhang herausgelöst und in Beziehung zu einem theoretischen System gesetzt. Stellen sie sich als reproduzierbare, also überzufällige Ereignisse heraus, werden sie durch spezifische Verarbeitung zu wissenschaftlichen Tatsachen. Insofern schuf die wissenschaftliche Konstituierung des Jugendbegriffs neue Fakten, die in der ausserwissenschaftlichen Realität so nicht wahrnehmbar waren. Die wissenschaftlichen Fakten haben selbst wieder die gesellschaftliche Realität beeinflusst.

Die psychologische Erforschung des Jugendalters ist - verglichen mit der kinderpsychologischen Forschungstätigkeit - ein spätes Ereignis in der wissenschaftlichen Psychologie. Neben den erwähnten gesellschaftlichen Veränderungen derer es bedurft hatte, um in der Jugend ein Phänomen zu sehen, an dem es galt, Bedürfnisse nach Erkenntnisgewinn, Systematisierung und Kategorisierung zufrieden zu stellen, waren auch verschiedene Entwicklungen innerhalb der Psychologie dafür von entscheidender Bedeutung. Vier Denktraditionen innerhalb der Psychologie haben auf die psychologische Erforschung der Jugend entscheidenden Einfluss genommen.

Die Bewusstseinspsychologie (oder Elementenpsychologie) ist - besonders in der zur Zeit des Beginns intensiver jugendpsychologischer Forschung noch weltweit führenden deutschsprachigen Psychologie - die vorherrschende wissenschaftliche Denktradition des ausgehenden 19. und beginnenden 20. Jahrhunderts (vgl. Schmidt, 1981).

Im Sinne Wundts betrachtet die Naturwissenschaft ihre Erfahrungsgegenstände "in ihrer vom Subjekt unabhängig gedachten (objektiven) Beschaffenheit, die Psychologie untersucht sie in ihren Beziehungen zum Subjekt und den ihm von diesem unmittelbar beigelegten Eigenschaften. So kann die Psychologie als Wissenschaft von der *unmittelbaren Erfahrung* definiert werden" (Wundt, 1880, zit. nach Pongratz, 1967: 101; vgl. Schmidt, 1981). Die beiden Hauptmethoden zur Feststellung und Hervorbringung solcher Bewusstseinsstatsachen waren das Experiment und die reine Beobachtung.

Insbesondere Ch. Bühler und ihre Schüler fühlen sich dieser Denktradition verbunden, indem sie die naturwissenschaftliche Methode des Experiments in der Jugendpsychologie als einen Grundpfeiler der wissenschaftlichen Erkenntnis verstanden wissen wollten. Allerdings - und das muss betont werden - stimmt der Begriff des Experiments, wie Bühler ihn in ihren jugendpsychologischen Arbeiten verwendet, keineswegs mit dem Verständnis des psychologischen Experiments überein, wie er heute in der experimentell ausgerichteten Psychologie vorherrscht: Im strengen Sinn können ihre Experimente heute als kontrollierte empirische Untersuchungen oder bestenfalls als Feldexperimente bezeichnet werden (vgl. dazu Preiser, 1976).

Eine zweite Richtung innerhalb der psychologischen Denktradition, die für die jugendpsychologische Forschung Bedeutung erlangt hat, ist jene der Verstehenden Psychologie, der sich besonders Spranger (1924), aber auch Bühler (1975) in ihren jugendpsychologischen Gesamtdarstellungen verpflichtet fühlten. Verstehen meint ein geisteswissenschaftliches Erkenntnisverfahren, bei welchen "geistige Zusammenhänge in der Form objektiv gültiger Erkenntnis als sinnvoll" aufgefasst werden (Spranger, 1924: 3). Sinnvoll ist, was in ein "Wertganzen" eingeordnet ist; einen Menschen verstehen würde dann heißen, "jede einzelne Wesensseite in ihm als sinnvoll in bezug auf das Wertganze seiner individuellen Lebenseinheit deuten" zu können (Spranger, 1924: 5). Spranger geht von der Voraussetzung

aus, dass der einzelne Mensch zwar vieles erlebt, aber wenig von sich selbst und den Zusammenhängen, in die er eingebunden ist, weiss. Dies bleibt einem aussenstehenden (erwachsenen) Beobachter überlassen. Daraus folgert er schliesslich, dass das Verstehen nicht bloss ein anschauliches Nacherleben bleiben kann, sondern zumindest ein kategorial geformtes Nacherleben, d.h. ein Einordnen in Sinnkategorien sein muss. Spranger meint damit über die blosser Subjektivität hinauszugehen, Sinnzusammenhänge verstehen zu können, die zwar das Subjektive Erleben des einzelnen bedingen, aber von ihm nicht erfasst werden können.

Bühler sieht in der Anwendung der Prinzipien der Verstehenden Psychologie das Erleben in den Mittelpunkt ihrer Betrachtung gestellt. Die vorwiegende Verwendung von Tagebuchaufzeichnungen für die Darstellung jugendlicher Entwicklung einerseits und das Abfassen (lassen) von Biografien und deren Analyse über berühmte historische Persönlichkeiten andererseits sollten die Begrenztheit der klassischen Bewusstseinspsychologie überwinden helfen.

Eine dritte psychologische Richtung, der sich einige Jugendpsychologen (so auch Bühler) zumindest in der Verwendung ihrer Erhebungsmethode verpflichtet fühlten ist der Behaviourismus. Bühler hatte den Behaviorismus und seine Vertreter während eines Forschungsaufenthaltes in den USA kennengelernt, einige der Behavioristen studierten bei ihr in Wien (vgl. Bühler, 1965). Sie wollte mit der Methode der "Beobachtung jugendlichen Verhaltens" die durch Tagebuchaufzeichnungen gewonnenen Analysen ergänzen und "objektivieren" (Bühler, 1975: 10f); allerdings sind diese einfachen Verhaltensbeobachtungen - wie sie später feststellt (vgl. Bühler, 1931) - bei Jugendlichen fast nicht mehr anwendbar.

Eine vierte psychologische Richtung, in der man sich mit der Jugend beschäftigte, war die Psychoanalyse. Den Beginn stellen Sigmunds Freuds "Drei Abhandlungen zur Sexualtheorie" (1905) dar. In dieser Arbeit beschreibt Freud die Pubertät als Lebensperiode, in der die

Veränderungen einsetzen, "welche das infantile Sexualleben in seine endgültige normale Gestaltung überführen sollen" (Freud, 1972: 112). Diese Veränderungen sind nach Freud dreierlei: das Zusammenwirken der bislang mehr oder weniger unabhängig voneinander wirkenden Partialtriebe zur Erreichung des Sexualzieles und die Unterordnung der erogenen Zonen unter den Primat der Genitalzone; die Bestimmung neuer, für beide Geschlechter verschiedener Sexualziele; die Wahl von Sexualobjekten ausserhalb des Kreises jener Personen, mit denen der Pubertierende durch die Inzestschranke verbunden ist.

Während nach der allgemeinen und der Auffassung der zeitgenössischen Psychologie das Jugendalter bzw. die Pubertät als Beginn und Ursprung des sexuellen Lebens anzusehen ist, hat Freud mit diesen Ausführungen entscheidende Hinweise für das Verständnis bislang unverstandener Entwicklungsvorgänge und Verhaltensweisen gegeben. Die wissenschaftliche Beschäftigung mit dem Unbewussten, das von Freud behauptete Faktum der infantilen Sexualität und das Betonen der Bedeutung der libidinösen Entwicklung für den Menschen hat bei den zeitgenössischen Jugendpsychologen - die alle zumindest die "Drei Abhandlungen zur Sexualtheorie" kannten - zu heftigen Gegenreaktionen geführt. Der Libidotheorie wurde der Anspruch unterschohen, das gesamte Erscheinungsbild der Adoleszenz erklären zu wollen (ein Anspruch, den die Psychoanalytiker selbst nie erhoben) und die Argumente für die Zweizeitigkeit der sexuellen Entwicklung wurden negiert. Insbesondere Siegfried Bernfeld hat sich kritisch und differenziert mit den Auffassungen seiner zeitgenössischen Kollegen, die sich mit der Psychologie des Jugendalters beschäftigten, auseinandergesetzt und aus der Sicht eines Psychoanalytikers über sie ein vernichtendes Urteil gesprochen (Bernfeld, 1974).

In diesem so skizzierten Umfeld wurde in der Psychologie ein Jugendkonzept entwickelt, das im wesentlichen folgende Kennzeichen aufweist:

(1) - Die Suche nach dem "normalen" Jugendlichen. Die Psychologen dieser Zeit wünschten ein Bild von "normalen" Jugendlichen zu

entwerfen, sprich: einer idealisierten Gestalt des Jugendlichen, der nachzueifern lohnend erschien und der Pädagogisierungstendenz gegenüber Jugendlichen Rechnung trug. Dementsprechend befassten sich die Autoren vorwiegend mit der geringen - nur zehn bis fünfzehn Prozent der Jugendlichen umfassenden - Zahl der Jugendlichen aus bürgerlichen Mittelschichtsfamilien (so Bühler, Hoffmann, Spranger, Tumlirz). Dieses Vorgehen stiess schon bei Zeitgenossen auf teilweise heftige Kritik (vgl. Bernfeld, 1974; Lazarsfeld, 1931). Diese Einseitigkeit der wissenschaftlichen Analysen wurde von den Angegriffenen mit einigen Argumenten aus der wissenschaftlichen Welt geschafft: die Psychologie sollte sich mehr mit den "normalen" Menschen, nicht mit den "Höhen- und Tiefenlagen" beschäftigen (Hoffmann, 1930); man könne in der Beschränkung auf gebildete Jugendliche die psychische Charakteristik der Jugend überhaupt besser erkennen (Spranger, 1924), und schliesslich sei es für den Psychologen sehr viel schwerer an jugendliche Arbeiter und Bauern heranzukommen als an Gymnasiasten, weil sie zu Fremden - und solche stellen die Psychologen für diese Jugendlichen dar - kein Vertrauen fassen können (Tumlriz, 1930). Eine derartige Einschränkung auf gebildete Jugendliche hatte auch den Effekt der Bevorzugung einer spezifischen Forschungsmethode, der Tagebuchanalyse, auf die hier aber nicht näher eingegangen werden kann (vgl. dazu Bernfeld, 1931).

(2) - Die einseitige Betonung des Entwicklungsaspekts. Praktisch von allen wichtigen Autoren werden die Jugendlichen in einer individuumzentrierten Sichtweise als unreife und unfertige Menschen beschrieben. Für Spranger (1924) bedeutet das Jugendlich-Sein ein Zwischenstadium "zwischen der typisch unentfalteten Geistesstruktur des Kindes und der fertigen Geistesstruktur des erwachsenen Mannes oder der Frau" (Spranger, 1924: 18). Jugendlicher zu sein hiess demnach, sich von einem Zustand partieller Nicht-Existenz langsam an das Mensch-Sein anzunähern. Für Bühler ist das Jugendlich-Sein ein Markstein in der gesamten Entwicklung des Menschen zwischen der unbewussten Kindheit und dem überhöht-idealisiert gesehenen Erwachsenenalter: das Kind sei quasi noch "ausserhalb der Welt", der

Jugendliche wachse "in sie hinein" und der Erwachsene sei schliesslich "in ihr".

(3) - Die negative Einstellung gegenüber der Jugend. Die allgemein weit verbreitete Abwehr dessen, was Jugend ausdrückte und von ihr artikuliert wurde, war sehr gross. Am deutlichsten hat dies wohl Spranger formuliert und so zur wissenschaftlichen Wahrheit erhoben, wenn er über den Jugendlichen meinte: "Er ist in Wahrheit noch nichts" (Spranger, 1924: 150). Auf der Grundlage eines so durch Vorurteile belasteten Feldes konnte natürlich eine Vielzahl wissenschaftlicher Lehrmeinungen entstehen, die Jugendliche in einem negativen Licht sahen und ihnen alle jene Eigenschaften und Verhaltensweisen zusprachen, die man entweder nicht an sich selbst sehen konnte, also als Projektionen wirksam wurden oder für die auch die sie bedingenden Zusammenhänge familiärer und gesellschaftlicher Art nicht erkannt wurden.

Der Erwartungsdruck gegenüber Jugendlichen war sehr hoch und wurde durch das Fehlen realer Vorbilder in unangenehmer Weise verstärkt (Hoffman, 1930). Dabei wurden die Schwierigkeiten, die Jugendliche hatten, mit Akribie verfolgt und beschrieben. Allerdings hat dies die allgemeine Verständnislosigkeit gegenüber ihren Problemen und deren mögliche Lösung keineswegs vermindert, sonder scheint im Gegenteil diese Unfähigkeit nur verdeckt oder gar verstärkt zu haben. Spranger meint dazu resignierend: "Man kann dem jungen Menschen ernstlich nicht viel helfen" (Spranger, 1924: 152). Auffallend ist an den Beschreibungen jugendlichen Verhaltens die Betonung von depressiven Verstimmungen, Einsamkeit, Trauer und Hilfsbedürftigkeit (vgl. die Ansichten Ziehens zum Suizid bei Jugendlichen, 1927).

(4) - Die Abwehr des Sexuellen: Die Einstellungen der akademischen Jugendpsychologie zur Sexualität kann bestenfalls mit "ja, aber" charakterisiert werden. Die sexuelle Entwicklung wird übereinstimmend als Krise, ja als Katastrophe verstanden (vgl. Bühler, 1975; Ziehen, 1927; vgl. aber auch die heute gängigen Katastrophen-

theorien in der Jugendpsychologie: Ewert, 1983). Die Einstellung der wissenschaftlichen Jugendpsychologie gegenüber der jugendlichen Sexualität stand unter dem Einfluss der starken Sexualfeindlichkeit der Gesellschaft, welche die sexuellen Kontakte der Jugendlichen einschränkte und die sexuelle Not der damaligen Jugend bestimmte (vgl. Muchow, 1959). Die grosse Unterschiede in den Auffassungen der Vertreter der einzelnen psychologischen Theorien in bezug auf die Bedeutung und die Folgen der sexuellen Entwicklung werden vor allem von Bernfeld (1974) in einer geistvoll-kritischen Abhandlung gekennzeichnet. Er setzt sich dort auch mit der Psychoanalyse-Rezeption seiner Kollegen auseinander und wirft ihnen vor, die wesentlichen Aussagen der Psychoanalyse zur sexuellen Entwicklung nicht verstanden zu haben.

(5) - Die Männlichkeitsideologie. Die Beeinflussung der "klassischen" Jugendpsychologie durch die - prinzipiell männliche Jugendliche betreffende - bürgerliche Jugendbewegung und die zeitgemässige Minderachtung der Frau haben es mit sich gebracht, dass sie sich fast ausschliesslich um die Darstellung männlicher Jugendlicher bemüht hat (z.B. Hoffman, 1930; Spranger, 1924; Stern, 1925). Nur wenige Autoren haben auch die Entwicklung der weiblichen Jugendlichen berücksichtigt (Bühler, 1975; Tumlirz, 1927). Die Tatsache, dass sich hauptsächlich Männer als Jugendforscher betätigten, hat wohl auch einen Einfluss auf die Bevorzugung der männlichen Jugendlichen: Hoffmann steht auf dem Standpunkt, er könne als Mann nicht über die seelische Reifung der weiblichen Jugendlichen schreiben. Tumlirz hingegen schafft dies, indem er die Beobachtungen seiner Frau in seine Beschreibungen einbaut. Studien die sich ausschliesslich mit der Lage weiblicher Jugendlicher befassen, sind selten und erscheinen auch relativ spät: Franzen-Hellersberg (1932) kritisiert ein erschreckendes Desinteresse der zeitgenössischen Forscher an den Problemen der weiblichen Jugendlichen.

Die psychogenetischen Beziehungsmuster zwischen Eltern und Kinder, die sozialen Bedingungen und die Entwicklung in der wissenschaftlichen Psychologie liessen die Jugendpsychologen der "klassischen" Periode ein Konstrukt Jugend schaffen, das für die folgende Jugendforschung und den praktisch-pädagogischen Umgang mit Jugendlichen entscheidende Weichenstellungen bewirkte.

Die hier skizzierte Konstituierung des psychologisch-wissenschaftlichen Jugendbegriffs hat wesentlichen Einfluss auf die folgende wissenschaftliche und ausserwissenschaftliche Diskussion um das Phänomen Jugend, stabilisierte den Jugendbegriff in einer bestimmte Richtung, war ausschlaggebend mitbeteiligt für die Bildung des Generationskonzeptes, für die immer stärker werdende Beschulung Jugendlicher und die allgemeine Auffassung von der Notwendigkeit ihrer Erziehung und Betreuung, die Ansicht, dass Jugendlicher zu sein etwas ausserhalb des Normalen befindlichen sei und man daher Jugendliche in die Gesellschaft zurückführen müsse und verstärkte schliesslich die Ausblendung dessen, was "Jugend" im psychologischen Sinn bedeutet.

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'INHIBITION': NOTES ON CONTEXT IN THE
NINETEENTH CENTURY

Roger Smith

Department of History
University of Lancaster

Summary

The term 'inhibition' was used widely in nineteenth-century psychology, physiology and medical psychology as a way of conceptualizing controlling relations between different parts, and particularly the anatomical and evolutionary levels, within the nervous system. This usage reveals that technical and experimental science shared a conceptual structure with everyday or popular psychologies of human nature. The paper surveys the history of inhibition as an integral part of the history of the individual as a selfregulating agent in society. It shows how experimental neurphysiological research on inhibition added an empirical dimension to existing theoretical representations of 'higher' functions (of brain or mind) restraining 'lower' processes. This conceptual structure existed in a variety of national, cultural and medical settings, continuing into the present century.

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The history of inhibition is the history of a concept which is both technical and popular, both philosophically abstruse and socially specific. the term 'inhibition' ('*Hemmung*'), a key concept in the nineteenth-century 'mental sciences', had meaning within institutionalized, or professional, experimental studies and within public and 'commonsense' representations of the social individual.¹ This is not an argument that 'external' as well as 'internal' factors were at work in physiology and psychology; rather, I wish to replace that crude historiography by describing a common discourse - a single conceptual possibility across a range of apparently disparate social locations. Underlying this approach is the presumption that the history of nineteenth-century psychology is a history of representations of human action, with all the social and political dimensions that entails.

The present discussion is a prolegomenon to a more extended historical study, which I hope will appear soon. I will therefore indicate here only some main points, without close attention to detail and methodology. But this should suggest why the topic has significance.

The concept of inhibition acquired prominence in nineteenth-century attempts to conceptualize relations between physiological and psychological explanation, and - in a logically connected issue - relations between determinist causation and voluntarist actions in human life. These dual issues were recurrent, but a newly systematic and empirically informed neurophysiology in the nineteenth century gave them unprecedented urgency. Most writers found a mechanistic view of human nature logically, morally and socially intolerable. Many of them utilized 'inhibition' as part of a structure of mediations between superficially incompatible directions in psychological explanation. Further, I wish to stress that this structure was common to technical science and to popular informal psychology. Questions of the meaning of 'inhibition' are therefore questions which the historian should address to 'the common context'.

It is no coincidence that it is writers advocating a *physiological* psychology who have attributed historical significance to a concept of inhibition. The Americans Franklin Fearing and Raymond Dodge in the 1920s believed that the failure to elucidate the mechanism and function of inhibition was central to psychology's failure to establish its neural basis. Fearing, for example, referred program-matically to the psychological activity of attention as a neural 'facilitating and inhibitory process'.² somewhat later, but with broadly the same programme in mind, Solomon Diamond and his collaborators argued 'that the full potential of the concept of inhibition could not be realized by psychologists until it was given a sound physiological basis'.³ In addition, Diamond argued for inhibition as a fundamental psychological relation, of equivalent

status to (and the mirror image of) excitation. This, I believe, gave due recognition to the a priori theoretical characteristics in nineteenth-century thought: the concept was necessary to any view of human individuals as controlling systems, where the option of non-action or ceasing actions balances action. Inhibition's theoretical quality as a controlling relation explains how the term could convey common meaning within technical science and everyday psychology: it represented individual action in terms of control. Put another way, if 'excitation' and 'inhibition' by definition refer to relations of a controlling character, then these relations may be expressed equally in popular terms (e.g. conduct), psychological terms (e.g. response), or physiological terms (e.g. motor innervation). Thus, modifying a nineteenth-century definition, 'by inhibition we mean the arrest of the functions of a structure or organ [or conduct or response], by the action upon it of another, while its power to execute those functions is still retained, and can be manifested as soon as the restraining power is removed'.⁴

The conceptual standing of inhibition, as a negative controlling relation, is rarely perceived in the literature. Instead, commentators - expressing the dominant scientific ethos - prefer to focus on the empirical discovery of inhibition. Indeed, the discovery by the brothers E.H. and E.F. Weber of the vagus nerve's depressive action on the heart beat is a classic of experimental physiology, repeated as a model by generations of students. But this approach makes it very difficult to follow the subsequent development of research on inhibition in studies of the nervous system and in psychology. The consequence is a variety of historical accounts, covering a vast range of only partly overlapping research, which make it very difficult to see any sense in which the study of inhibition is a single topic.⁵ Reading these sources is a very confusing experience since there is no agreed delimitation of the subject area. Such confusion is, I think, the inevitable result of searching for the linear development of an empirical discovery, when in reality what is 'discovered' exist as part of the concep-

tual structure of popular physiological and psychological accounts of controlled action. Something of this was perceived by Hebbel E. Hoff, in his detailed study of vagal inhibition, when he observed that, after 1850, 'the concept of inhibition was returned [from theories of the heart beat] to the central nervous system, *where it had in reality originated*'.⁶

Reinterpreting Hoff's remark, consideration of the mind and nervous system as controlling processes had established the context in which empirical research on inhibition could flourish. I will now support this contention with a historical overview, attending particularly to 'inhibition's' range of usage.

The English term 'inhibition' was not in common use in the first half of the nineteenth century, its earlier meanings referring either to a legal or ecclesiastical action of forbidding, or to hindering generally, including loosely physiological senses as in 'inhibition of spirits'. The German verb '*hemmen*' was in much wider use to indicate both mechanical opposition or resistance and personal or psychological prevention.⁷ In English 'inhibition' did not convey any special implications not carried by other words and thus, when it became more common, it simply took over describing one (psychological or physiological) function hindering another. As far as I know, it came into common physiological use after a discussion by Joseph Lister of Pflüger's 'inhibitory system of nerves' (*Hemmungs-Nervensystem*) in 1858.⁸ Whatever the truth of this, the point is that physiological usage promoted a word to take over pre-existing meanings. 'Inhibition' came to provide a focus for theorizing on hierarchical control arrangements, whether expressed in physiological, psychological, or a mixed psycho-physiological language. To argue this point historically requires knowledge of popular psychological works and medical texts, often in themselves ephemeral, designed as guides to the workings of body and mind in conduct (which is of course moral and social). I have detailed knowledge of this literature only for Britain, but I suspect that the literature is duplicated in other national cultures.

It was a commonplace for writers concerned with good habits and conduct to discuss the conditions in which 'higher' mental powers could or should control (the ambivalence was intrinsic) 'lower' bodily functions. A genre of moralistic writing in the 1830s and 1840s (often influenced by phrenology) sought to popularize physiological understanding as a prerequisite of right conduct.⁹ Clarification of the spinal reflex in the 1830s provided such writing with a particularly rich empirical resource. The English physician Marshall Hall turned earlier physiological commonplaces into a specific theory of nervous functions: a lower, reflex (or 'excitomotory') system, and a higher, volitional (or 'sentient and voluntary') system, each with a supposed separate neuroanatomical basis.¹⁰ As a contemporary of Hall's expressed it, writing in the idiom of British natural theology, 'the function of the "true" spinal cord, and that of the brain, would appear to antagonise, and for a wise and salutary purpose'.¹¹ The antagonism of bodily and moral action exhibited a 'design' for preserving life while leaving scope for Christian conduct. The capacity of the higher functions, and conversely, the incapacity produced by 'bad habits', alcohol and insanity (when the lower functions were no longer controlled), established the discursive space later described by the term 'inhibition'. After 1845 the neurophysiological concept of inhibition, elaborated through detailed experimental and clinical studies of the functional relations between levels in the central nervous system, provided hierarchical theories of control with a new objectivity and scientific authority.

From the 1830s a number of British physicians - especially those concerned with 'medical psychology' (broader than what would now be called 'psychiatry') - made a concerted attempt to establish and communicate a physiological dimension for psychology.¹² In part this served their own interest in achieving professional authority; more significantly, it provided an objective rhetoric for Victorian moralism: it was a system for the 'naturalization of values'. W.B. Carpenter's textbook and periodical writings provide one of the

clearest cases: he described a hierarchy of levels of nervous action, the will acting via attention to regulate the formation of good or bad habits (whose physiological substratum was the reflex). He provided a rich resource for documenting the ways in which individual people were (or should/could be) in control. Importantly, he and other Victorians (such as the alienists J.C. Bucknill and Henry Maudsley) considered the lower nervous levels to be automatically active, unless controlled - or 'inhibited' - by the higher brain levels influenced by attention.¹³ Late nineteenth-century advocates of 'mental hygiene' viewed preventive psychological medicine as the fostering of the conditions of inhibition. As Charles Mercier observed in 1888, 'inhibition' had become a general theory, established both a priori and empirically, of healthy psychological function: 'The frequent use of the phrase "Loss of control" ... has surrendered the whole position. For control to be lost it must first be present; and if maintained upon some centres, why not upon all? The hypothesis [of general inhibition] already exists, therefore, and is already largely accepted; and indeed, it is of such indispensable necessity to a comprehension of the physiology of the nervous system, that ... "If it did not exist it would be necessary to invent it."¹⁴

Mercier's mentor was the neurologist John Hughlings Jackson. Jackson had a decisive influence on British neurology in both the short and long-term, introducing order in the classification of automatistic and paralytic symptoms by equating a diachronic evolutionary hierarchy of control functions with the synchronic hierarchy of levels within the individual nervous system. As he described it, disturbance of function was the 'removal of control', beginning with the most recently evolved centres, when the reduction or loss of inhibition permitted the free play of lower and more primitive levels.¹⁵ As is well known, Jackson's work was of direct importance to Freud, and it is now widely argued that Freud's neurobiological and clinical studies established a conceptual framework perpetuated in the psychoanalytic period.¹⁶ Less

well known is the fact that Jackson's work also re-emerged as a significant factor in American neurobiology in the 1940s, particularly in conceptualizing the 'limbic system' - a supposed level or centre for emotional expression, supposedly 'released' by destruction of cerebral functions.¹⁷

Jackson's work is so obviously central to scientific neurobiology that it is worth recalling that he customarily used the metaphor of government to explain what he meant by the higher control of lower functions. More especially, he referred to the removal of government control to illustrate a state of anarchy - corresponding to neurological disorder.¹⁸ This was no idle whimsy. As I have argued, representing human nature in terms of the mind (or its nervous correlates) inhibiting inherently active automatic functions was also an ideology of the individual's relation to society. The metaphor of government in neurophysiology carried meaning owing to the common structure of hierarchical control arrangements in the political and psychological spheres.

The equation of government and inhibition existed in contexts which, though independent, shared elements of the language of power. I will illustrate this by reference to other nineteenth-century usages of 'inhibition'. German philosophical psychology understood the logical principle that the formation of mental content required exclusion as well as inclusion. The British associationist tradition in psychology gave this point comparatively little emphasis since the differential aspect of the formation of mental content was built into the laws of association. By contrast, the Leibnizian tradition, which attributed self-activity to the elements ('*Vorstellungen*') forming mental content, posed strongly the question of the interaction, balance and opposition between these elements.¹⁹ J.F. Herbart used the verb '*hemmen*' extensively in this context. Simplifying to psychological terms (which distorts the extensive but incoherent philosophical setting), Herbart argued that conscious unity occurs with the balance of '*Vorstellungen*' to

the extent to which their 'self-preservations' ('*Selbsterhaltungen*') do not inhibit each other. He used two apparently distinct metaphors to discuss this process; but the metaphors share the relational quality which, as I have insisted, is a logical property of conceptualizing the function of control. First, he thought in terms of the mechanics of forces between moving bodies, suggesting a dynamics of consciousness; this also suggested a manner in which psychology might be quantified and thereby transformed into 'science' - in the way that Kant, Herbart's predecessor at Königsberg, had ruled out.²⁰ Second, Herbart thought in terms of political government - '*die Statik und Mechanik des Staats*' - understood equally to be about the balance of forces, with balance achieved through interaction and inhibition.²¹

Herbart's work, and the principle of the competition of sensory elements in the formation of mental content, was well known to psychologists after 1840, but the degree of direct influence on psycho-physiological accounts of inhibition is unclear to me. At present it needs to be noted only that the prominent late nineteenth-century theoretical and experimental topics of apperception and attention perpetuated discussion of inhibition in psychological terms. Wundt, for whom 'apperception' ('*Apperception*') was the intrinsically active (and logically and empirically synthetic) process in mental content, eventually concluded that its elementary basis might exist as the inhibitory relations within the manifold of conscious contents.²² 'Inhibition' also appeared in other psychological contexts: for example, G.E. Müller used the term in describing factors affecting memory retention and loss.²³

German experimental psychology in the second half of the nineteenth century was intellectually and institutionally separated from psychological medicine. But this was not so earlier: Herbart himself was only among who wrote about insanity in terms of the weakening or destruction of higher functions, or the loss of control over animal faculties.²⁴ As mentioned, British moralists used

the same structure of argument to warn about the effects of drink, indulgence, immorality and so on. In the 1840s the young German alienist Wilhelm Griesinger used Herbartian concepts (including inhibition) to theorize insanity in terms of incapacities of the Ego. Indeed, these concepts preceded and underlay the contribution for which he is now better known, the turning of German psychological medicine in a physicalist and academic direction.²⁵

Enough has been said to indicate that 'inhibition' existed as a concept in a variety of linked settings prior to its 'discovery' as a specific empirical phenomenon in 1845. But even within the specialized and technical area of physiology it had long been understood that the brain might act by hindering as well as exciting the nervous system. Physiological writers such as Whytt, Prochaska and Magendie had established the notion of a nervous organizational hierarchy. In addition, clinical and experimental evidence that removal or damage to the brain could increase activity at lower levels implied that control involved prevention as well as initiation of action. In the 1830s it was accepted to be a physiological commonplace that lower level functions were more active after removal of the brain in animals.²⁶ Reflex action theory enabled this to be expressed with a new precision, generating a research programme on the anatomical relations and functional regulation of the spinal reflexes, including the relation between volition and spinal processes.

Nevertheless, prior to 1845, the arguments for inhibition (or its analogues) were logical and functional. The experimental demonstration of nervous inhibition of the heart beat immediately precipitated the search for (a) a specific inhibitory mechanism, and (b) specific sites of inhibitory action (i.e. for localization of function). The programmes were never really distinct, though the former aspect encouraged a great deal of speculation, resulting in general agreement only in the present century with research on chemical inhibition at the neurone synapses. I will say no more

about this. The latter aspect led to the description of many inhibitory effects and even, for a time, the postulation of specific structures within the nervous system. One cannot conclude however that there was a simple relation between the Webers' discovery and the subsequent discoveries and accounts. Rather, physiological theories of organization and control acquired their characteristic experimental quality in the light of the discovery of vagal inhibition.

Perhaps largely for experimental reasons, most early specific studies of inhibitory effects concentrated on what is now known as the automatic nervous system, i.e. on the regulation of functions and reflexes associated with involuntary activity. Eduard Pflüger described the inhibitory effect of the splanchnic nerve on the contraction of involuntary muscle in the gut; the regulated nature of such automatic movements then led him to propose the existence of an inhibitory system of nerves.²⁷ Such was some researchers enthusiasm for inhibitory effects at one time that Goltz expostulated, 'let us resist this flood of inhibitory nerves!'²⁸ From the point of view of understanding the regulation of life-sustaining functions, three sets of researches were of special importance: continued study of the heart beat; Claude Bernard's work in the 1850s on the opposed action of vasoconstrictor and vasodilator nerves; and in the 1860s a series of studies (especially by Rosenthal, Hering and Breuer) showing that extension of the lung inhibited inspiration.²⁹ Such studies translated a conception of control, namely, the balance of excitation and inhibition, into detailed physiological terms.

At no time however were these studies of involuntary function completely separate from the psycho-physiology of control in the whole organism. E.F. Weber had noted in 1846 (directly following his statement on the inhibition of the heart beat), that 'the experience that the will restricts spasmodic convulsions, if they don't occur too strongly, and can inhibit the origin of many reflex

movements, which occur more easily if the brain is deprived or stunned than with whole or uninjured animals, also demonstrates that the brain can have an inhibitory action on movements'.³⁰ This question of central nervous regulation was bound-up inextricably with structural and functional research on the spinal cord, levels in the brain, and the interaction of reflex functions. It was against this background that the Russian physiologist I.M. Sechenov in 1863 reported his famous experiment purportedly demonstrating the existence of a localized inhibitory centre in the mid-brain region. Such an apparently precise experimental discovery again focused research and theoretical attention; but, as I have emphasized, this focus was possible because of the many ways in which control (both voluntary and involuntary) was already conceptualized in terms of a higher inhibitory effect on lower levels.

Sechenov studied physiology at various European centres, and in 1862 worked in an ante-room to Bernard's laboratory (though Bernard was strikingly uninterested in anyone else's research). He transected a frog's neuraxis at the mid-brain level, suspended the frog above an acid bath, and then measured the speed of reflex action to irritation. He showed that a salt crystal placed on the posterior cut end of the mid-brain inhibited the spinal reflex, and he claimed that this demonstrated the existence of a localized inhibitory centre in the brain.³¹ It is not really clear what specifically led Sechenov to this work, though there were clearly good reasons in current experimental programmes to clarify an approach to central inhibition. But in addition, both Sechenov himself (in incomplete autobiographical comments) and Soviet historians have drawn attention to this early interest in psychology as a secular, 'westernizing' and anti-conservative intellectual force.³² From this point of view, Sechenov's concern with inhibition was part of a programme to establish physiological analogues for mental processes and thereby lay the basis for the 'science' of human nature - an intensely political commitment in the period of 'liberalization' following the accession of Tsar

Alexander II. Specifically, he conceptualized inhibition as the analogue for volitional control. According to M.G. Yaroshevskii, Sechenov recognized the need for a mechanism to explain 'the ability to resist external influences and put up barriers against unwanted impulses. But precisely this important feature could not be explained by the dominant ideas on the function of the brain.'³³ Sechenov had indeed introduced his retrospective description of his experiments by observing: 'The question of how the will is capable not only of evoking but also of repressing movement has been noted probably from the time when people began to notice in themselves and their neighbours the ability to depress "involuntarily" impulses to movements ... and to withstand in general any temptation for various actions.'³⁴

More significant than the origins of Sechenov's experiment however was what he did with it in his famous programme for a physiological psychology, *Reflexes of the brain* (published in articles in 1863 and as a book in 1866). There may be problems in interpreting this publication: it was published in Russian (which I do not read) and translated into English in Moscow at a time when Sechenov had achieved canonical status as both 'the father of Russian physiology' and the pioneer of a 'truly scientific' psychology (subsequently developed by Pavlov) - i.e. a psychology which exemplified the principles of Marxism-Leninism.³⁵ The text therefore needs discussion by a scholar of the Russian language. In the meantime it is nevertheless clear that in English translation, and for Soviet commentators, *Reflexes of the brain* projects a brilliantly innovative programme for a physiological psychology which is not crudely reductionist (not 'vulgarly materialist') but treats neuropsychic processes as adaptive and regulatory, mediating the dialectic between organism and environment. Conceptualization of relations between excitation and inhibition was fundamental to the argument: it regulated involuntary activity, it permitted the exercise of voluntary control through the brain, and it suggested that thought was a reflex with the final term (or motor effect) inhibited.

I have seen no evidence that *Reflexes of the brain* was known outside Russia in the 1860's. But Sechenov's experimental report (in German) of an inhibitory centre provoked a flurry of papers on whether control was localized in the way suggested, or whether control was rather a property of the mutual interaction of impulses and reflexes at various levels in complex ways. Another Russian, A. Herzen, writing under the influence of his mentor J.M. Schiff (who had constantly opposed the idea of nervous inhibition with a theory of nervous fatigue), argued that Sechenov's results should be interpreted in terms of the temporal and spatial pattern of excitation at different levels in the spinal system. The complexity of the issues quickly became apparent.³⁶ Doing scant justice to a mass of highly technical studies, one can say that in the long run the concept of functional localization proved less helpful than that of 'integration', especially when C.S. Sherrington systematized the latter concept in a lifetime of meticulous theoretical and experimental study.³⁷ Sherrington's treatment of control in terms of the 'integrative action' of neurones, mediated at synaptic junctions, laid the basis for twentieth-century western research on inhibition as a general chemical property of synaptic transmission. Nevertheless, the representation of control as 'integrative action' was not separate from representations of control in terms of a hierarchy of levels within the central nervous system. The latter continued to gain prominence whenever psycho-physiologists considered the regulation of the whole organism in interaction with the environment. It appeared to be a *sine qua non* of physiological *psychology*, though the practice of much neurophysiology became institutionally separated from this goal.

There were continued attempts to demonstrate experimentally relations between higher brain and lower spinal levels. Bubnoff and Heidenhain at Breslau in 1881 produced the first evidence that the cerebrum had a direct inhibitory function. Though this result depended on a skilled technique of localized electrical stimulation of the cortex, the physiologists significantly framed their discus-

sion with observations on hypnosis.³⁸ This is yet another indication of the broad - and both technical and lay - psychological context which structured discussion of inhibition throughout the century. Heidenhain in particular deployed the idea of decreased cortical inhibition, producing increased reflex irritability, to explain the phenomena in the hypnotized state.³⁹ 'Inhibition' also continued to feature in studies of decerebrated animals, the brain's regulation of tonic contraction and reciprocal muscular action, and in the voluntary arrest of reflex movements.⁴⁰ Thus 'inhibition' became refined into detailed neurophysiological questions, except when attempts were made to sustain a genuinely psychological context. In such cases, 'inhibition' had value for the way it integrated logical, empirical and social content, an integration stemming from conceptualization of human beings as loci of control and regulation.

Finally, I offer support for this conclusion by reference to two major twentieth-century schools in psychology which, however contrasted in other ways, both maintained and indeed elaborated a conception of hierarchical inhibitory control: Freudian psychoanalysis and the Pavlovian theory of 'higher nervous activity'.⁴¹ It is perhaps polemical to link these programmes, but in both inhibition was basic to the conceptual structure. This was so because both programmes addressed the development of individually regulated conduct in terms deriving from the nineteenth-century historical context which I have been discussing. Freud took over the notion of a structurally embedded evolutionary hierarchy and provided an attempt to understand culture as the 'higher' inhibition of instinct. Pavlov provided a physiological psychology of learning and behaviour as biological regulation, dependent on the spatial distribution of excitation and inhibition in the brain, in the manner which Sechenov had pioneered. In the English language and in Anglo-American culture, a psychological and quasi-Freudian sense of 'inhibition' has become the accepted everyday meaning. In describing people, emotions or actions as 'inhibited', the language reflects

the continuing social context which, since the nineteenth century, has provided the psycho-physiology of control with its conditions of intelligibility.

Notes

1. I follow convention in translating 'hemmen' as 'to inhibit'; but there are other words with which inhibition may be cognate (e.g. restraint, repression), signifying the same relation of temporary prevention of action.
2. Fearing (1964, p. 208); Dodge (1926a; 1926b).
3. Diamond, Balvin and Diamond (1963, p. 4).
4. Brunton (1883, p. 419).
5. E.g. Eckhard (1881, pp. 78-120); Meltzer 1899.
6. Hoff (1940, p. 478; emphasis added).
7. Cf. *Oxford English Dictionary* and J. and W. Grimm, *Deutsches Wörterbuch*. In French, Littré (first published 1863-72) assigns 'inhibition' only a jurisprudential meaning: *Dictionnaire de la langue française*. P. Robert, *Dictionnaire alphabétique et analogique de la langue française*, attributes a physiological meaning to Brown-Séquard about 1870. But I have not yet adequately explored the French literature.
8. Lister (1858); Pflüger (1857).
9. R. Cooter, 'The power of the body: the early nineteenth century' in Barnes and Shapin (1979, pp. 73-92).
10. Hall (1837). Cf. Eckhard (1881, pp. 53-78); Fearing (1964, pp. 128-58); Leys (1980).
11. Barlow (1839, p. 572).
12. Jacyna (1982); K. Danziger, 'Mid-nineteenth-century British psycho-physiology: a neglected chapter in the history of psychology' in Woodward and Ash (1982, pp. 119-46).
13. Carpenter (1876); Bucknill and Tuke (1858); Maudsley (1874). Cf. Smith (1981), and 'The boundary between insanity and criminal responsibility in nineteenth-century England' in Scull (1981, pp. 363-84); Clark (1982, pp. 115-22). For the wider evolutionary context: R.M. Young, 'The historiographic and ideological contexts of the nineteenth-century debate on man's place in nature' in Teich and Young (1973, pp. 344-438).
14. Mercier (1888, p. 370).
15. E.g. Jackson, 'Remarks on evolution and dissolution of the nervous system' (1958, II, pp. 92-118; first published 1887). Cf. Young (1970, pp. 197-223); Temkin (1971, pp. pp. 326-54).
16. For a synthesis of this argument: Sulloway (1979).
17. Durant (1984).
18. E.g. in discussion comments following Mercier (1888, p. 392).
19. The logical structure of this tradition and its continuation in Wundt and Brentano is analysed in Rappard (1979).
20. Herbart, *Psychologie als Wissenschaft* (1890-92, V, esp. pp. 273-80; first published 1824-25); *Lehrbuch zur Psychologie* (1890-92, IV, pp. 369-83; first published 1815; translated in 1891, pp. 9-32). Cf. Dunkel (1970); Leary (1980).

21. Herbart, *Psychologie als Wissenschaft* (1890-92, VI, pp. 14-36).
22. Wundt (1902, I, pp. 323-24; translated in 1910, pp. 317-18). I am not clear about the development of this argument through the earlier editions of the *Gründzüge*.
23. Murray (1976).
24. Herbart, *Lehrbuch der Psychologie* (1890-92, IV, pp. 355-62; translated in 1891, pp. 108-18).
25. Griesinger (1843). As Otto Marx points out (1972), Griesinger's substantial historical role was directing the study of insanity into the academic setting.
26. For the background: Canguilhem (1955); Eckhard (1881); Fearing (1964).
27. Pflüger 1857. Cf. E.H. Starling, 'The nervous and muscular mechanisms of the digestive tract' in Schäfer (1898-1900, II, pp. 313-37).
28. Goltz (1863).
29. Cf. Hoff (1940); Geison (1978); Schäfer (1898-1900, II); Eckhard (1881); Bernard (1858, II, pp. 146-71, 470-78).
30. Weber (1846, p. 47; my translation).
31. Sechenov (1863).
32. Sechenov (1965); M.G. Yaroshevskii, 'I. M. Sechenov - the founder of objective psychology' in Wolman (1968, pp. 77-110); Todes (1981, pp. 239-92). I have not seen the standard Russian biography by Yaroshevskii.
33. M.G. Yaroshevskii, 'The logic of scientific development and the scientific school: the example of Ivan Mikhailovich Sechenov' in Woodward and Ash (1982, pp. 231-54, on p. 242).
34. Sechenov (1965, p. 107).
35. Twice published in English, in slightly different versions: (1935, pp. 263-336; 1960, pp. 31-139). (It also appeared in French in 1884).
36. It was shown, for example, that chemical and electrical excitation had different effects, and that touch and pain reflexes possessed different mechanisms. Cf. Herzen (1864); Goltz (1869); Eckhard (1881, pp. 99-120); Yaroshevskii, 'The logic of scientific development' in Woodward and Ash (1982, pp. 231-54).
37. Sherrington (1961; first published 1906), and 'The spinal cord' in Schäfer (1898-1900, II, pp. 833-46, 884-919); Swazey (1969).
38. Bubnoff and Heidenhain (1881). In the 1940s, when there was renewed interest in cortical inhibition, the American translators of this article assumed that hypnosis was irrelevant and missed out these allusions - presuming their discipline was only experimental: Bubnoff and Heidenhain (1944). The theory of cerebral inhibition was important already in the 1870s: Ferrier (1876, pp. 280-89).
39. Heidenhain 1880. For the context: Ellenberger (1970, pp. 749-84).
40. E.g. E.A. Schäfer, 'The cerebral cortex' in Schäfer (1898-1900, II, pp. 711-13). Inhibition was an important feature of

the neurological models of psychological function characteristic of the 1880s and 1890s, especially of Meynert, Wernicke and Exner.

41. See for example: Freud (1959, esp. pp. 87-90; first published 1926); Pavlov (1960, esp. pp. 43-130, 395-411; first published 1927).

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VARIABILITY OF CULTURE AND HUMAN
SELF-CONSCIOUSNESS

Jerzy Bobryk

Department of Psychology
Warsaw University

Summary

The author proposes "subjective self" as a name for the human ability of self-consciousness and self-regulation, and "objective self" as a name for the result of the self-cognition process.

The paper presents the thesis that the subjective self as a state of consciousness in which an individual is the object for himself is the result of human social interactions and language use. In the author's opinion the objective self as content of the self-concept depends not only on an individual's experience but also on his cultural environment and the social conception of human beings.

The last part of the paper contains examples of historical variability of the social concept of human beings, and an attempt to present the interdependence between the individual's cognition and the development of social cognitive categories.

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Self-consciousness as a philosophical problem

One can use the word 'I' as a subject - "I see the high walls of the jungle towering over the water", or as an object - "I am the captain of a river steamer". It seems that the subjective self coexists with every content of the human introspective consciousness but it is contentless in itself. Philosophical conceptions of the subjective self comprise a collection of various solutions:

According to David Hume (1960) all human experiences alternate, it is impossible to find any constant element of consciousness. The idea of the self, however, has to be unalterable in order to guarantee the human sense of constancy and identity. Consequently, the idea of the self does not exist, the self is only the continuation of sensations, and it results from associations of ideas.

Immanuel Kant's solution of the question of the self is a continuation of Hume's ideas. According to Kant (1900) the idea of the self does not result from introspection. Self cannot be an object of human thought, it is contentless, although it coexists with human thinking. Self is only an 'a priori' form of human consciousness. All human experiences are ordered according to the subject-object opposition, it is indispensable and does not require any learning.

Gilbert Ryle (1949) presented a radical and clear solution of the self problem. According to him 'I' is not an idea or a concept, it is only an empty index-word similar to 'here' and 'now'. The word 'now' indicates the point in time, the word 'here' indicates the point in space, the word 'I' indicates the person.

All the above-mentioned philosophical conceptions of the subjective self emphasize the elusiveness and inapprehensibility of 'I'. At the same time one can inquire: Why is the subject-object opposition the main category of human mind? The clearest, though metaphysical solution to this problem has been presented by Kant: The self is an 'a priori' structure of human mind.

It is well to mention here the conception of the French linguist Emile Benveniste (1971). According to him 'I' exists only as an opposition to 'you'. 'I' appears only in the real act of speaking. In human communication the opposition of 'I' and 'you' is habitual and the self is comprehensible as an opposition to another. This means that 'you' (another person) as the clearer category constitutes subjective self.

Social genesis of the human mind

The human being lives in a social environment and uses signs to communicate with other human beings. Self-consciousness just as logical thinking or self-control is occasionally called a higher psychological function. According to Vygotsky (1966) social influences change the course of mental processes and lead to new abilities and behavioural acts, or in other words, to higher mental functions. The general law of the development of higher mental

functions has been formulated as follows:

Any higher function in the child's cultural development appears twice, on two planes, first on the social plane and then on the psychological plane, first among people as an intermental category and then within the child as an intramental category. The simple example of the development of the ability to point illustrates the essence of this law:

The first level of this development is the child's unsuccessful handgrip. In the case of a child trying to get something which is out of his reach his mother helps him and gives the object to the child. At this level the child's movement has a meaning only for his mother, the child does not intend to communicate anything. The repetition of the above situation teaches the child the new reaction - unsuccessful handgrip becomes a pointing-gesture. Training is the means of the learning process and the trained gesture is not conscious at first. We can teach an animal the same thing in the same way. The consciousness of acting appears later, this is the last level of the development of the pointing-gesture.

The instrument which serves society in forming an individual's mentality consists of signs. In the course of human development behaviours become the mediated acts with signs as mediating elements between stimuli and reactions:

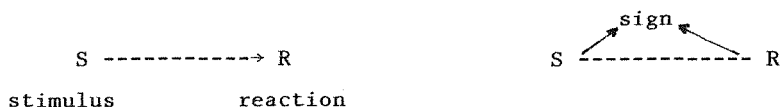


Figure 1

In the early developmental stages the mediating elements are external signs produced by the closest social environment of an individual; later they may be replaced by external signs which are produced by the individual himself, to become finally the signs which are internalized in the individual's mentality.

We can study this developmental process thoroughly by investigating the growth of a child's ability to control his own behaviour. At

first a child's behaviour is determined by the stimulus of the external or internal environment. Next appears a new additional specific stimulus - a sign produced by the social environment which is in most cases the verbal instruction of an adult:



Figure 2

The child is taught to obey verbal signs, just as an animal is, and the training is the means of the learning process.

The child's behaviour is a combination of his own activity and the modification of that activity by the much more sophisticated adult partner (Shotter and Gregory 1976).

The second stage of growth in a child's capacity of self-control and self-regulation begins when the individual takes an attitude toward himself similar to that which other people have taken toward him. This means that the child instructs himself with egocentric speech. The child's instructions are an imitation of an adult's instructions:

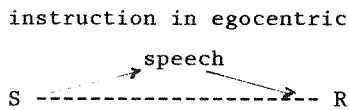


Figure 3

The last stage begins when egocentric speech grows into inner speech and the individual's thoughts control his behaviour:

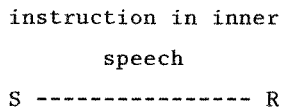


Figure 4

At this stage the individual becomes a self-conscious agent, the subject and the object at the same time. Due to the influences of society and the relevant signs the individual may become a person, or a self, a bifurcated thing described by G.H. Mead (1934), that is both agent and patient in action and subject and object in thought.

Inner speech and mediated consciousness of an individual

In the course of the development of individual thinking the role of verbal thinking increases and finally a great deal of thinking is performed in verbal form. The vehicle for individual thought is internal speech. According to Vygotsky (1978), the first developmental stage of individual thinking is external speech which is the imitation of the speech behaviour of an adult. This is rather speaking than thinking. The next stage is egocentric speech which is external too, but is produced by the individual himself. This is communication with oneself and "external thinking". The last stage of the development is internal speech which has strictly psychological characteristics.

This means that the social instrument of cognition, that is, ethnical language, is the instrument of thinking for individual subjects. This does not mean that there are no differences between external and internal speech or that verbal thinking is the only form of thought. There are different forms of thinking (sensori-motor thinking, imagination), but because of social influences verbal thinking is the most developed and most frequent one. The relations between language and thinking are shown in the following aphorism of Vygotsky: "Thought is not merely expressed in words; it comes into existence through them" (Vygotsky 1978: 86). This means that the subject can not become aware of his own thought if he does not find the vehicle for it. Usually the vehicle of individual thinking are signs of natural language (inner speech). Every fully developed and conscious thought is mediated by its vehicle. The mediating elements are signs from social semiotic systems; human consciousness is a result of this mediation.

Subjective self: the basis of self consciousness

According to many authors symbolic consciousness is the basis of self-consciousness. If one is thinking, or rather speaking to oneself in inner speech, during the perception of trees: "I see the high walls of the jungle ...", it means that one is a self-conscious subject.

According to Mead (1934) a person is a subject when he is the object for himself. In every social interaction one person is the object for another (the object of perception, the partner of communication, the object of evaluation) and another is the object for him. It seems that an individual becomes a subject when he has found the common element of these reciprocal relations:

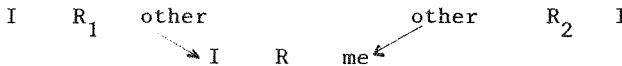


Figure 5

It seems that the special state of human consciousness in which the opposition of a subject and an object accompanies all human thinking is not a result of innate ideas but is rather a result of human social interactions and the use of language. The use of language as internalization of external speech requires two persons or one which is able to become two persons.

The process of self-cognition

According to M. Bakhtin (Bakhtin 1975) there are two different kinds of cognition. The first one is typical of scientific and practical thinking. This kind of cognition is directed toward the achievement of closed sense. For instance, the univocal definition of a concept, which is the aim of the mathematical sciences; or perhaps the prototype of an invention, which is the goal of technical science.

The second kind of cognition is directed toward the expansion of sense and is especially attributable to art and humanistic thinking, to the acquisition of self-knowledge, and to the compre-

hension of another subject. This kind of cognition always takes a discursive (dialogical) form (Todorov 1981). The dialogue is carried out by two active subjects with the use of external speech, or by the ego and alter ego with the use of thinking. This dialogue is a confrontation between two utterances, between asking and answering, between the assertion and the negation. Ordinary conversation suggests that dialogue is not only a good mode of expression, but it is the best way of thinking as well. For instance, when someone attempts to convey a new or complicated idea to us, if we want to be quite sure we have understood him completely, we try to express his idea in other words, our own words, or we try to find a well known context as a reference for that idea. According to Bakhtin, the essence of dialogical thinking is a confrontation of two expressions in which one expression is the context for another. This confrontation leads to a new expression which is the new context and which creates new sense. It seems that the dialogue is a way of developing ideas. The name 'discursive thinking' shows the essence of human thinking. According to Vygotsky and Piaget critical thinking appears in children as the result of internal modeling of the disputes in external speech which the child has held with others. Critical thinking is the imitation of these external disputes (disputes of adults, disputes with adults, and disputes with other children).

According to Bakhtin the anthropomorphisation of inanimate objects or natural forces which can be observed in art, and sometimes in our ordinary thinking, is a form of defense against the objectifying of man. Man's objectivity means in this case the closed sense which must be the termination of self-cognition. If one does not have an active partner one cannot develop his self-cognition. The cognition in exact sciences always has an impersonal character: since the distinctness of a subject and an object is not derived in the course of thinking, there is not an active subject in this case, but only a passive object.

On the other hand, self-cognition is achieved through the opposition of a subject and an object, or another subject. More precise-

ly, self-cognition is not a single identification, but the process by which relations between the ego and everything else are established. The difference between two subjects is the driving force of self-cognition.

A slightly different point of view has been presented by Bibler (1975). According to him creative thinking is first of all dialogical. Creative thinking is based on the process of concept expansion, which takes place due to the dialogue between the concept and its opposition. The opposition of concepts expose the latent features of these concepts. The concept is not the unit of thinking but its totality, it is impossible to be completely aware of one's own concept, because its meaning is always open. The meaning of a concept is the other concept or a set of concepts, the meaning of the concept which is the reference of the first mentioned concept is the next concept or concepts, etc. Bibler argues that the basis of his ideas is the Gödel theorem. It is impossible to demonstrate that a mathematical system is not contradictory if we use only the means of this system. Fundamental concepts are not defined, and axioms are not evidenced. It means that any concept of a formal system can reveal new unexpected sense. The system of knowledge stored in human memory is less closed (compact) than a formal system. This means that the process of self-awareness of the concept's meaning is, so to speak, the process of creating it. The process of concept creation or concept expansion takes a dialogical form, and it becomes the dialogue between the ego and alter ego.

There are three meanings of the social character of human thinking: The first refers to the genesis of the individual's thinking. A child assimilates social cognitive categories in the course of his growth and education. Vygotsky argues that the conceptual system acquired in school is the basis and the sample for the development of the remaining concepts.

The second meaning of the social character of human thinking concerns its form. A great deal of thinking is performed in verbal form. The vehicle for individual thought is inner speech. The

content and the form of human thinking are strongly connected. The ethnic language is not one single system which determines the individual mind. The whole of culture, ethnic language and other semiotic systems (religion, art, education, customs, etc.) force upon the individual categories of thinking and values. There are social modeling systems which model the individual mind.

The third meaning of the social character of individual thinking refers to its structure. Probably, human thought is often an internal dialogue which an individual holds with himself. But because of the genesis of human thinking discourse is probably a more convenient form of creative and critical thinking. Some psychological observations confirm this thesis.

Universal and culturally conditioned aspects of the individual's self

The individual's self has universal and culturally relative aspects. The universal aspects of the individual self emerge from the properties of a person as a human being. A human being lives among other human beings, interacts with them and uses language as a means of communication and cognition. Ongoing interactions change during an individual's life, they depend on the kind of culture and social structure, yet any one kind of social interaction may continue during the whole of the individual's life. Both facts (language and interaction) create the special state of an individual's consciousness, which has been called in this paper the subjective self. The subjective self as a basis of the human ability of self-control and self-consciousness is the universal aspect of an individual's self.

Another aspect of human self, which is the content of the self-concept or self-image, or in other words the result of self-cognition, has a relative character.

Figure 6 shows the relations between subjective self and objective self:

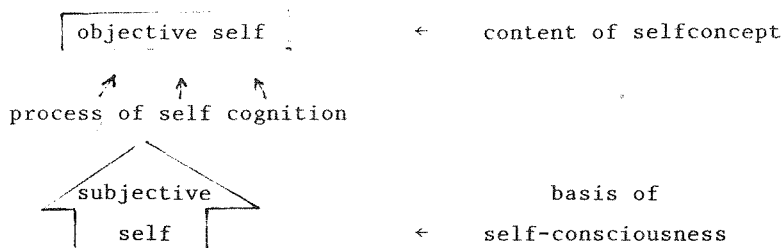


Figure 6

The individual's objective self depends on his particular way of life, but it also depends on the individual cultural environment and the social image of man being created by cultural semiotic systems.

Semiotic systems and cultural model of reality

As has been mentioned above, the instruments which serve society in forming an individual's mentality consist of signs. The sign is always an element of semiotic systems. This means that the whole semiotic system rather than a single sign influences the individual's mind. The signs of semiotic systems are names and symbolized events, objects and their features and by this naming things may be grouped and classified. From interactions with other beings and from the acquirement of signs one learns how to classify his physical and social environment. The social structure and the organization of signs within the semiotic system, and the mutual relations of semiotic systems enforce upon the individual the criteria of classification of his environment and consequently, determine the structure of his cognitive processes. Man lives in a symbolic environment, he is responding directly to symbols and his relationship to the external world is mediated through symbols. Human reflection of the external world is a form of reconstruction by means of symbols of semiotic systems (Lucid 1977, Lotman & Uspenskij 1971). The Tartu School (e.c. Lotman & Uspenskij 1971) calls semiotic systems modeling systems. Ethnic language is the most important modeling system, which describes the whole human environ-

ment and its vocabulary refers to all aspects of human experience. Language is a primal modeling system. Other modeling systems, like religion, art, artificial languages, customs, and ceremonies describe more specific aspects of the human environment. For instance, mathematics describes quantitative relations in reality, the symbols of religions express the moral and universal questions and solutions of mankind. These semiotic systems are called 'secondary modeling systems'. If we follow this line of thought we can decide that a culture at any time creates its model of reality, or in other words, the social picture of the world. The social modeling systems are the objectivizations of the cultural picture of reality. Concepts of time, space, causality, and man are important elements of the social image of the world.

Social images of man

Man not only learns to make and to be influenced by statements about his environment, he also learns to formulate verbal propositions about himself similar to those which other people have formulated about him. The social environment decides which properties of an individual are important and valuable and which are worthless, and it suggests the cognitive categories in which he should describe himself and the values to which he should ascribe.

Several examples can illustrate the changeability of the social image of the man:

It seems that in primitive societies the position of the individual almost exhausts the sense of the individual self. In this type of culture an individual's name often depends on his age or on the season of the year, because the social role of the individual may change in different periods of his life and in different seasons. Some tribes change the individual's name when his social position has changed. Sometimes the individual's name is replaced by the term of his relationship (Mauss 1966, Levi-Strauss 1966) and in many primitive cultures and societies one individual can be easily replaced by another one if he can perform the social role of the former (The killer of an Indian chief can become the new chief of the tribe).

The child in a Medieval painting looks like an adult (the same proportions of the face and the body). Also, education in this period did not depend on the age of the subject; children and adults were taught together (Gurevic 1972). The concepts of development and developmental changes were very weak in this period, because Medieval man believed that the entire reality had a hierarchical and fundamentally static character. According to Medieval mind God's act of creation made the whole universe a static and permanently complete Chain of Being (Lovejoy 1960) in which all elements have fixed roles. The essence of man is unalterable and depends on man's place in the universe; every individual, has his place in the hierarchical universe and his own role in the hierarchical social structure. According to Averroes, the intellect of the human soul is entirely passive and has a potential for knowledge, which is actualized by the active intellect (or agent intellect). The agent intellect exists outside the human soul, it is a superindividual divine substance (Leahey 1980). In the Middle Ages the human being was understood as an empty container which could be filled by external content. This content could be wisdom, humility, courage or other virtue, or in some cases the devil (which could be cast out by a priest). In these cases an individual is not an autonomic person, he is a carrier of universal virtues or sins. At the end of the XVIII century the Chain of Being was understood as a process in which all forms (all beings) are gradually realized in the order of time (Lovejoy 1960). This means that the conception of a complete universe was replaced by the conception of universal progress, and the philosophical conception of God who created the complete universe was transformed into that of a God who manifests himself through becoming and a gradual evolution of the universe. Since then man who, as a moral agent or as an artist, would imitate God, was called to add something of his own to the process of evolution. The notion of mind, which is the perfect mirror of reality, vanished after Kant's introduction to philosophy of the category of subjectivity. The literature of Romanticism celebrates the subjective, emotional and personal experiences of

the individual. The Romantic artist, who is a creator almost equal to God and who enriches the universe through his personal uniqueness, presents the new and continually present image of man. Since the 'Romantic revolution' any individual is a unique and creative being who attains value in and of himself.

The above examples suggest that the concept of man and his place in the world depends on time and culture. If we assume the social genesis of the individual's conceptual categories and the phenomenon of the symbolic mediation of human experience we can decide that the self-image of any particular person depends on the cultural image of man.

Individual and evolution in social cognition

One can wonder if individual autonomy of thinking is possible at all, and how evolution of cognition does take place. A culture understood as the totality of human semiotic systems is expressed in various kinds of signs. As a matter of fact, any culture comes into existence through the behaviour of individuals, because these signs are the result of human behaviour. On the other hand, any individual, from birth on, lives within a particular pattern of culture which programs a great deal of his behaviour.

Man is the only being which is aware of himself, which is an object for himself and subsequently, which is a subject. His cognition and self-cognition are processes which often take a dialogical form by which the relations between his particular ego and his external world are established. The cultural picture of the world is the usual context for individual utterances (verbal expressions, novels, philosophical systems, etc.). The social image of man is the starting point and the core of a process of individual self-cognition. Consequently, an individual who is a product of a particular culture is the only source of cultural development. It is only because of individuals that one epoch can hold discourse with another and the dialogue between two epochs (or two patterns of culture) creates ideas for the next one.

The theory of the social genesis of individual cognitive categories does not deny man's creativity. Furthermore, the analysis of the cultural image of man can reveal, for instance, why individuals reared in a cultural tradition which has not elaborated the concept of individuality have difficulty in comprehending the difference between egoism and the will to preserve human dignity.

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CRITICAL PSYCHOLOGY

A semi-participant observation

Gien Tuender-de Haan,

University of Amsterdam.

Summary

Critical psychology has pointed out some theoretical problems of contemporary psychology, which are gradually being perceived by other theoretically interested psychologists. Nevertheless direct discussion of the critical-psychological point of view has been relatively scarce. Some reasons for this lack of communication are explored in this paper.

Three phases are distinguished in the development of critical psychology so far:

1. Criticism of bourgeois psychology.
2. Redesigning psychology by means of the functional-historical method.
3. Specifying the theory for use in research and application.

Reactions from outsiders have been particularly scarce during the second and third period.

This may be partly due to external circumstances: prejudices and obstacles that have arisen against its outspoken marxist stand. Partly, however, critical psychology itself seems to have remained aloof from discussions with outsiders.

Hopefully, the emerging emphasis on specification of key concepts will provide a common ground for communication with other theoretically inclined psychologists.

1. Introduction

Communication between critical psychology and its traditional counterpart has been a strangely one-sided business. Strangely so, because its critique of existing psychology is directed towards problems which are very near to the core of scientific psychology:

- a. the arbitrariness reigning with respect to theory construction - leading to the patchwork pattern of psychological knowledge we all have learned to live with somehow; a state of affairs which has already in 1966 been aptly described by M.D. Dunnette as "Fads, fashion and folderol in psychology".
- b. The lack of external relevance of psychological knowledge - anyone who has tried for instance to apply the psychology of learning to the teaching of psychology will know what this means.

These problems have of course not gone unnoticed outside critical psychology. Among social psychologists in particular criticisms in the same vein have been seriously discussed.

Perceived reasons and proposed remedies vary. Sometimes the reasoning comes very close to the critical - psychological stand, but critical psychology itself is seldom directly referred to. In an evaluation of the use of the experimental method in social psychology, for instance, Henri Taifel, complains about "the triviality of much of the current research" and interprets it as a result of trying to apply "general laws of individual motivation" to explain social behaviour. Interaction between the social context and the social conduct is neglected, thus forcing the social psychologist to explain the social behaviour of essentially pre-social individuals. (2)

More radically, Serge Moscovici in his treatise on social influence proposes a change of model for social psychology. The widely accepted functionalist model has favoured an interpretation of social influence in terms of conformity and stability. It should be replaced by a "genetic model" stressing the interdependence of individuals and the social system and allowing a view of social influence as possibly innovative.

It is interesting that Moscovici recognizes a general reluctance among (social) psychologists to approach problems at this level. "The reasons for this reluctance are well-known: it is feared, that

a speculative tendency will become too strong, that abstract cogitations will fail to produce any concrete research. This fear is not justified. In fact, social psychology is greatly in need of a breath of fresh, speculative air". (3)

It is an intriguing question, why critical psychology has remained relatively isolated, despite the growing recognition of the problem it states. In this paper I set out to explore some possible reasons for this phenomenon.

I shall try first to summarize the development of critical psychology so far. Then an overview of its reception by main-stream psychologists follows. Concluding remarks will present my own - provisional - evaluation.

2. *Criticism, design and practice*

To summarize the history of critical psychology, it seems useful to distinguish three phases in its development, characterized by different accents in theory and research.

The three strands are not sharply definable in time, and certainly one common characteristic should be noticed: viz. the continually strong emphasis which critical psychology puts on the anthropological assumption of an intrinsic relationship between individual and society. Not the individual, but the relations between individuals and society should be the object of psychology in this view.

2.1. During the initial stage, reflected mainly in Klaus Holzkamp's book *Kritische Psychologie* this emphasis emanates in a critique of bourgeois psychology, along lines comparable with the Critical Theory in philosophy and sociology. (4)

With much epistemological sophistication it is shown, how hidden presuppositions about the independent status of the individual lead to a dependent position of subjects in psychological research. Thus the usefulness of psychological research for control over human beings is enhanced, in stead of furthering their emancipation.

This strand of criticism continues after the first stage, for instance in Maschewsky's monograph on the experiment in psychology

(5) or in a forthcoming book by Maiers, where an epistemological comparison of traditional and critical psychology is announced to be found (6).

2.2. The second phase sets in which Holzkamp's *Sinnliche Erkenntnis* (1973) in which conceptions from the Sovietschool of cultural historic psychology (derived particularly from Leontjev) are combined with a reinterpretation of the "most advanced" bourgeois theories on perception and cognition. Here the first application of the historical method is to be found, which is to become the key-stone of critical-psychological theory. Ute Holzkamp-Osterkamp's *Motivationsforschung* (part I, 1975, part II, 1976) continues this ambitious project of *redesigning psychology as a historic-materialistic science*.

Recently, the still programmatic character of these works (and of others, contributing on specific topics (7) is elaborated in Holzkamp's new book: *Grundlegung der Psychologie* (8).

Here, some unclarities of the theory are dealt with. The functional-historical method, which was used in different ways in *Sinnliche Erkenntnis* and *Motivationsforschung*, is now described more clearly in its application to the phylogenetic origins of human subjectivity (9). And, very emphatically, the purpose of deriving categories for a psychological theory is stated: the laying out of foundations is not meant to replace contemporary research. The categories are to be used as instruments to analyse specific concepts and partial theories and point out the implications of the theoretical foundation in each instance. (10)

An example of such an analysis is given in the last part of the book, where from the theoretically founded possibility of intersubjective understanding is derived the necessity of cooperation between researcher and subjects as a methodological principle. (11)

2.3. At present a third phase can be discerned, where the concepts arrived at in stage two, are being put to test in research and in practical work.

The interplay of theory and practice has been, of course, strived for in the earlier phases as well. But the relation is a strained one, as appears repeatedly in discussions in the Marburg Congresses of 1977 and 1979. The theoretical categories are indeed used in actual research but not always very consistently. (12) Neither have the attempts at application come through any systematic way in the theoretical work. However, as the theoretical foundations become clearer, possibilities for application may open up.

As an example might serve Holzkamp's description of a project with parents of newborn babies (13) about which more publications are announced to follow. Some results of this research-project seem to be reflected already in the *Grundlegung*. (14)

A really thorough incorporation of critical-psychological concepts in the practice of therapy is to be found in Ole Dreier's report on family-therapy. (15)

This renewed interest in the interplay of theory and practice is manifest also in a recent discussion in the *Forum Kritische Psychologie* (1982), which resulted in a meeting of theoretists and practitioners in february 1983.

Moreover, the next Marburg Congress will have as its central theme the methodology of research in critical psychology, and the theory-practice interplay as one of its subthemes.

3. *Establishment and outsiders*

The impact of critical psychology on the psychological community has been strongest during its first, most polemic, phase. Particularly the Holzkamp - Albert debate on value-orientation in psychology in the *Zeitschrift für Sozial Psychologie* (1971) (16) stands out as an attempt to communicate widely divergent views. The debate ends, however, with Holzkamp's giving up hope of changing psychology by talking about it, and the decision to concentrate on the development of critical psychology itself.

During the second phase many books have been published, which seem to have elicited notably few direct reactions from colleagues

outside of the marxist sphere of interest. Neither do outsiders venture to contribute to the *Forum Kritische Psychologie* - with one exception: an interesting reaction to Holzkamp's arbitrariness-thesis by Burkhard Brocke and Heinz Holling in 1982. (17)

These authors agree with Holzkamp's claim, that criteria should be developed to judge the relevance of psychological theories. As it is, the lack of critical reflexion in psychology is based on its strong social system rather than on its potential for survival as a science! However, they do not follow Holzkamp's choice of the functional-historical method as the royal road to theory-construction. In their opinion the functional-historical procedure is as yet not sufficiently clear to be considered the sole possible alternative. It should rather be presented as an interesting metaphysical innovation. Their own stand is more revisionist: increasing the variety of research methods and -situations, combined with a more systematic testing of the implications of theories, would be the less spectacular but safer way out of arbitrariness. The necessary breakthrough in theory-construction could be derived from a systematic reconstruction of practical knowledge available in the psychological professions. (17)

Another exchange during this period is to be found in the *Zeitschrift für Sozialpsychologie* (1978), where Volker Gadenne discusses the arbitrariness-thesis.

Gadenne also questions the scientific value of the functional-historical method. Since the analysis of natural history is based on still doubtful ethological theories, it does not reduce arbitrariness but only displaces it. Neither is the societal-historical analysis any help - should not psychology always take social circumstances into account?

In Gadenne's view arbitrariness can be overcome by combining a pluralistic stand in theory-construction with a rigorous methodology. (18)

A very well informed survey of the implications of critical-psychological theory for therapeutic practice is formulated by Christian Niemeyer from a psychoanalytic point of view. (19)

Knowing fully well how strongly "freudomarxism" has been rejected by critical psychologists, he nevertheless proposes an integration of critical psychology and psychoanalysis. Insights from critical psychology on phylogenetic and societal history should be combined with psychoanalytic knowledge of individual development.

Contributions from critical psychology on individual ontogenesis are characterised as sparse and unnecessarily shunning psychoanalytic ideas. I have not been able to find reactions from critical psychologists to this proposal.

The lack of academic comment does not imply a general isolation during this period. From outside the academic world contributions were made to the second Marburg Congress in 1979, by representatives of the DGB and of the Italian democratic psychiatry.

In the Netherlands, reactions to critical psychology have followed a different pattern. During its first and second phase it has been virtually ignored by academic psychologists. Attempts to provide summaries and introductions for the Dutch readership have been made mainly by groups of students. The late professor Duijker e.g., in his widely used *Textbook of Psychology* depicts critical psychology as a "theory of salvation". Although part of his criticism may be relevant, the effect is spoiled by its political dogmatism in his view (20). Only recently a more serious review by Van IJzendoorn has appeared. (21)

The relative isolation of critical psychology may be explained partly by external circumstances. The experience of facism and stalinism has left academics with a strong fear of contaminating science with politics. In the Federal Republic of Germany in particular the period of revolutionary optimism from which critical psychology originates, was followed by a political polarisation culminating in restrictive measures like the *Berufsverbote*. Apart from this some aspects inherent in critical psychology itself have tended to discourage serious interest from outsiders: one perhaps superficial but nevertheless real impediment has been the uninviting, almost ritualistic use of language in most of its publications.

Once 'labour' has been defined, for instance, as a co-operative and social process it seems superfluous to repeat the adjectives 'co-operative' and 'societal' in every instance where 'labour' is mentioned.

More important are two other characteristics markedly coming to the fore during the stage of theory-construction:

- a. A certain aloofness towards the most recent topics of research and discussion in psychology as well as in western marxism. Neither the cognitive accent of psychology, nor the extensive debate on ideology in contemporary marxism seem to have affected critical psychology much. In *Sinnliche Erkenntnis* for example, Holzkamp discusses Gibson's work as being the "most advanced" in traditional psychology, and seems to neglect Neisser's more cognitively inspired research on perception.

In the same vein, there is some discussion between Ute Holzkamp-Osterkamp and the *Project Ideologietheorie*. (22).

Although Holzkamp-Osterkamp may be right in pointing out that the theorists of ideology are not very clear about the reasons for development of change of ideologies, they do present a very strong case for the persistency of cultural forms and traditions. Whereas resistance to change in critical psychology is usually interpreted as a fear of the involved risks, the theory of ideology shows how consent about existing society can be consolidated by traditions, life-styles and common sense notions which form valued ways of living and interpreting one's life.

- b. A neglect of the interplay of theory and practice, despite the outspoken adherence to its importance. This complaint is voiced most generally by students and professional psychologists who try to integrate critical psychology in their work. However, since critical psychology in its third phase of development is paying more attention to this problem, I hope it will be overcome.

4. A personal view

So far I have summarized the reactions of "psychologists in general" to the challenge of critical psychology. To conclude I would like to touch on some of my own experiences in this respect. During the last ten years my subject of research has been unemployment, in particular the possibilities for action for women who want to have paid jobs.

Here the central thesis of critical psychology, that individual subjectivity is a social phenomenon, had been very relevant. In the case of unemployment, the pressures to isolate individuals and consider them responsible for situations on which they have only very slight influence come most markedly to the fore.

Although traditional psychology may have useful ideas about the meaning of work in people's lives - it does not seem to notice this very general practice of blaming the victim. Maybe this is so because it makes it its business to view individuals as unique?

The fundamental category in critical psychology with respect to unemployment is of course Ute Holzkamp-Osterkamp's "productive motivation". The second Marburg Congress, which had labour and unemployment as its central themes, has shown that the implications of this concept are not yet completely clear. Assuming that labour in a capitalist society offers only restricted possibilities for fulfillment of this productive need, it is not easy to understand why unemployment should be worse than working in debasing circumstances. One important aspect of paid work is pointed out by Holzkamp-Osterkamp: economic independence. In my research with women it appears however, that the quality of the available work is for some of them a very strong prerequisite. I interpret this finding as an indication that Holzkamp-Osterkamp's category needs to be elaborated before it can be applied to concrete situations.

A more specific theory of productive motivation would have to include qualifications about the sexual division of labour, the quality of available labour, existing alternative occupations and sources of income, etc. in a certain historical period. However, this problem has recently been recognized in the *Grundlegung* (24).

A similar problem of lack of specificity seems to be inherent in the critical psychological adage that for individuals to influence society, they need to be part of an organisation. Especially on this topic I think that more recent theories of ideology would be relevant to understand why so many people are "naturally" distrustful of organisations - even of those who profess to defend their rights. The crucial issue for the present third phase of critical psychology could be to arrive at more specific concepts and questions for research. A possible way of arriving at such theories of the middle range might in my opinion be found in supplementing the historical method by biographical research. A few examples are already available:

- a group of women from different trade-unions has taken up the strategy of writing reports about everyday experiences and discussing these. Their aim is from an evaluation of their subjective recollections to arrive at conclusions about objective, structural conditions. (25)
- Ute Holzkamp-Osterkamp reports on a small study analysing the preserved biographies of three German fascists, in order to arrive at a clearer understanding of the appeal of fascism. (26) Here it is recognized that other sentiments than the fear of risk may play a part in people's accommodating to the powerful: a wish to be important or to belong to a "community", and rancour against society.

In taking a clearer stand on the prerequisites of contemporary research, the *Grundlegung* at the same time poses new problems. Appointing research-subjects to the role of co-researchers is in principle a very important shift of emphasis, which has been promoted and attempted for some time in feminist research as well. (27) However, this new orientation implies so many difficulties - e.g. how to organise subjects who are perhaps only vaguely interested - that it may in practice limit research to those who are already active in the same circles as the researchers. (28)

To sum up, it seems to me that critical psychology has during its short history done a very thorough job of *stating the problem*. Its

formulation of the problems which confront and divide scientific psychology may have had a wider influence than is suggested by the scarcity of direct reactions.

In the reactions that do exist, however, repeatedly a reproach of "political dogmatism" has been uttered. In my opinion this is not a sufficient reason to break off communications. It is clear that the rugged individualism of traditional psychology is most easily perceived from a marxist viewpoint, but this does not disqualify the criticism in itself.

The proposed solutions that critical psychology has to offer are as yet only partially visible. The theoretical foundations having been laid out, the further impact will now depend on its capacity to stimulate research on specific problems and reflect on practical implications. An important prerequisite for this development is in my opinion constructive criticism and support even from those psychologists who do not endorse its views completely.

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IN DEFENCE OF VYGOTSKIJ
AN ANALYSIS OF THE ARGUMENTS THAT LED TO THE
CONDEMNATION OF THE CULTURAL-HISTORICAL THEORY¹

R. van der Veer
University of Leiden

Summary

In this article we deal with three arguments leveled at Vygotskij's cultural-historical theory in the thirties. The first two, "empiricism" and "idealism" are only dealt with in passing. The third criticism, "eclecticism" is analysed in some detail. Our conclusion is that at least two of the three charges, "empiricism" and "eclecticism" were clearly unfounded. Whether the third argument, "idealism", was valid has to be a topic of future research.

1. Introduction

In 1936 Vygotskij's cultural-historical theory fell into disgrace. His well-known *Thought and Language* was suppressed and his work and that of his close collaborators couldn't be mentioned or quoted for some 25 years. The exact reasons for this unfortunate course of events are difficult to lay bare. We know that in scientific and political journals several arguments were leveled at Vygotskij's theory and in the following we will deal with three of them.

In Soviet psychology, and especially in the thirties and forties, a purely rational, scientific debate is hardly possible. Almost every scientific point of view had political or ideological overtones. This means that to present the arguments

¹ The author is indebted to professor Marinus H. van IJzendoorn (Leiden, professor Luciano Mecacci (Rome), and Hans Rappard (Amsterdam) for their valuable comments.

which were leveled at the cultural-historical theory as purely rational arguments in a purely rational debate is to distort historical reality. The problem is that it is so difficult to uncover the ideological and political backgrounds. In the following we will touch upon some of these backgrounds, but surely other circumstances, as yet unknown, have played a role. In this sense, the picture presented here is a rational distortion or reconstruction of the actual course of events.

2. "Empiricism" and "idealism"

The first two arguments will only be dealt with in passing. First of all, most historians of psychology mention the fact that Vygotskij and his co-workers made extensive use of foreign sources in constructing the cultural-historical theory. It was claimed by critics that Vygotskij believed it possible to dissociate the "facts" of (bourgeois) psychology from the theories used to explain these facts, which would imply the so-called fault of "empiricism" (McLeish, 1975, p. 122; Vos, 1976, p. 101). This is a difficult charge to meet. Let us first have a look at Vygotskij's position as a theorist. An essay written in 1927, entitled *The historical significance of the crisis in psychology. A methodological study* reveals his epistemological point of view. He explicitly condemns an empiricist approach and defends the position that scientific facts are penetrated by theory. This is so, because (1) we select certain facts from a theoretical point of view, and (2) we phrase these facts in words, which are prototheories in themselves (Vygotskij, 1982, p. 316). The latter point Vygotskij took from one of his linguistic teachers, Potebnja. These arguments for the "theory-ladenness" of facts meant for Vygotskij that an atheoretical registration of facts is impossible and that the registration-induction model of science is untenable for psychology as well as for the natural sciences. Is Vygotskij's position compatible with the dialectical-materialistic epistemology? Yes, it is, in fact it is based on the writings of Engels and Lenin. Vygotskij explicitly refers to Engels' *Dialectics of Nature*, in which Engels condemns

empiricism, stating that in science we do not only observe certain phenomena, but use our intellectual capacity (*Denktätigkeit*) as well (Engels, 1978, p. 506). The emphasis on theory is also in accordance with Lenin's notion of the importance of theory (see Boeselager, 1975, p. 29). We thus may conclude that Vygotskij, at least in theory, was not an empiricist and that his epistemological position was in accordance with the classical dialectical-materialistic epistemology.

Now that we know Vygotskij's theoretical position, we can turn to his work as a practical scientist. Vygotskij indeed made extensive use of foreign data in constructing his cultural-historical theory. But he selected these data from his (dialectical) point of view and rephrased them in the conceptual language of dialectical materialism. This is completely consistent with the epistemological point of view formulated above. Perhaps we can clarify this procedure by mentioning Vygotskij's distinction between real facts and scientific facts. The term "real fact" refers to the raw material of science, the reality. The term "scientific fact" refers to such a real facts phrased in words and selected from a certain point of view (Vygotskij, 1982, p. 313). In this terminology, Vygotskij used the real facts of other theoretical schools, but made them into scientific facts compatible with his cultural-historical theory. In fact, he explicitly stated that (marxist scientists have to conquer the facts of bourgeois psychology by rephrasing them and reinterpreting them in marxist concepts. Can Vygotskij, the practical scientist, be considered an empiricist then? This depends, of course, on what we mean by the word "empiricist". In our opinion, one should restrict the word "empiricist" to those who believe that in science we deal with raw data and with theories based on these raw data. In this sense Vygotskij was, as we have tried to show, not an empiricist.

Another charge leveled at Vygotskij was that his conception of the role of the sign in mental development was in contradiction with Lenin's theory of reflection. According to this theory thought

is a product of (highly developed) matter. Thought and matter can't be dissociated (see Payne, 1968, p. 26). Vygotskij's notion of signs as a source of the development of higher psychological functions was considered to be in contradiction with this theory (see Van IJzendoorn & Van der Veer, 1984, p. 47-51). The signs were transmitted from adult to child in social interaction (*obšenie*) and it wasn't clear how these signs referred to (reflected) matter. If culture is transmitted from adult to child through signs, the origin of the development of the mind is seen as the result of a subject-subject interaction, rather than as the result of a subject-object interaction (Rahmani, 1973, p. 45). In fact, in the first edition of *Language and Thought* the editor, Kolbanovskij, had already stated this criticism in his introduction. The sign in Vygotskij's theory, he wrote, is not connected with labor or practical activity. In addition, he didn't think the connection with Pavlov's theory of the higher nervous activity satisfactory (Kolbanovskij, 1934, p. vi-xxxv). We thus see that according to this criticism Vygotskij's cultural-historical theory missed a clear materialistic underpinning. This is another way of saying that the theory was "idealistic". Elsewhere we have shown how later Soviet researchers have tried to avoid this criticism by adjusting Vygotskij's theory using the concept of activity (*dejatel...nost*) (Van der Veer & Van IJzendoorn, 1984). We there defended Vygotskij's point of view by stating that he tried to develop a dialectical view of mental development. In such an approach both reducing higher psychological processes to lower ones or to physiology and denying any genetical relationship between these two types of processes is inadmissible. It is clear that Vygotskij tried to retain some principal distinction between lower and higher psychological functions. It can also be shown that his distinction has several shortcomings (see Van IJzendoorn & Van der Veer, 1984, p. 47-51). Does this mean that Vygotskij really was guilty of "idealism"? This question is difficult to answer. In our opinion, Vygotskij did see the important role of material, biological factors in child development (see the next paragraph). But his opti-

mistic point of view might be called "idealistic". On top of that Vygotskij can indeed, from a dialectical-materialistic point of view, be reproached for not having given a material foundation of the signs of language. Such a foundation has been given by his contemporary Bakhtin (1930).

3. *Eclecticism*

A third and perhaps the most serious error which lost Vygotskij the favor of the communist party was his so-called devotion to foreign fashions and, in particular, to the mental testing movement. To make use of the theories and data of all sorts of foreign schools and currents in psychology was called "eclecticism" and this was considered to be a serious fault. Why was the mental testing movement, which was part of a new approach called "pedology" considered harmful? In a recent Soviet textbook we can find some answers. The editor of the book, Smirnov, gives the following description of the "pedology" movement. According to him "pedology" was a current in bourgeois pedagogics which spread to the Soviet Union and which defended the reactionary and fatalistic conception that the fate of children is determined by biological factors (e.g. genes) plus a fixed, immutable environment. The "pedologists" didn't see the important formative role of education in child development. Using mental tests they measured the so-called IQ, a quantity that can never reveal the possibilities of the child. As a result of this procedure, says Smirnov, children were judged in a purely negative fashion and gathered in remedial schools according to negative characteristics. These schools were filled with children, who were not "backward" or "disabled" but just neglected. The authorities were extremely dissatisfied with this situation and in 1936 the party's resolution *On pedagogical distortions* resulted in the termination of all pedological activity (See Bejn, Levina & Morozova in Vygotskij, 1983, p. 354).

Because Vygotskij and his co-workers were identified with the pedology movement they also fell into disfavor. But did Vygotskij really underestimate the formative role of education in child deve-

lopment? Was Vygotskij really a proponent of mental tests used in the way Smirnov describes? These questions we will try to answer in the last part of this article. We will show that Vygostkij defended a much more sophisticated position, using his writings on defectology to do so. Most of these writings can be found in the recently published fifth volume of Vygotskij's collected works (Vygotskij, 1983).

3.1. Defectology: cultural tools

From the very beginning of his scientific career Vygotskij had worked with disabled children. He started to work with deaf and blind children in 1924 and continued to do so until his death. During the last years of his life he was head of the Experimental Defectological Institute in Moscow.

At first Vygostkij in his publications on disabled children was strongly influenced by various authors, notably Alfred Adler. Adler had written that organic inferiority through the subjective inferiority complex is transformed into the striving for compensation and overcompensation. Curiously enough, this paradoxal thesis seemed to Vygotskij to be compatible with a dialectical view of mental development. In these years he time and again returned to Adler's work for inspiration. It is only later, when the cultural-historical theory gets shape, that he develops a truly original view of the disabled child.

In his cultural-historical theory Vygotskij attached great importance to social interaction, through which the higher psychological processes develop. All the more serious, then, when a child is in danger of being cut off from this social interaction, as is the case with children with physical handicaps such as blindness or deafness. It is also well-known that to Vygotskij social interaction was chiefly verbal interaction. One might even say that he conceived higher psychological functions as verbal functions. Because it is so difficult for deaf children to learn to speak, it is in particular the mental development of the deaf child that Vygotskij believed endangered. However, he certainly did not regard

the situation of these children as hopeless. On the contrary, he believed these children capable of full social existence if they are provided with the means or tools to develop intellectually. The means particularly suited for achieving this goal, such as speech and writing, have been adapted to the average person in full possession of the faculties of hearing and sight. It is the task of psychologists, Vygotskij said, to uncover new, adapted means of enabling the disabled child to participate in society. If they fail, the child will remain primitive. In this regard Vygotskij made a rather daring comparison with primitive peoples lacking the means or tools which enable a culture to flourish, such as a written language (Vygotskij, 1983, p. 25). In both cases cultural development is limited for lack of tools. The notion of "primitivity" Vygotskij took from the French anthropologist Lévy-Bruhl (see Cole & Scribner, 1974, p. 20; Lévy-Bruhl, 1976, p. 31-42).

Vygotskij's point of view can be illustrated by the case of the blind child. He does not regard the loss of sight as fatal, for culture is still accessible through such cultural instruments as speech, reading and writing. These cultural instruments are not bound to one particular sense. The blind child does not read with its eyes but with its fingers. The deaf child does not speak with its mouth, but with its hands. In principle, Vygotskij believed, there is no difference. The culture is just as accessible to the disabled child, only through other means by using other cultural tools (Vygotskij, 1983, p. 171). Indeed, Vygotskij refused to regard the disabled child as inferior, but repeatedly emphasized that it is simply different. In 1931 he even wrote:

"We assume that even if human beings only had four senses, it would make no essential difference in the knowledge they could acquire, for fundamentally, thought - the means by which we assimilate experiential data - would remain the same. The picture we develop of the reality around us is not only based upon perceiving it directly, but also upon our rationally assimilating experience. In principle, both the blind and those possessing sight know much more than they imagine; they know much more than they can observe through the five senses

(...). And so for the blind child, thought is the most important means of compensating for this inadequacy in perceiving images" (Vygotskij, 1983, p. 211).

In this way Vygotskij linked his view of the disabled child to the epistemological notion that direct perception does not necessarily result in valid knowledge. This notion had been underlined in the work of Engels (see Kolakowski, Vol. I, p. 394, 1981).

3.2. Defectology: the need for a qualitative diagnosis

To Vygotskij's mind then, the deficiency of a lower function arising in phylogenesis, such as perception, can be compensated by utilizing a cultural instrument. In this regard a "disabled" child does not differ from a "normal" child at all. When, for example, the "natural" direct memory fails, the normal child will also use language (categorizing and labeling the objects to be remembered) and all kinds of mnemonic systems. The notion that psychological functions can be brought about in different ways, Vygotskij took from Binet (Vygotskij, 1931 in Vygotskij, 1983, p. 122). For each failure of an organic function psychologists must attempt to find a cultural instrument to replace it. In order to do so, a qualitative diagnosis of the disabled child is required. It is not only necessary to determine exactly which function is disturbed, but also which functions have remained unimpaired. Time and again Vygotskij opposed a purely quantitative diagnosis, determining only how far a child lags behind and what it is incapable of doing. The psychologist needs a qualitative analysis of the child's strengths and weaknesses. A mental test can perform a supportive role in this diagnosis, but no more than that (see also Lurija, 1979, p. 82). Measurement and diagnosis are two entirely different aspects of psychological research. "The tape measure sees nothing", Vygotskij wrote in 1931, "without subjective assimilation, that is, without thought, without interpretation, (without) decoding the results (without) evaluating the data, there is no scientific research" (in Vygotsky, 1983, p. 299).

We thus see that both questions raised above must be answered in the negative. Vygotskij neither underestimated the role of education in mental development nor was he a proponent of an unbridled use of mental tests. He explicitly condemned the latter approach because children are then characterized as "mentally retarded". But is that not like telling the patient that he is ill, he wrote, without specifying which actual disease he is suffering from? Only on the basis of a detailed, qualitative analysis recovery is possible.

In the nineteenthirties Vygotskij was accused of underestimating the formative role of the environment. Nowadays, in our opinion, it would seem that he expressed a rather optimistic point of view. After all, these children do suffer from an organic defect. Vygotskij did acknowledge that there are organic deficiencies, which in and of themselves cannot be remedied. But in his dialectical view of mental development a successful reorganization of the mind is possible. Vygotskij: "... the biological processes primarily responsible for the first stage in the development of the mentally retarded become concealed; they are not eliminated but neutralized (*snjatami*) during the process of development ..." (Vygotskij, 1931 in Vygotskij, 1983, p. 118). With this explicit reference to Hegel's dialectic Vygotskij once again expressed his optimistic view of the human being. Nature is overcome by nurture "The word overcomes the blindness" (Vygotskij, 1983, p. 95).

4. Conclusions

Three arguments aimed at Vygotskij's cultural-historical theory were presented. It was shown that two of them, "empiricism" and "eclecticism" were unfounded. Moreover, it was demonstrated that Vygotskij's ideas were in accordance with the general Marxist-Leninist framework. Whether the third argument, "idealism" was valid, is doubtful. It caused, however, much controversy in Soviet psychology and eventually resulted in the activity approach, of which Leont'ev was the principal spokesman (see Wertsch, 1981).

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CLARK HULL UND DIE PSYCHOANALYSE

Dr. Angela Schorr

Katholische Universität Eichstätt *

Summary

Clark Hull's approach to scientific theory building still represents one of the most ambitious endeavours of this kind in the history of psychology. While his later works are open only to a few experts, mostly contemporaries and scholars, earlier presentations of his theory (before 1940) already display the central aspects both of his learning theory and his view of scientific progress. His truly openminded attitude towards psychoanalysis and especially the development of his concept of drive by analogy with Freud's concept of libido, throw light on Hull's peculiar way of thinking as a creative scientist and a contemporary. Following this aspect of his work, our myth of the yet unattained scientific ideal of a 'supra-theory' on the basis of rigorous deduction may give way to a more substantial analysis of the pros and cons of his effort.

-o-o-o-o-o-

Clark Hulls zumeist hochformalisierte Gedankenwelt (Hull 1943, 1951, 1982) historisch aufzuarbeiten ist kein leichtes Unterfangen.¹⁾

Und das, obwohl er uns durch eine Hinterlassenschaft von insgesamt 73 "Notebooks" einmalige Einblicke in sein Denken gewährt hat (Hays 1962, Ammons & Ammons 1962).²⁾

Zentrales Element der Hullschen Lerntheorie ist die Verstärkungstheorie, wobei es sich bei der Identifikation des Verstärkungsprozesses mit dem Prinzip der Bedürfnisreduktion jedoch um eine reine Hypothese handelt, deren Gültigkeit sich experimentell nur indirekt erschliessen lässt (vgl. Hilgard 1948).

* John Dollard (†), E.R. Hilgard, Neal E. Miller, O. Hobart Mowrer, Robert R. Sears, sowie mein guter Freund Cedric A. Larson haben durch ihre Gesprächsbereitschaft und Offenheit viel zu dieser Untersuchung beigetragen.

In dem Versuch, dem Ursprung dieser - seinen eigenen Vorstellungen von wissenschaftlicher Theorienbildung im Grunde zuwiderlaufenden³⁾ - Konzeption auf die Spur zu kommen, ist Hulls Beschäftigung mit der psychoanalytischen Theorie bisher nicht berücksichtigt worden. Ja, man hat sogar - seine eigenen Intentionen fehldeutend - diesen Bereich und seinen Beitrag dazu (z.B. Hull 1939) systematisch ausgespart. Die folgenden Ausführungen stützen sich auf unveröffentlichte Passagen aus seinen "Notebooks" und Gespräche mit Zeitgenossen.⁴⁾

Zur Lage der Psychologie Ende der zwanziger, Anfang der dreissiger Jahre.

Mit Beginn der dreissiger Jahre wuchs das Bedürfnis der amerikanischen Psychologen nach Orientierung und fundierter wissenschaftlicher Forschung. Der Theorienpluralismus, dokumentiert in den Sammelbänden Murchison (1926, 1930), liess ernste Zweifel am Sinn experimentalpsychologischer Forschung aufkommen⁵⁾ und man begann, die Wirkungen von Sektierertum und Fanatismus zu fürchten (Murchison 1930, Hull 1935, 1952, Watson 1928). Zwei Theorien, die sich gut in das anfänglich so hoffnungsvolle Klima der zwanziger Jahre eingefügt hatten, waren der Behaviorismus und die Psychoanalyse. Beide fanden sich unter dem Schlagwort "the new psychology" wieder⁶⁾, beide stellten das Bewusstsein und seine Begleiterscheinungen in Frage. Und gegenüber beiden bewahrten sich die amerikanischen Psychologen auch über dieses Jahrzehnt hinaus eine ambivalente Haltung.⁷⁾ Bücher wie Wohlgemuts (1923) *A Critical Examination of Psychoanalysis* und Dunlaps *Mysticism, Freudianism and Scientific Psychology* (1920) waren angesichts der grossen Popularität der Psychoanalyse in den zwanziger Jahren Balsam für die geplagte Wissenschaftlerseele⁸⁾ (May, 1976).

Neben der allgemeinen Kritik zeigte sich jedoch auch so mancher prominente Empiriker begeistert von den neugewonnenen Fakten und Einsichtsmöglichkeiten (Holt 1915, Watson 1916, 1917, 1919, Humphrey 1920, 1921, Burnham 1924, Hollingworth 1926, Moss & Hunt 1931). Konzepte wie Rationalisierung, Projektion und Widerstand

waren einfach aus der persönlichen Erfahrung heraus überzeugend⁹⁾, und so gingen viele analytische Konzepte nach und nach in das professionelle Denken der Psychologen ein, während man die Psychoanalyse offiziell weiterhin bestenfalls als Pseudowissenschaft klassifizierte.¹⁰⁾ (Klein 1933, Heibreder 1940) Bereits 1924 bezeichneten es so namhafte Wissenschaftler wie L.L. Thurstone, James A. Leuba, K. S. Lashley und Joseph Jastrow als das Verdienst der Psychoanalyse, schwerwiegende Mängel in der psychologischen Theorienbildung, insbesondere im Bereich *Motivation* aufgedeckt zu haben. Den heuristischen Wert der Theorie Freuds für die psychologische Forschung schätzte man sehr hoch ein und machte erstmals auch Vorschläge zu ihrer Verwissenschaftlichung.¹¹⁾

Auch John Watsons Behaviorismus hatte die Psychologie seiner Zeit verändert. Nach seinem unfreiwilligen Ausscheiden aus der Forschung im Jahre 1920¹²⁾ hatte Watson damit begonnen, seine Ideen durch populäre Schriften einem grösseren Publikum zugänglich zu machen. Er schrieb für *Harpers Magazine*, *McCalls* und den *Cosmopolitan*. Sein Buch *The Psychological Care of the Infant and Child* (1928) wurde über Nacht zum Bestseller.¹³⁾ Doch mit all diesen Erfolgen wuchs auch die Kritik aus den eigenen Reihen. Kollegen wie James R. Angel kritisierten die Vermarktung des behavioristischen Standpunktes (Watson 1936), man monierte Watsons populären Stil (Cohen 1979) und das Prinzip des konditionierten Reflexes war bis zum Äussersten strapaziert worden.¹⁴⁾ Das akademische Establishment suchte weiterhin nach einem Mittelweg zwischen Introspektionismus und Behaviorismus, und selbst überzeugte Behavioristen betrachteten ihn als "a kind of fiction".¹⁵⁾ Der grossen Popularität des Behaviorismus in den USA stand gegen Ende der zwanziger Jahre ein Minimum an empirischer Forschung gegenüber. John Watson hatte in seiner kurzen Karriere nur wenige Studenten ausbilden können, die in der Lage und bereit waren, seine Forschung fortzuführen (Samelson 1980). Zwar gab fast jede Universität, wie er 1928 selbst feststellte, einen Kurs in seiner Theorie, doch mehr um der hitzigen Diskussionen willen, als aus ernsthaftem wissenschaftlichem Interesse (Watson 1928). Erst die Übersetzung der Arbeiten Pavlovs (1927, 1928) und die damit verbundene Wiederentdeckung einer den

Amerikanern lange verborgenen Welt der wissenschaftlichen Experimentation zum konditionierten Reflex war für junge, ambitionierte Experimentalisten der Ansporn, die Forschung im behavioristischen Paradigma auf einer neuen Ebene fortzuführen.

Clark Hull und das Institute of Human Relations

Clark Hull begann als einer der ersten gleich nach Erscheinen von Anreps Übersetzung der Pavlovschen Vorlesungen (1927) mit experimentellen Studien.¹⁶⁾ 1929 wechselte er nach Yale und zum neugegründeten *Institute of Human Relations*, überzeugt von seiner Mission, der *Neukonzeption der Psychologie als Naturwissenschaft*. Bis zu seinem 50. Geburtstag im Jahre 1934 wollte er sein "magnum opus" geschrieben haben, ein Buch über die "habit".¹⁷⁾ Zeit seines Lebens geplagt von einem kranken Körper konnte er zwar im Juni 1933 vermelden, dass er nun mit seinen Untersuchungen erstmals über die russischen Arbeiten hinausgehe und neue Phänomene entdeckt habe¹⁸⁾, doch beschloss er im darauffolgenden Jahr, die Ausarbeitung der *Theorie* den experimentellen Arbeiten voranzustellen.¹⁹⁾ Schon frühzeitig vertrat er aus forschungsökonomischen wie auch konzeptionellen Gründen die Auffassung, Fortschritte in einer Wissenschaft seien nicht nur durch Beobachtung und Experimentation, sondern darüber hinaus auch durch sogenannte "Gedankenexperimente" möglich (Hull 1930). In einer strategisch gut geplanten²⁰⁾ Abfolge von Aufsätzen führte er zwischen 1929 und 1934 in der Darlegung seiner Auffassungen eine Reihe neuer Konzepte ein, wie das der Triebstimuli und der antizipatorischen Zielreaktionen (1931), die Hypothese des Zielgradienten (1932) und das Konzept der "habit family hierarchy" (1934).

Die moderne Psychologie - so Hulls Diagnose - krankte nicht an ihrer Experimentation, sondern an der Theorienbildung (Hull 1935). Eine neue, wissenschaftlichere Theorienbildung soll auf dem Prinzip der Deduktion basieren, durch Deduktion der Hypothesen von bekannten Erklärungsprinzipien liess sich laut Hull ein wissenschaftliches System gezielt erweitern²¹⁾ (Hull 1930). Und diese Erklärungsprinzipien konnten auch - wie noch zu zeigen sein wird -

der *psychoanalytischen Theorie* entlehnt sein. Denn entscheidend war für Clark Hull nicht das Ansehen, dass eine Theorie in einer eng umschriebenen scientific community genoss. Schon frühzeitig hatte er erkannt, wie vage die Massstäbe zur Bewertung wissenschaftlicher Theorien formuliert waren und wie individuell man die Kriterien der "Konsistenz", "Klarheit" oder "(empirischen) Evidenz" gewichtete. Hull vertraute auf das Potential seiner deduktiven Methodologie, und nicht ohne Grund vermutete Mills (1978), dass es das "grand design" seiner Theorie war, das viele Zeitgenossen Halls für seine Theorie begeistert hat.²²⁾

Das *Institute of Human Relations*, als reine Forschungseinrichtung mit dem Ziel gegründet, durch ein konsequent interdisziplinäres Vorgehen die sozialwissenschaftliche (Grundlagen-) Forschung so zu intensivieren, dass für die praktische Tätigkeit (von Medizinern, Juristen, etc.) relevante Informationen rascher zur Verfügung gestellt werden konnten (Dollard 1964, May 1971), bildete für Halls Ziele einen sehr heterogenen und zugleich stimulierenden Hintergrund.²³⁾ Das Institut selbst stiess innerhalb der *Yale University* auf grosse Resonanz, und es fehlte zunächst nicht an Wissenschaftlern aus den verschiedensten Fachbereichen, die sich anschickten, den bereitgestellten Mittelfond auszuschöpfen. Doch blieb die Frage unbeantwortet, welchen spezifischen Inhalt die dort betriebene, interdisziplinäre Forschung eigentlich haben sollte. Vor dieser Frage stand man schliesslich 1935, als die finanziellen Ressourcen weitgehend verbraucht waren und man in einer Zeit tiefster wirtschaftlicher Depression nach einem überzeugenden Programm suchte, um die *Rockefeller Foundation* zu einer Erneuerung der Mittelzuweisung überreden zu können. Ausgehend von der Erkenntnis, dass ein "multiscience approach" erst sinnvoll sein kann, wenn ein Gebiet ausreichend aufbereitet ist, beschloss man zunächst nach Integrationsmöglichkeiten auf *theoretischer Ebene* zu suchen - ein kreativer Sparbeschluss!²⁴⁾

Halls Einstellung zur Psychoanalyse

Clark Halls Ziel, eine allgemeine Theorie des Verhaltens zu entwerfen, die auch die als "kognitiv" bezeichneten Verhaltensaspekte

einschloss²⁵⁾, liess ihn seine Forschungsthemen nicht nur im Bereich des Lernens suchen. Auch lenkte die unruhige Zeit - dem wirtschaftlichen Absturz war eine Phase tiefer emotionaler Verunsicherung in der Bevölkerung gefolgt - sein Forschungsinteresse. Anfang der dreissiger Jahre rangierten hinter der Lernforschung in seiner Planung bereits Fragen zum Thema Frustration, Fragen zur Psychopathologie, Freudschen Psychologie und zur Untersuchung des emotionalen und instinktiven Lebens.²⁶⁾ Schon frühzeitig überlegte er, ob sich nicht auch andere theoretische Standpunkte mit der mechanistischen Psychologie verbinden liessen.²⁷⁾

Hull suchte damals ein zentrales Problem zu lösen: Auf der Suche nach Möglichkeiten einer Harmonisierung der Konditionierungstheorie nach Pavlov mit den bei Thorndike beschriebenen Phänomenen war er bemüht, über das (experimentell nicht abgesicherte) Prinzip der Stimulussubstitution nach Hollingworth²⁸⁾ hinaus weitere Konzepte zu verwenden, die den motivationalen Aspekten des Verhaltens Rechnung trugen.²⁹⁾ Bis Mitte der dreissiger Jahre blieb er ein "incentive motivation" Theoretiker, d.h. er ging von der motivierenden Wirkung des Stimulus zur Reaktion aus. In einer deterministischen Theorie des Verhaltens sollte die Belohnung jedoch nicht etwas sein, das der Organismus zielgerichtet anstrebt. Noch in seiner - sehr differenzierten - Rezension von Thorndikes *Fundamentals of Learning* (Hull 1935) blieb Hull bei dieser Konzeption, erwog jedoch erstmals öffentlich, motivationale Aspekte des Verhaltens als Theorem von der Grundannahme der Konditionierung abzuleiten. Thorndikes "law of effect" betrachtete er sowohl als Lernen wie als motivationales Gesetz und fragte sich weiter:

"Erzeugt die Motivation (das Streben) Lernen (eine Stärkung der Verbindung) oder produziert das Lernen die Motivation oder bewirkt ein dritter, noch grundlegenderer Prozess beides? Thorndike scheint davon auszugehen, dass das Streben primär ist, obwohl er dies nicht ausdrücklich sagt. Ich hingegen neige eher zu der Auffassung, dass die 'Konditionierung', die Stärkung der Verbindung primär ist. Damit ist gemeint, dass das Streben möglicherweise als Theorem von den Prinzipien der Konditionierung als grundlegenden Annahmen ableitbar ist." (Hull 1935, S. 821; übers. v. Verf.).

Diesen Gedanken fortführend kam Hull damals zu dem Schluss: "Also wird der Organismus durch den einfachen Prozess der Konditionierung

nach Zuständen streben, die positiv verstärkend sind." Doch wie liess sich diese Überlegung konzeptionell fassen?

Clark Hull suchte Antwort in einer populären Motivations-
theorie, der Psychoanalyse. Dieser Gedanke kam nicht von ungefähr,
denn das *Institute of Human Relations* war damals an psychoana-
lysierten und analytisch vorgebildeten Experimentalpsychologen bzw.
Sozialwissenschaftlern reich.³⁰⁾ Mit Earl Zinn wurde 1935 der erste
professionelle Psychoanalytiker am Institut fest angestellt.³¹⁾
Hull schätzte Freud als konsequenten Deterministen und legte in
bezug auf die Validität psychoanalytischer Erkenntnisse eine - an
seinen eigenen Zielsetzungen orientierte - positiv - liberale
Haltung an den Tag. So sprach er der Psychoanalyse zwar den
Charakter eines formalen wissenschaftlichen Systems ab, erkannte in
ihr aber dennoch "a good deal of logical structure" zu. An seine
Studenten gewandt meinte er später:

"Was immer man von Freuds Auffassungen denken mag, niemand
kann leugnen, dass er sich mit einer Vielzahl extrem wichtiger
Probleme beschäftigt hat. Der Student sollte Freuds Auf-
fassungen weder als Evangelium betrachten, noch zurückweisen,
weil er sie geschmacklos empfindet - die Wissenschaft bewertet
Fragen auf der Basis von Beweisen, nicht aufgrund von 'Wunsch-
denken'."³²⁾

*Was lag also näher, als Hulls Forschungsproblem mit den Interessen
seiner Kollegen und Mitarbeiter und der prekären Situation des In-
stituts zu verbinden, d.h. nach Möglichkeit zur Integration der
(Hullschen) Lerntheorie mit der Psychoanalyse zu suchen?*

Zur Entstehung des Triebkonzepts (D) in der Hullschen Lerntheorie

Hierzu gibt es verschiedene Theorien.³³⁾ 1957 kritisierte Eysenck
Hulls Konzept eines allgemeinen, undifferenzierten Triebes, den er
durch das multiplikative Theorem als sich summierend auffasste,
treffsicher als vergleichbar einem allgemeinen Fundus libidinaler
Energie (Eysenck 1957, S. 104). Tatsächlich befasste sich Hull in
den Sitzungen seines informellen, nach langwierigen Verhandlungen
mit John Dollard zustande gekommenen und der Integration dienenden
Seminars "On Habit Dynamics and Motivation" zentral mit der Natur
der Libido.³⁴⁾ Er hatte sich mit Begeisterung in die Ausbeutung der

psychoanalytischen Theorie vertieft und suchte dabei Thorndikes motivationale Lerntheorie einzubringen. Denn natürlich ging es ihm - wie der Titel des Seminars bereits ahnen lässt - primär darum, im Rahmen des so anspruchsvoll angekündigten Integrationsversuches auch sein eigenes theoretisches Problem zu lösen.

Ausgangspunkt des Seminars bildete das Buch von Healy, Bronner & Bowers, *The Structure and Meaning of Psychoanalysis as Related to Personality and Behavior* (1930), eine leicht verständliche und zugleich vergleichend-kritische Einführung in Freuds Theorie. Dieses Buch, das für viele Psychologen der dreissiger Jahre den ersten Einstieg in die Psychoanalyse darstellte, kam in seiner Konzeption der suchenden Perspektive der Wissenschaftler in Yale sehr entgegen: Während auf den geradzahligen Seiten versucht wurde, Teilstücke der Freudschen Theorie möglichst werkgetreu darzustellen und zu erläutern, enthielten die Seiten mit ungeraden Seitenzahlen in engem Druck Kommentars, Erläuterungen, Deutungen und Modifikationen der Theorie durch bekannte Psychoanalytiker und Psychotherapeuten. Freuds Theorie vermittelte so den Eindruck des Unfertigen, des noch nicht Ausdiskutierten. Gerade das Konzept der Libido, das erste, mit dem sich die Autoren in ihrer Darstellung befassten (Healy, Bronner & Bowers 1930, S. 2 - 6), bot sich in seiner Vieldeutigkeit für Uminterpretationen an.

Hull betrachtete sie als eine Energie, die sich in einer erstaunlichen Vielfalt von Verhaltensformen manifestieren konnte. War es also denkbar, dass einfaches Versuch-und-Irrtum Lernen durch die Libido motiviert wurde?³⁵⁾ Generell konzipierte er die Libido bewusst im Gegensatz zur analytischen Auffassung als *undifferenzierte Motivationsorganisation*³⁶⁾, klärte im Gespräch mit Zinn und Homberger³⁷⁾ ab, dass sie verschieden ist von S_D , engte ihre Definition dann noch einmal auf die analytische Definition der Libido als kennzeichnend für die Stimulation erogener Zonen (einschliesslich aus bestimmten Aktivitäten entstandenen Formen propriozeptiver Stimulation) ein und meinte dann weiter:

"Obwohl es riskant ist, bin ich doch versucht, sie (die Psychoanalytiker, Anm. d. Verf.) beim Wort zu nehmen, weil diese (vielleicht künstliche) Vereinfachung in mein eigenes System perfekt passt, da sie als grundlegendes Konditionierungs- und Verstärkungsagens verstanden werden kann, von dem - so hoffe ich - alles übrige Verhalten ableitbar wird." (Notebooks, 16.02.1936, S.45, übers. v. Verf.).

Im Seminar bombardierte Hull die Analytiker weiterhin mit Fragen zur Natur der Libido, zum Konzept der Besetzung, Regression, etc., die er jedoch jeweils als im Kontext von Laboratorium oder Klinik (!) abklärungsbedürftig bewertete.³⁸⁾ *Und je mehr er im Rahmen des Integrationsversuches des Triebkonzepts bedurfte, desto mehr fühlte er sich in der Richtigkeit seiner Konzeption bestätigt.*³⁹⁾

Worin lag nun zu diesem Zeitpunkt die besondere Faszination des Libidokonzepts für Clark Hull und welche Spuren hinterliess seine intensive Auseinandersetzung mit der Freudschen Theorie letztlich in seinem eigenen System?

In der psychoanalytischen Theorie fasste man die Libido als eine veränderliche Kraft auf, als Substrat der Umwandlungen des Sexualtriebes. "Ihre (...) Produktion, Vergrösserung oder Verminderung, Verteilung und Verschiebung (soll) uns die Erklärungsmöglichkeiten für die beobachteten sexuellen Phänomene bieten", schrieb Freud 1905 (Freud 1905, S. 118, zit. n. Laplanche & Pontalis 1972, S. 285). Das quantitative Moment begeisterte Hull, wobei er zugleich qualitativ eine Uminterpretation in Richtung auf eine Desexualisierung der Libido (s.o.) vornahm. Nun hob Freud bereits in seinen frühen Schriften zur Angstneurose hervor, dass sich die durch eine Insuffizienz der psychischen Libido verursachte Anhäufung der sexuellen Spannung auf somatischer Ebene ohne psychische Bearbeitung in Symptome verwandeln konnte (Freud 1898, 1905). Hulls behavioristische Version des Prozesses der Symptombildung lautete 1939 folgendermassen:

"Als letzte konzeptuelle Äquivalenz greifen wir den Fall heraus, in dem ein Individuum mit schwerer Angst durch Versuch und Irrtum herausfindet, dass diese - zwar nur zeitweilig - durch ein spezielles Verhalten reduziert wird. Dieses Verhalten mag angemessen gewesen sein, als es das erste Mal auftrat. Nichtsdestoweniger wird das Effektgesetz es verstärken und es wird in allen möglichen unangemessenen Situationen wiederkehren. Der Bekanntenkreis des Patienten

wird ihn als eigentümlich oder neurotisch einstufen, und das sonderbare Verhalten wird als neurotisches Symptom betrachtet werden, was es ja auch tatsächlich ist. Dies ist die behavioristische Übersetzung des vielleicht bedeutsamsten psychoanalytischen Prinzips, des Prinzips der *Symptombildung*." (Hull 1939, S. 82, übers. v. Verf.).

Wie in Hulls späterer Konzeption des Lernprozesses unter Zuhilfenahme des neuen Triebkonzepts wurde auch bei Freud im Rahmen der Herausbildung von Symptomen ein *negativer Hedonismus* postuliert. Ausgangspunkt war ein Spannungszustand, der reduziert werden musste; seine Reduktion erfolgte im ungünstigen Fall durch die Entstehung von Symptomen, deren Beständigkeit sich aus lerntheoretischer Sicht nur verstärkungstheoretisch - im Sinne einer durch das symptomatische Verhalten kurzzeitig möglichen Angstreduktion - erklären liess. Da Clark Hull und seine Kollegen am *Institute of Human Relations* jedoch Symptome schon frühzeitig als *erlernte Verhaltensweisen* auffassten (vgl. Hull 1939, Mowrer 1939, Dollard & Miller 1950), - was lag näher, als den bei Freud beschriebenen *Lernprozess* auch von seiner theoretischen Konzeption her für die lernpsychologische Forschung nutzbar zu machen? Dass dieser Gedankengang für eine kleine Gruppe zugleich forschungsorientierter und klinisch engagierter Psychologen damals nicht ungewöhnlich war, zeigt u.a. auch die Vorwegnahme der Hullschen Konzeption des Lernprozesses, wie man sie wohl am deutlichsten in der Arbeit von English Bagby (1928) findet.⁴⁰⁾

Einziges heute noch zugängliches Ergebnis der frühen Integrationsversuche am *Institute of Human Relations* in Yale ist ein umfangreiches Manuskript, das Hull stark gekürzt 1939 veröffentlichte (Hull 1939). Entscheidender ist jedoch, dass Hull bis zu seiner "Presidential Address" im September 1936 in *Hannover* (vgl. Hull 1937) sein Motivationsproblem gelöst hatte. Neben der "incentive motivation" - Theorie präsentierte er in diesem ersten "Miniatursystem" das wesentlich bedeutsamere Prinzip der Motivation durch die Reduktion eines unangenehmen oder schmerzzerzeugenden Bedürfniszustandes. Die assoziative Verknüpfung von Reiz und Reaktion wurde nun als durch Trieb und Belohnung geregelt aufgefasst. Hull definierte den Trieb als einen starken Stimulus und unter-

schied zwischen primären und sekundären, d.h. erworbenen Trieben. Ausgangspunkt war hier die Annahme eines generellen Trieblevel D im Organismus, gebildet aus allen Einzeltrieben; dieses aktivierende Agens löste die Triebstimuli aus, deren schnelle Reduktion verstärkend wirkte.

Clark Hull blieb auch in den darauffolgenden Jahren ein engagiertes Mitglied der "Monday Night Group", wie sich die Forschergruppe in Yale nun nannte.⁴¹⁾ Jüngere Kollegen profilierten sich im Rahmen des mit ihm begonnenen Forschungsprogrammes. Der visionäre Gehalt seines Integrationsansatzes begeisterte angesichts der schwindenden Übereinstimmung zwischen akademischer und (aufstrebender) Klinischer Psychologie in den USA Zeitgenossen und Fachwelt.⁴²⁾ Das *Institute of Human Relations* erhielt aufgrund der erfolgreichen Forschung bis in die fünfziger Jahre Mittel aus der *Rockefeller Foundation*. Und ihr Initiator, Clark Hull selbst, meinte im Januar 1937 rückblickend, das Seminar habe ihn zwar von seiner zentralen Aufgabe, dem "magnum opus", abgelenkt, doch habe dieser Exkurs seine Theorie "reicher, wenn auch um einiges komplexer" gemacht.⁴³⁾

Abschliessende Bemerkungen

Tatsächlich hat Clark Hulls Theorie bis in die sechziger Jahre hinein viel fruchtbare Forschung angeregt. Doch stimmt sein Beispiel für eine deduktive Theorienbildung - ohne ausreichende bzw. kontinuierliche experimentelle Absicherung einzelner konzeptioneller Schritte - auch kritisch. Ein frühes Produkt des Hullschen Vorgehens war die Triebreduktionshypothese; ein späteres die Formalisierung seiner Theorie bis hin zur Unverständlichkeit (Hull 1952).⁴⁴⁾ Clark Hulls grosse Wirkung auf die Psychologie seiner Zeit war in seinem theoretischen Anspruch, aber auch - und dies ist bisher in der Bewertung seines Werkes nicht berücksichtigt worden - in seinem vom *Ziel der Integration* der vielen unterschiedlichen und unverbundenen psychologischen Theorien seiner Zeit bestimmten Programm und einer damit einhergehenden, wirklich ungewöhnlichen, geistig-wissenschaftlichen Liberalität begründet.

Hulls theoretischer Formalismus und seine in bezug auf die - damals als völlig unwissenschaftlich klassifizierte - Psychoanalyse offenen-interessierte Haltung kennzeichnen nicht nur die Person. Sie sind auch Eckpfeiler eines spannungsreichen Forschungsprogramms, das die wissenschaftliche Psychologie bisher nur sehr einseitig rezipiert hat. Solche Rezeptionsprozesse und ihre Ursachen offenzulegen, ist eine zentrale Aufgabe der historischen Forschung in der Psychologie.

Nicht zuletzt - und dies kann hier nicht weiter ausgeführt werden - dokumentieren die in Yale damals geschlossenen "Kompromisse" schlaglichtartig Momente der Entwicklung der wissenschaftlichen Psychologie von einem grundlagenorientierten Fach hin zur anwendungsorientierten Wissenschaft. Inwieweit Hulls Theorie Umsetzungsprozesse dieser Art gefördert hat - z. B. im Falle der südafrikanischen und englischen Verhaltenstherapeuten der fünfziger Jahre - bleibt noch zu untersuchen.

Anmerkungen

- 1) Bis heute liegt keine Biographie vor, es bietet sich an, seinem Lebenswerk durch differenzierte Analysen einzelner Aspekte seiner Arbeit näher zu kommen, vgl. z. B. bei Triplet (1982).
- 2) Sie befinden sich in der Sterling Memorial Library, Yale University, New Haven, einzelne Informationen enthalten auch die "President Angell Collection" sowie der Nachlass des "Institute of Human Relations". Tatsächlich bildeten Hulls Notebooks ein wichtiges Instrumentarium seiner theoretischen Arbeit, die häufig auf sogenannten "Gedankenexperimenten" basierte. So beginnen viele Eintragungen mit Worten wie 'It struck me this morning ...' etc., werden als 'Idea', 'Note', 'Later Note', 'Theoretical question', 'Plans regarding s.th.' tituliert.
- 3) Clark Hull fasste den wissenschaftlichen Fortschritt als zunehmende Integration und zahlenmässige Reduktion wissenschaftlicher Theorien auf. Sein Ziel war eine übergreifende Theorie des Verhaltens, ein Ansatz, der auch sogenannte "psychische" oder "kognitive" Phänomene einschliessen sollte. Herausragendes Prinzip seiner theoretischen Arbeit war das Prinzip der Deduktion. Durch die Deduktion einzelner Hypothesen von bekannten Erklärungsprinzipien sollten wissenschaftliche Systeme erweitert werden. Für die abgeleiteten Hypothesen galt die Forderung, dass sie experimentell überprüfbar sein mussten. Hulls hypothetische Konstrukte (Hull 1935) - auch das Triebkonzept (D)-befanden sich jedoch immer in Gefahr, verdinglicht zu werden.

- 4) Wenig über Hulls Haltung zur Psychoanalyse erfährt man z. B. in so differenzierten Darstellungen wie Amsel & Rashotte (1977) oder Lindzey (1954). Ausgespart wurde dieser Aspekt ebenso bei der auszugsweisen Veröffentlichung seiner Notebooks (Ammons & Ammons 1962) und auch in anerkannten Lehrbüchern (z. B. Hilgard 1948). Mc Clelland (1957) unternahm einen weitgehend unhistorischen Vergleich beider Ansätze.
- 5) Zwar verwahrte sich Dunlap (1927) gegen die Schlussfolgerung einiger Kommentatoren des ersten Bandes (Murchison 1926), die meinten, dass es nutzlos sei herausfinden zu wollen, in welche Richtung sich die Psychologie eigentlich entwickle. Aber auch Murchison selbst erschienen die experimentellen Methoden nur noch als Momente mehr oder weniger systematischer Theorien verschiedener Experimentatoren, und er sah darin eine 'pädagogische Gefahr' für Studenten und Öffentlichkeit: The pedagogical danger here is caused by a tendency of each of these theoretical groups to think of its rivals in terms of caricature, and so to describe them to the public and to young students. The result is that theoretical tradition becomes established in certain educational communities, and students are born structuralists or behaviorists just as one may be born a democrat or a presbyterian." (1926, Preface).
- 6) S. dazu auch Angela Schorr: "Frühe Formen der Verhaltenstherapie und die Konfrontation mit der Psychoanalyse - Amerika 1890 - 1930", unveröffentlichte Diplomarbeit, München 1978.
- 7) So beurteilte z. B. William James Freud als "a man obsessed with fixed ideas", meinte jedoch nach Freuds Besuch in den USA 1909: "I hope that Freud and his pupils will push their ideas to their utmost limits, they can't fail to throw light on human nature." (zit. n. Stendler 1947, S. 194), deutlich wurde dies auch auf dem "Symposium on the Contributions of Freudism to Psychology" (Jastrow 1924, Lashley 1924, Leuba 1924, Thurstone 1924), dessen Veranstalter damals bereits die American Psychological Association war. Während man einerseits bedacht war, seine Respektabilität als Naturwissenschaftler zu wahren, und die Psychoanalyse vor allem auch aufgrund ihres deskriptiven Vokabulars als 'arm-chair method' ablehnte (Brown 1940, Klein 1933), überzeugte die persönliche Erfahrung als "man in the street" (Heidbreder 1940, S. 1927), siehe dazu auch Burnham 1967).

Ambivalenzen gegenüber dem Behaviorismus Watsons kündigten sich bereits früh an (Angell 1913). 1915 bezeichnete Angell ihn als "scientifically unsound and philosophically essentially illiterate" und Titchener bezweifelte, dass diese neue "Technologie" die Psychologie als Wissenschaft ersetzen könne (zit. n. Sexton & Misiak 1966), siehe auch die Ergebnisse einer Umfrage Dunlaps 1916, ebenso die Korrespondenz zwischen John B. Watson und Adolf Meyer, damals Amerikas prominentester Psychiater, sowie zwischen John B. Watson und Robert M. Yerkes zwischen 1912 und 1920, erstere in "The Adolf Meyer Archive", Johns Hopkins University, Baltimore, letztere in der "Robert Mearns Yerkes Collection", Sterling Memorial Library, Yale University. Ebenso Samelson (1979, 1980, 1981) und Watson (1928).

- 8) So schreibt May 1976: "Während die Psychoanalyse in Europa nach dem ersten Weltkrieg noch auf viel Feindseligkeit stieß, genoss sie in den USA zwischen 1920 und 1930 ein Mass an Popularität, dass später nie mehr erreicht wurde. Die Psychoanalyse wurde Mode (...) 1920 wurden allein fast 12 000 Exemplare der Vorlesung zur Einführung in die Psychoanalyse in Amerika verkauft. Zeitungen brachten ausführliche Interviews mit Psychoanalytikern (Adler, Brill, Wittels, Ferenczi, Freud usw.) und von Patienten verfasste Berichte über ihre Therapie. Reporter reisten nach Wien, um den 'genius' zu ein paar Worten zu bewegen." (1976, S. 1229). Aber auch Dunlaps minutiöser Verriss der Psychoanalyse wurde schon allein wegen einiger beissender Besprechungen bekannter Analytiker zu einer Sensation. (Jones 1922, Woodworth 1922).
- 9) Die Wirkung solcher "emotional befriedigender" Erklärungen (Dunlap 1920, S. 93) wurde frühzeitig erkannt (s.a. Burnham 1924, S. 158), aber Anerkennung und Kritik wechselten einander ab, wie z. B. bei Hollingworth 1926 (Preface), der zwar weiterhin skeptisch blieb in bezug auf die Libidotheorie, das Konzept des Unbewussten, etc., jedoch Freuds Theorie zugestanden, dass sie einige "sober contributions" enthalte.
- 10) Bedeutsam war auch der Mangel an klinischer Erfahrung bei vielen Psychologen sowie die Tatsache, dass die psychologischen Fachbereiche an den Universitäten unter dem Druck der öffentlichen Diskussion dazu übergehen mussten, einen neuen Typus von Kurs einzuführen, der unter dem Titel "human adjustment" lief, jedoch auch "Freudsche Themen" einschloss. (Skakow & Rapaport 1968). So mancher junge Wissenschaftler, z. B. auch Clark Hull, begann damals seine Laufbahn im Bereich der wissenschaftlich wenig angesehenen "abnormal psychology" (Hull 1952).
- 11) Siehe dazu Thurstone (1924), Hamilton (1925), Wells (1922). So warb Wells damals z. B. mit dem Argument: "Ideally, it (psychoanalysis, Anm. d. Verf.) is to experimental psychology something of what the good hospital is to the good school" (Wells 1922, S. 456).
- 12) Die wissenschaftliche Karriere Watsons fand 1920 ein jähes Ende. Durch einen Scheidungsskandal und die dabei aufkommenden Gerüchte über Experimente zur menschlichen Sexualität, die er damals begonnen hatte, verlor er seine Professur an der *Johns Hopkins University* (Magoun 1981). Obwohl nur wenige seiner Kollegen auch nach dem Skandal zu ihm hielten, sorgte Watson bis Ende der zwanziger Jahre durch zahlreiche Veröffentlichungen dafür, dass sein behavioristischer Ansatz in der Psychologie stets präsent war.
- 13) So berichtete "The Book World" vom 08.04.1928 über Watsons (1927) Buch unter der Überschrift: "Dr. Watson's Book on Child-Nurture Called as Epoch -Making as Darwin's Findings". Der bekannte Philosoph Bertrand Russell meinte in der "Evening Post" vom 18.05.1928: "Whether or not Dr. Watson is right in all his details, his book has undoubtedly one very great and rare merit, and that is that its approach to the problem is scientific."

- 14) Siehe dazu Humphreys frühe Kritik 1925 sowie die Untersuchungen von English (1929) und Bregman (1934), ebenso die Korrespondenz zwischen H. E. Jones und Elsie Bregman vom 19.11.1934, Archives of the History of American Psychology, Akron.
- 15) Gespräch mit Orval Hobart Mowrer in Champaign, Illinois, vom 10.10.1979.
- 16) Hulls Forschungs- und Publikationsthemen lagen zuvor im Bereich Eignungsdiagnostik, der Hypnose und Suggestion. Wie viele seiner damaligen Kollegen interessierte er sich eigentlich für die Grundlagenforschung, musste seine Legitimation jedoch lange Zeit in angewandter Arbeit suchen. Dazu auch O'Donnell (1979).
- 17) Notebooks, 04.06.1933, siehe auch Hull (1962), S. 853.
- 18) Siehe Fussnote 17).
- 19) Notebooks, 09.09.1934, siehe auch Hull (1962, S. 857).
- 20) Alle Konzepte finden sich bereits in einer Eintragung vom 09. August 1930, in der er auch ihre Präsentation plant, bereits 1931 verwendete er z. B. das Konzept der "habit family hierarchy" in seinen Vorlesungen (Mills 1978).
- 21) Als grundlegend betrachtete Hull für die Ableitung von Hypothesen jedoch die Forderung, dass sie experimentell überprüfbar waren und der "impartial arbitration of facts" zugeführt werden konnten. (Hull 1930, S. 252).
- 22) Die Kennzeichnung wissenschaftlicher Theorien als "Propagandazentren" zur Stützung spezifischer Forschungsprogramme war Hull auch in späteren Jahren nicht fremd. Vgl. "Abstract of Professor Hull's Informal Seminar", May 25, 1939.
- 23) Initiatoren des IHR waren der Präsident der Yale Universität James R. Angell, der Dekan der Yale Law School Robert Hutchins sowie der Dekan der Yale Medical School, Milton Winternitz. Einerseits wollte man das alte Ideal von der Freiheit der Forschung realisieren, andererseits die *Fortschritte in der Grundlagenforschung mit der Ausbildung von Studenten für praktische Aufgaben verbinden*. Forschungsfreiheit galt als Synonym für hohe Produktivität. Dieses Programm begeisterte viele, doch als die *Rockefeller Foundation* die Mittel für den Aufbau des Instituts nur begründet bewilligte, liess das Interesse interessierter Juristen, Mediziner, Pädagogen, Theologen und Sozialwissenschaftler nach. So kam es lediglich zu einer Vielzahl von Einzelprojekten, die jedoch auch in den einzelnen Fachbereichen realisierbar gewesen wären. Diejenigen Wissenschaftler, die sich in ihrem eigenen Fach bereits einen Namen gemacht hatten, brauchten diese neue Art der Forschung nicht mehr, und jüngere Wissenschaftler konnten sich durch konsequente interdisziplinäre Forschung in ihrem eigenen Fach nicht profilieren. Auch rein organisatorisch empfand man das Institut im Rahmen der Universität als Fremdkörper und neidete den dort tätigen Kollegen die ausschliessliche Möglichkeit zur Forschung, während man selbst noch Lehrverpflichtungen hatte (Dollard 1964, May 1971).
- 24) Mark A May: "Memorandum on the Institute of Human Relations". April 23, 1935, sowie: "Memorandum on Future Research Plans of

- the Institute, September 1935". Beide "IHR - Collection", Sterling Memorial Library, Yale University.
- 25) E.R. Hilgard meinte dazu in einem Brief an A. Schorr vom 14.04. 1980: "He wanted his own behaviorism to cover everything including thought processes and purpose." Siehe dazu auch Hull (1937).
- 26) Siehe Fussnote 17).
- 27) Hull bezeichnete diese Überlegung als "a true gedanken experiment". Notebooks, 09. September 1934.
- 28) Hollingworths Prinzip der "redintegration" wurde von ihm selbst 1930 folgendermassen formuliert: "Mental activity, whether normal or abnormal, is seen to be characterized, in its most general terms, by the redintegrative sequence. Partial stimuli now occurring function for former antecedents of greater complexity. This is the general pattern of mental activity. Two aspects of such processes we designate 'learning' and 'sagacity'." (Hollingworth 1930, Preface). Vgl. Hollingworth 1920, 1926, 1928 und 1930. Ähnlich wie Edwin Guthrie vertraute Hollingworth mehr auf die Plausibilität seines experimentell nur schwer zugänglichen Konzeptes, vertrat zugleich jedoch mit Nachdruck den Standpunkt eines empirischen Wissenschaftlers - eine für die zwanziger und dreissiger Jahre in der amerikanischen Psychologie recht typische Konstellation!
- 29) Hulls Experimente mit Ratten machten ihn jedoch auf die Bedeutung motivationaler Aspekte des Verhaltens aufmerksam: Die Tiere liessen sich häufig nur in depriviertem Zustand zu bestimmten Handlungen bewegen. Aus der Korrespondenz zwischen Neal Miller und Angela Schorr vom 15.05.1980.
- 30) Bereits 1929 hatte ein Mitglied des Exekutivkommittees des IHR, Donald Schlesinger, den Vorschlag gemacht, die psychoanalytische Theorie als Ausgangspunkt für ein interdisziplinäres Forschungsprogramm einzusetzen und dies mit Hilfe einiger ausgebildeter Analytiker, die man fest anstellen könnte. "Psychoanalysis and the Institute of Human Relations, Memorandum, January 1929". Dieses Vorhaben scheiterte am Einspruch von Dorothy Thomas, die ihm nur wenig Erfolgsaussichten einräumte, wenn Psychoanalytiker selbst beauftragt werden sollten, die Psychoanalyse wissenschaftlicher zu machen. Sie schlug statt dessen vor: "... the testing should be done by persons thoroughly trained in scientific observation, with a good working knowledge of psychoanalysis, but not by those who have been indoctrinated." "Comments by Dorothy Thomas on Mr. Schlesinger's memorandum entitled 'Psychoanalysis in the Institute of Human Relations'", undatiert, beides IHR-Collection, Yale University.

Und tatsächlich folgte man dieser Empfehlung: Neal Miller, ein hochbegabter junger Experimentalpsychologe, der sich für die Psychoanalyse interessierte, erhielt wie schon im Jahr zuvor der Soziologie John Dollard ein Stipendium des Social Science Research Council für das Jahr 1935, um sich in Europa analysieren zu lassen. John Dollard ging zu Hans Sachs nach Berlin, Neal Miller im folgenden Jahr zu Hans Hartman nach Wien. (Aus

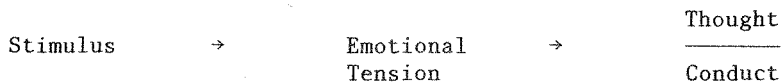
den Gesprächen mit John Dollard und Neal Miller vom 07.09. und 14.09.1979) Orval Hobart Mowrer, ebenfalls ein begabter Theoretiker und Experimentator, hatte sich aufgrund persönlicher Probleme bereits 1929 kurz in analytische Behandlung begeben und wurde später wegen seiner Ängste und Depressionen von John Dollard analysiert. (1935 bis 1938, aus dem Gespräch mit O. H. Mowrer vom 10.10.1979). John Dollard führte am Institut inoffiziell auch in den folgenden Jahren Analysen bei Kollegen und Studenten durch, verwandte sie jedoch mehr zu Forschungszwecken, d. h. er betrachtete sie als datengewinnende Methode. (Aus dem Gespräch mit Robert Sears, 17.09.1979). Robert Sears hatte unabhängig von der Forschergruppe in Yale an der University of Illinois mit experimentellen Untersuchungen zur Projektion begonnen (1934, 1936, 1937), angeregt durch einen Kurs in "abnormal psychology", den er als jüngeres Fachbereichsmitglied halten musste. Hull wurde so auf seine Arbeiten aufmerksam und holte ihn 1936 nach Yale.

- 31) Zinn hatte durch seine Zusammenarbeit mit dem Arzt, Behavioristen und Psychoanalytiker G. V. Hamilton (1925, 1928) im Auftrag des Social Science Research Council gleich einen guten Einstieg in das neue Projekt. Er setzte am Institut seine Aufzeichnungsstudien von der Psychoanalyse eines Patienten fort und gab informelle Seminare über die analytische Theorie, die auch Hull regelmässig besucht, da man erkannt hatte, dass man zunächst einmal voneinander lernen musste, um zu einer fruchtbaren Zusammenarbeit zu kommen. Hull bezeichnete Zinn einmal als "a psychoanalyst with a truly scientific point of view", der zwar ausschliesslich auf die psychoanalytische Theorie spezialisiert sei, den er aber auch als "sympathetic with the behaviorist interpretation" einschätzte. D. L. Hull, Memorandum an M. A. May von 23.04.1940, aus den privaten Unterlagen von Neal Miller.
- 32) "Es ist kaum glaubhaft, meinte Hull weiter, dass verschiedene Verhaltenswissenschaften wie die Pavlovsche Reflexologie, der amerikanische Behaviorismus und die psychoanalytische Theorie sich weiterhin unabhängig voneinander entwickeln, ohne eine Integration, die umfassender sein wird, als jeder dieser Ansätze allein." Clark Hull, Memorandum to Psychology 106, 23.04.1940. Aus den privaten Unterlagen Neal Millers.
- 33) Shakow & Rapaport (1968, S. 136), die sich lediglich auf die veröffentlichten Passagen aus Hulls Notebooks stützten, kamen zu dem Schluss, dass der Unterschied zwischen Freuds Konzept des "instinctual drive" und Hulls Konzept des Triebes zu gross sei, als dass man annehmen könnte, dass sein Konzept in Freuds Theorie seinen Ursprung hatte. Sie machten sich die Auffassung von May und Miller zu eigen, die sein Triebkonzept als aus der experimentellen Arbeit mit Versuchstieren entstanden auffassen. Neal Miller hat jedoch ein gutes Stück des Seminars nicht miterleben können, da er sich damals noch in Wien aufhielt.
- 34) Aus der Ankündigung zum Seminar: "This is the first attempt of a proposed series of seminars which will be devoted to an attempt to integration the major concepts and principles of the conditioned reaction with those of psychoanalysis." (Hull

1936, zit. n. Shakow & Rapaport 1968). Als zentrales Thema des Integrationsversuches auf theoretischer Ebene hatte Clark Hull im Jahr zuvor den Bereich der Motivation vorgeschlagen. John Dollard, der wohl einflussreichste Meinungsführer unter den jüngeren Mitarbeitern des Instituts, bewertete es jedoch zunächst als "just a white-wash und argwöhnte, dass diejenigen, die es vorgeschlagen hatten, im Grunde nur daran interessiert seien, ihre tierexperimentellen Studien fortzusetzen und sie mit den Zielsetzungen des Instituts zu legitimieren suchten. Erst Anfang 1936 liess er sich davon überzeugen, dass es möglich ist, motivationale und kulturelle Phänomene im Zusammenhang zu betrachten und stimmte der Durchführung eines ersten gemeinsamen Seminars zu. C. L. Hull, "Memorandum to Professor Mark A. May, Director of the Institute of Human Relations, May 31, 1935"; Brief von C. L. Hull an M.A. May, 04.06.1935; Brief von C. L. Hull an M. A. May, 20.06.1935; B. M. Castner & H. M. Halverson, "Memorandum on Motivation", 17.06.1935; Brief von John Dollard an M. A. May, 26.09.1935; C. L. Hull, "Memorandum to Mark A. May and his Committee on Agenda for the Institute of Human Relations", 03.12.1935; Brief von John Dollard an M. A. May, 16.01.1936. Alles in: "IHR - Collection, Sterling Memorial Library, Yale University.

- 35) "If this is so, schrieb Hull im Februar 1936, then it is to be expected at once that all the things found out experimentally about simple trial-and-error learning will hold true of the more complex situations in which psychoanalysis has studied the action of the libido and that the things found out about the libido in these latter situations will later be found to be true in the simple trial-and-error situations assumed at first." (Notebooks, 09.02.1936, S. 6 - 7).
- 36) Notebooks, 09.02.1936, S. 4 - 44.
- 37) Notebooks, 11.02. 1936.
- 38) Notebooks, 09.02.1936, S. 43.
- 39) Allerdings erschien er sich auch manchmal selbst fragwürdig in seiner unkritischen Akzeptanz psychoanalytischer Konzepte. So fragte er sich z. B. im März 1936, warum er, wie die meisten anderen Menschen auch, fast automatisch ein Konzept wie Regression, Evolution der Libido oder Kathexis als Theorie klassifiziere, obwohl sichtbar auch nicht die Spur einer logischen Deduktion darin enthalten sei. Und er erklärte es sich damit, dass die durch den Mangel an empirischer oder konventioneller Evidenz erzeugte Aura der Unrealität, die diese Konzepte umgibt, auch für vieles charakteristisch ist, was als wissenschaftliche Theorie durchgeht - also mit dem nach wie vor unbefriedigenden Zustand der wissenschaftlichen Psychologie. (Notebooks, 10. März 1936, S. 88).
- 40) In seinem Buch *The Psychology of Personality; an Analysis of Common Emotional Disorders* (1928) schlug Bagby erstmals ein neues Konzept, das "tension-reduction principle" vor und begründete dies damit, dass es. z. Zt. keine sinnvolle Behandlung des Triebes in der Psychologie gebe. Wie viele wissenschaftlich oreintierte Klinische Psychologen seiner Zeit war Bagby lerntheoretisch orientiert und interpretierte die Ent-

stehung emotionaler Fehlentwicklungen mit Hilfe von Watsons Prinzip der konditionierten emotionalen Reaktion. Zugleich war er jedoch - wie die Mehrheit der Klinischen Psychologen Ende der zwanziger und Anfang der dreissiger Jahre - fasziniert von der Theorie Freuds und ihrem interpretativen Potential im Bereich gestörten Verhaltens. Auch für English Bagby stellte sich die Frage, wie sich die Aufrechterhaltung falscher, d. h. symptomatischer Verhaltensweisen lerntheoretisch erklären liess. In Kapitel 5 seines Buches, "Die Reduktion emotionaler Spannungen", schlug er folgendes Modell vor: Ein bestimmter Stimulus erzeugt emotionale Spannung, die sich in kognitiven Prozessen und bestimmten Verhaltensweisen niederschlägt:



(vgl. Bagby 1928, S. 70).

Das Auftreten emotionaler Spannung auf einen bestimmten Stimulus hin erklärte Bagby über das Prinzip der klassischen Konditionierung. Wie jedoch wurden erklärte sich der zweite Teil der Reaktionskette? Bagby fand folgende Antwort: Die emotionale Spannung fungiert als Trieb, d. h. eine neue Situation löst zunächst (motorisches) Versuch-und-Irrtum Verhalten aus und mobilisiert Denkprozesse; beides kommt jedoch nur zustande, wenn ein Triebzustand vorliegt, z. B. Angst empfunden wird. In einem zweiten Stadium der motorischen Gewohnheitsbildung kommt es zu einer Reduktion der emotionalen Spannung, die verstärkend wirkt. Bagby selbst formuliert diesen zweiteiligen Prozess folgendermassen:

"An emotional visceral tension operates as a drive and determines that the individual, if facing an unfamiliar situation, shall begin immediately to exhibit trial-and-error behavior. If one of the trials or a combination of them results in the reduction of the tension, it becomes a habit, and functions whenever the drive and the situation are again present. Such is the origin of persistent traits of emotional conduct and thinking which constitute the human personality." (Bagby 1928, S. 73).

- 41) Unter dieser Firmierung arbeitete die "Monday Night Group", die sich als "informal seminar for the exchange of concepts and guiding ideas for research" verstand, noch einige Jahre weiter, lud viele bekannte Kollegen zum Informationsaustausch ein (u.a. Edward Sapir, der Analytiker Erik Erikson und Margaret Mead) und verschickte Manuskripte an interessierte Kollegen ausserhalb. Die Vision einer grossen Integration, einer "supra-theory", begeisterte in den vierziger Jahren viele Zeitgenossen. (Z. B. Brown 1940 a, 1940, Bergman 1943).
- 42) S. dazu auch: Angela Schorr: *Die Verhaltenstherapie. Ihre Geschichte von den Anfängen bis zur Gegenwart*. Weinheim 1984.
- 43) Am Ende des Seminars enthielt Hulls Notebook allein 40 experimentelle Entwürfe zum Thema Libido, 7 zu Kathexis, 25 zu Frustration und Regression. (Notebook 1936, S. 115) Zitat Notebook vom 04.01.1937 bzw. 17.01.1937.

- 44) Schon bei der Veröffentlichung seiner Artikel, wie auch später in der Monday Night Group erwog Hull sorgfältig, wieviel an "Kampf" mit seinen Deduktionen er seinen Kollegen zumuten konnte. (Fussnote 19 sowie Notebook, 16.02.1936).

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DIE ROLLE SOZIALPSYCHOLOGISCHER FAKTOREN IN
DER PSYCHOLOGIEGESCHICHTE: DER FALL WUNDT UND FREUD

Christfried Tögel

Bulgarische Akademie der Wissenschaften

Philosophisches Institut

Sofia, Bulgarien

Summary

The opposition of academic psychology and psychoanalysis is one of the most characteristic features of psychology in our century. The explanation of this fact is usually sought in the theoretical differences between these two systems.

The thesis of this paper is that, in addition to the theoretical differences, the social positions of Wundt and Freud as founders of experimental psychology and of psychoanalysis is of great importance. Their extraordinary roles as founders of schools and leaders of movements became for them an obligation: Both believed that they had to keep their theories "uncontaminated". In order to do this they kept their theories clear from alien influences and broke off relations with self-willed pupils (Külpe, Jung).

Moreover, the historiography of academic psychology as well as that of psychoanalysis have built up two types of self-understanding of their representatives which correspond only to a certain extent to the real roles, played by Wundt and Freud in the history of psychology. In this case, it seems, that historiography is reduced to a justification of their own professional existence: Psychology is identified with experimental psychology on the one side, and on the other psychoanalysis is presented as a victim of misunderstanding and unintentional isolation.

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"Eine neue wissenschaftliche Wahrheit pflegt sich nicht in der Weise durchzusetzen, dass ihre Gegner überzeugt werden und sich als belehrt erklären, sondern vielmehr dadurch, dass die Gegner allmählich aussterben und dass die heranwachsende Generation von vornherein mit der Wahrheit vertraut gemacht ist."¹

Diese Bemerkung Max Plancks in seiner *Wissenschaftlichen Selbstbiographie* ist wohl eine der radikalsten Absagen an die Verabsolutierung systematischer Faktoren in der Wissenschaftsentwicklung. Seit Thomas Kuhns Buch *The Structure of Scientific Revolutions*² ist das Interesse an ausserwissenschaftlichen Bedingungen der Entwicklung einzelner Wissenschaftsdisziplinen ausserordentlich gewachsen. Inzwischen gibt es auch eine beträchtliche Anzahl von Versuchen, Kuhns Konzept auf die Psychologiegeschichte anzuwenden.³ Allerdings beschränkt man sich dabei im wesentlichen auf die Frage, ob die Psychologie noch im vorparadigmatischen Stadium ist oder schon den Charakter einer "reifen" Wissenschaft hat. In Darstellungen der Psychologiegeschichte werden ausserwissenschaftliche Faktoren nur selten erwähnt.⁴ Man stellt die systematischen Gesichtspunkte dar und glaubt damit, den Gang der Geschichte des Faches hinreichend erklärt zu haben. Zweifellos sind innere Faktoren ganz entscheidend für die Wissenschaftsentwicklung, doch kann man nicht sie allein in für alles verantwortlich machen.

Mir scheint, dass in der Psychologiegeschichte neben "biologischen" Faktoren, wie denen von Planck angedeuteten, besonders sozialpsychologische eine wichtige Rolle spielen. Das soll im Folgenden anhand des Verhältnisses von akademischer Psychologie und Psychoanalyse gezeigt werden.

Die Namen Wundt und Freud werden selten in einem Atemzuge genannt. Beide Männer scheinen auch nicht viel gemeinsam zu haben. Der Schein trügt jedoch. Die Parallelen in der Entwicklung beider Forscher sind verblüffend. Und es sind u.a. diese Parallelen - nicht nur die Unterschiede im theoretischen System - die zu der für die Psychologie des 20. Jahrhunderts charakteristischen Gegenüberstellung von Psychoanalyse und akademischer Psychologie geführt haben.

Ich will versuchen, diese These plausibel zu machen. Sowohl Wundt als auch Freud haben Medizin studiert und sich dann auf Physiologie bzw. Histologie konzentriert. Sie arbeiteten als Assistenten in einem Physiologischen Institut - Wundt bei Hermann von Helmholtz in Heidelberg und Freud bei Ernst von Brücke in Wien.

Helmholtz und Brücke - beide befreundet und eng verbunden durch die gemeinsame Sache der "exakten" Physiologie, deren Grundlage sie in den vierziger Jahren des 19. Jahrhunderts gemeinsam mit Emil Du Bois-Reymond und Carl Ludwig gelegt hatten⁵ - wurden so zu Lehrern derjenigen beiden Männer, die eine traditionelle Psychologiegeschichte nicht zusammen denken kann. Wundt sah im täglichen Verkehr mit Helmholtz einen Vorzug, "der sicherlich nicht hoch genug eingeschätzt werden kann"⁶ und für Freud war Brücke "die grösste Autorität", die je auf ihn gewirkt hat.⁷

Beide sind um die vierzig als sie das Erbe ihrer Lehrer in die Psychologie einbringen: Wundt übernimmt aus der Physiologie das Experiment und gibt in seiner *Physiologischen Psychologie*⁸ eine systematische Darstellung der so neu entstandenen Richtung. Freud wendet die energetische Betrachtungsweise seines Lehrers Brücke auf psychische Vorgänge an und kommt so über seinen *Entwurf einer wissenschaftlichen Psychologie*^{8a} zur *Traumdeutung*⁹ und damit zu den Grundlagen der psychoanalytischen Theorie.

Wundt hatte 1873/74 als seine *Physiologische Psychologie* erschien den Standortwechsel von der Physiologie zur Psychologie längst vollzogen.¹⁰ Er war der erste, der sich nicht nur als Physiologe oder Philosoph über Psychologie geäußert hat, sondern der selber Psychologe geworden ist, der die Sache der Psychologie zu seiner eigenen gemacht hat. Wundt blickte von der Psychologie aus hinüber zu Physiologie, Biologie und Philosophie; seine Vorgänger dagegen hatten von jenen Wissenschaften aus u.a. auch einen Blick auf die Psychologie geworfen. Dieser Blickwinkel Wundts und sein Wissen um die Erneuerungsbedürftigkeit einer in der Krise steckenden philosophischen Psychologie schärften seine Aufmerksamkeit für Ergebnisse, Theorien, Methoden und methodologische Prinzipien der Nachbarwissenschaften, die für die Psychologie fruchtbar sein könnten.

Auch Freud hat einen Standortwechsel von der Physiologie zur Psychologie vollzogen. Auch seine Aufmerksamkeit gilt Ansätzen in den Nachbarwissenschaften, die die Psychologie auf eine neue Grundlage stellen könnten.

Neben der Physiologie ist es der Entwicklungsgedanke, der für beide entscheidende Bedeutung erlangt: er spielt die Rolle eines integrativen Elements sowohl in der Wundtschen¹¹ als auch in der Freudschen¹² Psychologie, obwohl der eine (Wundt) ihn voluntaristisch¹³ und der andere (Freud) ihn lamarckistisch¹⁴ interpretiert.

Die Parallelen liessen sich noch weiter verfolgen: über die Bedeutung assoziativer Prozesse sowohl in der Wundtschen als auch in der Freudschen Theorie¹⁵ bis hin zu den Altersinteressen beider Wissenschaftler an Fragen der Kultur. Wir wollen jedoch die Perspektive wechseln und die Parallelität der sozialen Rollen betrachten, in die Wundt und Freud als Schulengründer hineingeraten sind.

Im Jahre 1875 wird Wundt nach Leipzig berufen. Dort entsteht vier Jahre später ein psychologisches Laboratorium, in dem unter Wundts Leitung Studenten aus der ganzen Welt experimentieren, die meisten von ihnen aus den USA, Österreich, Rumänien, Bulgarien und Serbien. 1883 gründet Wundt dann die *Philosophischen Studien*, die erste Zeitschrift der Welt, die Ergebnisse experimentalpsychologischer Forschung publiziert, und im Jahre 1889 findet dann der "I. Internationale Kongress für physiologische Psychologie" in Paris statt. Innerhalb weniger Jahre wurde Wundt so zum international anerkannten Oberhaupt der Psychologie.¹⁶

Freuds Weg verlief analog: Im Oktober 1902 lädt er zur ersten "Psychologischen Mittwochs-Gesellschaft" ein - er ist damals genauso alt wie Wundt, als das Laboratorium entsteht. Aus dieser Gesellschaft geht 1908 dann die "Wiener psychoanalytische Vereinigung" hervor. Im gleichen Jahr findet auch der erste "Internationale psychoanalytische Kongress" in Salzburg statt und 1909 gründet Freud das *Jahrbuch der Psychoanalyse*. Zu dieser Zeit hat Freud schon Schüler in der ganzen Welt: C.G. Jung in Zürich, Karl Abraham in Berlin, Sandor Ferenczi in Budapest, Abraham Brill in New York und Ernst Jones in Toronto.

Wundt und Freud befanden sich also in der gleichen Lage: Beide hatten neuartige Institutionen geschaffen - Wundt sein Laboratorium und Freud die "Wiener psychoanalytische Vereinigung", beide hatten Zeitschriften herausgegeben, in denen Arbeiten zu bisher nicht

diskutierten Problemkreisen veröffentlicht wurden und schliesslich sind beide zu Häuptern von Bewegungen geworden, die den ganzen Erdball umspannten. Zu Beginn des 20. Jahrhunderts gab es auf dem Gebiet der Psychologie keine dritte, ähnlich exponierte Persönlichkeit.

Es wird nun zu zeigen sein, dass es neben den unüberbrückbaren sachlichen Gegensätzen¹⁷ auch diese aussergewöhnliche Stellung beider Forscher ist, aus der die Entgegensetzung von akademischer Psychologie und Psychoanalyse erwächst.

Sowohl für Wundt, als auch für Freud wurde diese Stellung zur Verpflichtung. Beide glaubten ihre Theorien "rein" halten zu müssen. Das geschah einerseits durch Abschirmung gegen äussere Einflüsse und andererseits durch Trennung von eigenwilligen Schülern. Z.B. findet die Kontroverse zwischen Wundt und Ernst Meumann um das Profil des *Archives für die gesamte Psychologie*¹⁸ ihre Entsprechung in der Auseinandersetzung zwischen Freud und Paul Federn um die *Zeitschrift für Psychoanalytischen Pädagogik*¹⁹: In beiden Fällen war die Angst vor Verwässerung und Popularisierung der Theorie das entscheidende Motiv auf seiten der Schulengründer.

Den anderen Abwehrmechanismus, die starre Verteidigung eines rational nicht begründbaren Dogmas bekam Oswald Külpe ebenso zu spüren wie C.G. Jung: beide gründeten als Reaktion eigene Schulen.

Um die Jahrhundertwende, als die Psychoanalyse noch keine Bewegung war, schienen Wundtsche und Freudsche Psychologie sich keineswegs von vornherein zu widersprechen. Es gibt Hinweise dafür, dass von seiten der Wundt-Schule frühe psychoanalytische Auffassungen durchaus akzeptiert wurden. So findet sich in der Arbeit "Psychologie und Nervenheilkunde" von Willy Hellpach folgende Bemerkung: "Gerade die von Breuer und Freud empfohlene Behandlung ... knüpft an die allbekannte, auch von Wundt anlässlich der Erinnerungsvorgänge geschilderte Tatsache an, dass unliebsame und ihrem Fortwirken ganz unberechenbare Stimmungen schwinden, sowie es gelingt, die sie tragende Vorstellungsgruppe zu apperzipieren."²⁰ (Hervorhebung von mir, C.T.). Hellpach, der bei Wundt 1900 über Farbenwahrnehmung bei indirektem Sehen promoviert hatte,²¹ bezieht

sich mit diesem Satz auf die "Studien über Hysterie".²² Er übt starke Kritik an der *Methode* Breuers und Freuds, hält ihre *Hypothesen* über die Ätiologie der Hysterie aber für "denkmöglich", da "die ersten geschlechtlichen Ereignisse" am allermeisten geeignet seien, im späteren Leben sich "unvermutet über unser Inneres zu breiten".²³

Auch von seiten Freuds schien es einige Anknüpfungspunkte zu geben, speziell in bezug auf die Fehlleistungen, die Wundt auch eher als Folge von psychischen Einflüssen, als aufgrund von z.B. Kontaktwirkungen der Laute erklärt.²⁴ Freud befindet sich in dieser Beziehung in "voller Übereinstimmung mit Wundt."²⁵ Ein paar Seiten weiter zitiert er dann Wundts Begründung für die Tatsache, dass wir uns leichter verschreiben als versprechen und nennt sie "bemerkenswert".²⁶

Diese Beispiele sollen keineswegs eine Gemeinsamkeit zwischen Wundtscher und Freudscher Psychologie suggerieren, die es in Wirklichkeit nie gegeben hat. Sie sollen lediglich die Tatsache illustrieren, dass es um die Jahrhundertwende keineswegs ausgemacht war, dass die Psychoanalyse vom akademischen Wissenschaftsbetrieb ausgeschlossen würde. Es stimmt nicht, wenn Freud sagt, dass "das Schicksal der Psychoanalyse, sich in einen Gegensatz zur offiziellen Wissenschaft zu stellen", seinen Anfang mit der *Traumdeutung* nimmt.²⁷ Dieses Buch ist - im Gegensatz zur offiziellen Geschichtsschreibung der Psychoanalyse - mit Interesse und zum grossen Teil auch wohlwollend aufgenommen worden.²⁸ Es scheint tatsächlich der - vielleicht unbewusste - Wunsch nach der Herausbildung eines Mythos zu sein, der Freud und seine Schüler zwang, ihre eigene Geschichte verzerrt darzustellen: Die Einsamkeit des von der offiziellen Wissenschaft isolierten Forschers lässt ihn zum Helden werden.²⁹ Mit der Etablierung dieses Mythos scheint die Möglichkeit einer fruchtbaren Zusammenarbeit mit der akademischen Psychologie von vornherein ausgeschlossen.

Wundt wurde ebenfalls zum Mythos, weniger durch sich selbst, als vielmehr durch seine Schüler. Bei ihm ist es nicht die Einsamkeit des Forschers, sondern die Tat des Begründens, die ihm

eine Sonderstellung einträgt. Er wird gefeiert als der Mann, durch den die Psychologie zur experimentellen Wissenschaft und damit zur selbständigen Disziplin geworden ist. Danziger³⁰ hat gezeigt, dass dies ebensowenig der Wahrheit entspricht wie das Bild vom einsamen Helden Freud. Für Wundt war experimentelle Psychologie nur ein Teil des Gesamtgebietes der Psychologie neben z.B. der Klinischen Psychologie und selbstverständlich der Völkerpsychologie. Seine Nachfolger dagegen erklären die Universitätspsychologie als ausschliesslich experimentell und verbauen damit nichtexperimentellen Richtungen den Zugang.

Meines Erachtens ist es nicht nur das Wesen der Wundtschen Psychologie, das der Anerkennung der Psychoanalyse als psychologischer Wissenschaft im Wege steht. Die Gründe sind auch zu suchen in der Sonderstellung der beiden grossen Begründer, aus der zwei Traditionen des Selbstverständnisses ihrer Nachfolger erwachsen, die der tatsächlichen Rolle, die Wundt und Freud gespielt haben, nicht gerecht werden. Dieses Selbstverständnis findet seinen Niederschlag in historiographischen Darstellungen, in denen die Geschichte des Fachs weitgehend mit der Geschichte der entsprechenden Richtung zusammenfällt. Es handelt sich dabei um eine Art Widerstandsphänomen, das besonders in Krisenphasen der entsprechenden Wissenschaft auftritt.³¹

Als die Psychologiegeschichte noch nicht in dem Masse etabliert war wie heute, hat Karl Bühler eine gemeinsame "historische Ausgangslage"³² für Denkpsychologie und Psychoanalyse gesehen und hielt es für möglich, dass "Trennungsmauern" zwischen Psychoanalyse und der übrigen Psychologie fallen können.³³ Bühler kann man sicher nicht mangelnde Kenntnis der einzelnen Richtungen unterstellen. Seine Bemerkungen scheinen mir deshalb ein Indiz für die Richtigkeit der oben erörterten These zu sein: die theoretischen Differenzen zwischen akademischer Psychologie und Psychoanalyse reichen nicht aus, um die etwa 1907 beginnende divergierende Entwicklung zwischen beide Richtungen zu erklären. Von grosser Bedeutung für diesen Prozess sind ebenfalls Phänomene sozialpsychologischer Art; besonders die soziale Rolle Wundts und Freuds haben eine fruchtbare Zusammenarbeit beider Richtungen behindert.

Anmerkungen

1. Planck (1948, S. 22)
2. Kuhn (1962)
3. Eine Übersicht über diese Versuche gibt Peterson (1981)
4. Eine Ausnahme bilden die Arbeiten über den Einfluss der jüdischen Tradition auf die Psychoanalyse Freuds; vgl. dazu Roback (1929), Bakan (1958), Lowenberg (1971), Robert (1976)
5. Vgl. dazu Bernfeld (1944)
6. Wundt (1920)
7. Freud (1927)
8. Wundt (1874)
- 8^a. Freud (1950)
9. Freud (1900)
10. Vgl. dazu Tögel/Danailow (1982)
11. Vgl. ebenda
12. Vgl. Sulloway (1979)
13. Vgl. Tögel/Danailow (1982)
14. Vgl. Sulloway (1979)
15. Die Erkenntnis, dass unangenehme Gefühle zur Verzögerung des assoziativen Prozesses führen (Wundt 1911, S. 76) ist auch für die psychoanalytische Therapie von besonderer Bedeutung. In dieser Gemeinsamkeit sieht Freud sogar eine Brücke zwischen Experimentalpsychologie und Psychoanalyse (Freud 1914, S. 163)
16. Im Vorwort zur Festschrift zu Wundts 70. Geburtstag wird von seinen Schülern der Wunsch ausgesprochen, er möge "noch viele Jahre ... als unser hochverehrtes Haupt an unserer Spitze stehen und walten." (*Philosophische Studien* 19, 1902).
17. Wundt stand dem in der Freudschen Psychologie zentralen Begriff des Unbewussten recht kritisch gegenüber (Wundt 1909, S. 251), wenn er auch das "Unbewusstwerden einzelner psychischer Inhalte" (ebenda) akzeptierte. Freuds Kritik an Wundt bezieht sich weniger auf dessen Allgemeine und Individualpsychologie, als vielmehr auf die Völkerpsychologie. In einem Brief an Ferenczi vom 17. Oktober 1912 schreibt er: "Wundt makes me furious. To have to read such balderdash after eleven hours at

analysis is a hard punishment." (zitiert nach Jones 1955, p. 353)

18. Vgl. dazu Bringmann/Ungerer (1980)
19. Vgl. dazu Besser (1977, S. 140)
20. Hellpach (1902, S. 211)
21. Hellpach (1899)
22. Breuer/Freud (1895)
23. Hellpach (1902, S. 210)
24. Freud (1901, S. 74)
25. Ebenda
26. Ebenda, S. 114
27. Freud (1913, S. 295)
28. Sulloway (1979, II, 9)
29. Vgl. dazu die Grundthese von Sulloway (1979)
30. Danziger (1979)
31. Vgl. dazu Tögel (1983)
32. Bühler (1927, S. 13)
33. Ebenda, S. III

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INTRODUCTION AND DEVELOPMENT OF FREUDISM IN SPAIN (1900-1936)

M. Vicenta Mestre

Helio Carpintero

Esteban Perez-Delgado

Facultad de Psicologia

Universidad de Valencia (Spain)

Since the end of the nineteenth century, an increased interest developed in Spain to incorporate the country in the new scientific and cultural movements dominating the Western world.¹ In a first stage, the minds were oriented to become familiar with the new ideas, but gradually they became more and more interested in producing science and culture in a creative manner. This was the aim of the well-known "Generation of 1898" (Unamuno, Ganivet) and of the following one, the generation of Ortega, Maranon and Rodriguez-Lafora, who were deeply engaged in the task of modernizing Spain. As Ortega said, "We are to-day pro-French, pro-British, pro-German; dead pieces of other civilizations are brought to our body in an unconscious manner... We want the Spanish interpretation of the world".² That is, the incorporation of ideas is not enough, we must create.

Under the heading of the "Europeanization of Spain" several movements in science, literature and art have been included. All the efforts were directed to acquire a positive mentality, free of old prejudices, by getting information about the latest developments in different areas of culture. Needless to say that those efforts must be evaluated in the broad context of a society with old concerns for a religious view of human life, and hostile to new ideas and progress³.

Little by little, the Spanish intelligentsia began to keep in touch with the innovative products of European culture. In this manner, Einstein's relativity theory, surrealism, modern mathematics and new pedagogical principles, were discussed in general and technical publications. All those ideas were also propagated through a great amount of translations.

Thus, psychoanalysis appears as an innovative theory about psychology and psychopathology, with an enormous social impact. Consequently, in some aspects of culture and art, psychoanalysis brought new and characteristic accents.

Some minority groups in Spain, interested in humanities, were very early interested in the works and ideas of Sigmund Freud. As a result, those ideas progressively penetrated in our country up to the moment of the beginning of the Civil War in 1936. After that social breakdown, there was hostility and rejection of Freud's ideas under the new political regime.

We will synthetize here some factual data concerning the acceptance of Freudian theories by the Spanish intellectuals; the period covered spreads from the beginning of our century to the first year of the war. Journal articles, books, notes and book reviews have been taken into account for that purpose.

The psychoanalytic ideas can be detected through citations, translations, reviews and commentaries on Freud's works. In the present study we are going to consider especially the commentaries and presentations of Freudian theory carried out by Spanish authors. We have made, for that purpose, a quantitative study of Spanish books⁴, as well as papers published in Spanish journals, dealing with psychoanalytic themes (Appendix I).

There are a few journals which published papers about psychoanalysis. Table I presents them, with the number of papers edited along the covered period. The results clearly indicate that Freud's impact can be tracked down to four specialized fields; medicine, pedagogy, law and philosophy.

Furthermore, the greatest frequency of papers about this topic appears in medical journals (28 articles, 7 journals), which cannot be considered as a surprise. Among these journals there are two of special interest. One of them is *Archivos de Medicina, Cirugía y Especialidades*, with J. Madinaveitia as editor; in it, Enrique Fernandez-Sanz (1872-1950), one of the first expositors of psychoanalytic ideas, published several pieces of work⁵. The other one is *Archivos de Neurobiología*, which appeared in 1920 under the editorship of two psychiatrists, G. Rodriguez-Lafora (1886-1971) and

Table I. *Distribution of articles about Freud's theories in Spanish journals*

Journals	Years of publication				Total of articles	
	1900-09	1910-19	1920-29	1930-39	f	f Accumul
a) <i>Medical and psychological journals</i>						
"Archivos de Medicina, Cirugía y Especialidades"			6	1	7	7
"Archivos de Neurobiología"				9	9	16
"Los Progresos de la Clínica"		1	1		2	18
"Revista Clínica de Madrid"	1				1	19
"Revista Médica de Barcelona"			1	1	2	21
"El siglo médico"			4	1	5	26
"Trabajos de la Cátedra de Historia crítica de la Medicina"				2	2	28
b) <i>Cultural magazines</i>						
"Boletín de la Institución Libre de Enseñanza"			1		1	29
"La Lectura"		1			1	30
"Revista de Occidente"			3		3	33
c) <i>Philosophical journals</i>						
"La Ciencia Tomista"			2		2	35
"Razón y Fe"			1		1	36
d) <i>Law journals</i>						
"Revista de los Tribunales"			2	5	7	43
e) <i>Pedagogical journals</i>						
"Revista de Pedagogía"			4	3	7	50
	1	2	25	22	50	-

J.M. Sacristán (1887-1957), and the philosopher J. Ortega y Gasset (1883-1955), all of them were much involved in the cultural Europeanization of Spain. This publication was specialized in psychological, psychiatric and biological subjects⁶ and in it Angel Garma (1904-) - perhaps the first Spanish psychoanalyst - published some interesting works. There are two other journals with significant contributions to our subject: The *Revista de Pedagogía* and the *Revista de los Tribunales*. The former was founded in 1922 by Lorenzo Luzuriaga, a pedagogist closely associated with Ortega y

Gasset's project and with the "Institución Libre de Enseñanza" (Free Institution of Education). In its pages appeared several papers on psychoanalysis, part of them written by R. Lafora and Sacristán dealing with child psychopathology⁷. The latter one, dedicated to legal problems, included in its pages many articles of judge Cesar Camargo, who was deeply interested in the legal consequences of Freud's ideas.

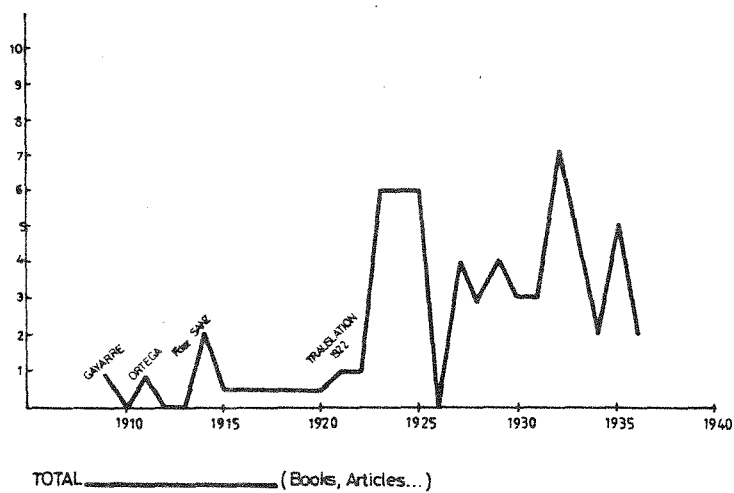
The study of books related to Freudian psychoanalysis (see Table II and Appendix I) also confirms the differential impact found in the periodical literature. The same three fields, medicine, pedagogy and law, include the vast majority of the works. From the twelve books listed here, six deal with questions related to law, five to medicine and one to pedagogy and education.

Table II *Spanish Books related with Freud's theories (1900-1936)*

Topics and Authors	Years of edition			
	1900-09	1910-19	1920-29	1929-36
a) Medicine				
Fernandez-Sanz, E.		1	1	
Juarros, C.			1	
Mira, E.				1
Garma, A.				1
b) Law				
Camargo-Marin, C.			1	2
Jiménez-de-Asúa, L.				1
Saldana, Q.			1	
Vazquez, R. and Hidalgo, M.				1
c) Pedagogy				
Jaen, J. and Peinado, J.				1
Totals	-	1	4	7 : 12

In the philosophical field, it is worth mentioning the long critical essay written by Ortega y Gasset at an early date, in 1911. Freudian psychoanalysis appeared to him as a mixture of suggestive hypotheses and a faulty logic, far removed from the true scientific theories. At the other side, in some philosophical journals published by religious institutions, namely the Dominican

Fig. 1.- TEMPORAL DISTRIBUTION OF SPANISH WORKS ON FREUD'S
PSYCHOANALYSIS. (1900 - 1936)



friars and the Jesuists, Father M. Barbado, O.P. (1884-1945), and Father E. Ugarte de Ercilla, S.I. (1865-1940) wrote several critical papers on the moral and ethical aspects of psychoanalysis.

When looking at the terms appearing in the titles of the articles (see Appendix I), we evaluated their frequency by means of a sociometric analysis of terms and determined those with a statistically significant frequency (with $p < 0.05$)⁸. The relevant terms, with at least four occurrences, are the following ones: "Psychoanalysis" (18 times), "Psychoanalytic" (8 times), "Freud" (4 times), "interpretation" (4 times) and "sexual" (4 times). These results show the great weight of the most generic terms, and suggest a dominant view of psychoanalysis as an interpretative method, basically applied to sexual questions, in the main related with the work of Freud: a very common and general view of that theory, indeed!

The temporal distribution of the publications dealing with Freud's ideas (Figure 1) shows an important increase after the appearance of the first volumes of Freud's Complete Works in a Spanish translation, in 1922. That translation had been suggested by Ortega y Gasset, and was carried out in an excellent way by Luis López-Ballesteros, a German language teacher whose work deserved the public approval of Freud himself. The translation began with *The Psychopathology of Everyday Life*, and did a lot from the point of view of the spreading of psychoanalysis in Spain.⁹

Before that date (1922), the main psychoanalytic ideas which began to be developed in Europe, had been presented here by some pioneers in a rather critical manner. Such was the case of Miguel Gayarre (1886-1936), a psychiatrist who in 1909 wrote in a critical way about the sexual genesis of hysteria. The same mood could be appreciated in Enrique Fernandez-Sanz, who in 1914 made a very complete revision of the situation of psychoanalysis to date, and in the essay written by Ortega y Gasset. All these authors were closely related to German culture. Their version of the psychoanalytic theory emphasized its succesful approach to the unconscious aspect of psychism.¹⁰

From 1915 to 1920 not much was published about psychoanalysis. The translation of Freud's works put a stop to that 'latent' period, that also coincided with the First World War. Most of the Spanish intellectuals were disappointed with German politics during those years, and one can easily imagine that the diffusion of psychoanalytic theory could be interpreted as a sort of propaganda of German culture.

As has been said, with the appearance of the first translated book of Freud's works, a great number of doctors, lawyers, pedagogists and philosophers became interested in psychoanalysis. That translation contained a foreword by Ortega y Gasset, in which the importance of the work for psychology and culture was emphasized.

Two main groups of professionals were influenced by the new theories: medical doctors and lawyers. Both groups could be considered as the professionals with the greatest social prestige. Since psychoanalysis has therapeutical applications, this was an aspect of the theory which created great expectations among therapists. The doctors were mainly interested in the effectiveness of Freudian techniques in psychotherapy. Among its criticism they included the broad and non-specific nature of these techniques and its very limited effectiveness, which perhaps was due to suggestion. Judges and lawyers, on their part, found in psychoanalysis a broad basis for legal theories about crime and responsibility of human subjects. Pedagogists were impressed with Freudian repression theory and its significance for educational matters.

When those authors are distributed according to their generational level⁹ (see Table III), a differential reaction may be clearly appreciated. It is the generation of 1876 - men born during a period of fifteen years around its center, 1876 - which produced the first reactions to the writings of Freud.

The largest group of authors belongs to the following one, the generation of 1886, that of Ortega, Rodriguez-Lafora, Juarros and others; they had arrived at social significance by the final years of that period. But, after all, only in the generation of 1901 is it possible to find the first "psychoanalysts", or psychoanaly-

tic-minded intellectuals, namely A. Garma, and at a distance, E. Mira and J. Germain. This is also the generation of the great Spanish surrealists, such as the painter Salvador Dali (1904-) and film director Luis Bunuel (1900-1983), in whose creations psychoanalytic themes were incorporated in an original manner.¹⁰ It also appears that the initial presentations were generally made through journal articles, by sensitive but non-psychoanalytic minds, and little by little, fuller expositions and more complete books began to appear.

When the Civil War began in 1936, the Freudian theories were well known in Spain. His works were read and discussed openly, and some Spanish intellectuals and thinkers tried to base some of their ideas on that monumental work. But the Civil War, and the following political regime under General Franco's dictatorship, changed substantially this state of affairs. The works of Freud became out of reach for the general reader; most of the authors we have been talking about were forced to leave the country. The psychoanalytic tradition was broken. Only when years had passed, that line of thought was reborn from its ashes.

Table III *Distribution of authors according their generational level*

Generation of: (Men born since -- till --)	1871 (1864-1878)	1886 (1879-1893)	1901 (1894-1908)	1916 (1909-192
(S. Freud: 1856 Generation of 1856)	Ugarte, E. Gayarre, M. Fernandez- Sanz, E. Saldaña, Q.	Juarros, C. Barnés, D. Camargo, C. Ortega, J. Barbado, M. Novoa, R. Rodriguez- Lafora, G. Sacristán, J.M. Villaverde, J. Jiménez-de- Asúa, L. Sanchís-Banus, J.	Mira, E. Garma, A. Gil Fagoaga, L. Moragas, J. Jaen, J. Vazquez-Zamo- ra, R. Germain, J.	Peinado,
Totals	4	11	7	1
(% of total sum)	17.4	47.8	30.4	4.4

Notes

1. See, for more details: Carpintero (1982), and Marías (1970a)
2. See Ortega (1910)
3. See, for example, Brenan (1960)
4. For this methodology, see Carpintero and Peiró (1983)
5. See for more details: Mestre and Carpintero (1983)
6. Peiró and Carpintero (1983), and Martínez-Pardo (1978)
7. See for more detail: Alfaro and Carpintero (1983)
8. For this methodology, see Carpintero and Peiró (1978)
9. The edition of Freud's works began in 1922, under the general title of Professor S. Freud's Complete Works' (Obras completas del Professor Sigmund Freud). The first volume included "The Psychopathology of Everyday life", and in the following years many volumes appeared, till vol. XVII, in 1934. The translation was done by L. López Ballesteros with great accuracy, and remained as a standard translation for many years.
10. M. Gayarre criticized Freud's ideas in different aspects. From his point of view, the freudian treatment for hysteria lacked due specificity, and there was no control of possible effects of suggestion induced by psychoanalyst on his patient; he also emphasized the lack of causal nexus between treatment and recovery, and was quite skeptical about the value of verbal catharsis as a therapeutic method.

E. Fernandez-Sanz, by its part, noted that freudian theories lacked scientific evidence and logical rigour; he also opposed its 'pansexualism', and stressed the limitations to psychoanalysis due to age and cultural level of patients in clinical practice of psychoanalysis.

J. Ortega y Gasset, the great spanish philosopher, wrote a lengthy essay on 'Psychoanalysis, the problematic science' ("Psicoanalisis, ciencia problemática"). There he criticized psychoanalytic theory on grounds of its descriptive, non explanatory character, as it failed to show the logical need for accepting their hypothetical explanations. Ortega proposed that Freudian ideas were accepted only as suggestions, or

'myths' as he called them, that could offer indications for scientific research; without refusing the great importance of sexual dimensions for understanding human life correctly, he stood against Freudian reduction of the whole behavior to sexual impulses.

Needless to say that all this criticism has to be regarded in connection with the historical moment it was done.

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APENDIX I

YEAR	AUTHOR	TITLE
1909	Gayarre, M.	"La génesis sexual del histerismo y de las neurosis en general", <u>Revista clínica de Madrid</u>
1911	Ortega y Gasset J.	"El Psicoanálisis ciencia problemática", <u>La Lectura</u>
1914	Fernandez Sanz E.	"El Psicoanálisis", <u>Los Progresos de la clínica</u>
1914	Fernandez Sanz E.	<u>Histerismo, Teoría y Clínica</u>
1921	Fernandez Sanz E.	<u>Las Psiconeurosis</u>
1922	Jimeno Riera J.	"La Histeria desde el punto de vista psicoanalítico" <u>Archivos de Medicina, Cirugía y Especialidades</u>
1922	R. Lafora G.	"Estudios psicoanalíticos sobre las obsesiones" <u>Archivos de Medicina, Cirugía y Especialidades</u>
1922	Ortega y Gasset J.	"Prólogo" a <u>Obras Completas</u> de S. Freud
1923	Fernandez Sanz E.	"La evolución del Psicoanálisis -los disidentes- el freudismo ortodoxo", <u>Archivos de Medicina, Cirugía y Especialidades</u>
1923	Fernandez Sanz E.	"Técnica de la Psiconálisis como instrumento terapéutico", <u>Siglo Médico</u>
1923	R. Lafora G.	"La Teoría y los Métodos del Psicoanálisis", <u>Los Progresos de la Clínica</u>
1923	Sacristán J.M.	"El Psicoanálisis como método de exploración del inconsciente", <u>Revista de Pedagogía</u>
1923	Sacristán J.M.	"Freud S.: Das Ich und Das Es", <u>Revista de Occidente</u>
1923	Sacristán J.M.	"La Teoría Psicoanalítica de Freud", <u>Revista de Pedagogía</u>
1924	Fernandez Sanz E.	"Observaciones polémicas sobre psicoanálisis", <u>Archivos de Medicina, Cirugía y Especialidades</u>
1924	Fernandez Sanz E.	"Psicoanálisis y Lógica", <u>Siglo Médico</u>
1924	R. Lafora G.	"La interpretación de los sueños", <u>Revista de Occidente</u>
1924	Sanchis Banús J.	"La cuestión del Psicoanálisis", <u>Archivos de Medicina, Cirugía y Especialidades</u>
1924	Villaverde J.M.	"Las últimas novedades en materia de Psicoanálisis" <u>El Siglo Médico</u>
1924	Villaverde J.M.	"Sobre el Psicoanálisis", <u>El Siglo Médico</u>
1925	Barbado M.	"Tratados, textos y orientaciones generales" <u>La ciencia tomista</u>
1925	Barnés D.	"El Psicoanálisis y la educación", <u>B.I.L.E.</u>
1925	Fernandez Sanz E.	"Sobre el concepto unitario de la libido: su interpretación biológica y social", <u>Archivos de Medicina, Cirugía y Especialidades</u>
1925	Gil Fagoaga L.	"El Psicoanálisis y su significación"
1925	Sacristán J.M.	"Freud ante sus contradictores", <u>Revista de Occidente</u>
1925	Ugarte de Ercilla E.	"La Escuela freudiana y la metapsíquica" <u>Razón y Fe</u>
1927	Barbado M.	"Psicoanálisis", <u>La ciencia tomista</u>
1927	Camargo M.	"Las teorías del profesor Freud ante la psicología experimental normal y onírica" <u>Revista de los tribunales</u>
1927	Gil Fagoaga L.	"Las interpretaciones de los sueños"

(continues)

YEAR	AUTHOR	TITLE
1930	Garma A.	"La interpretación psicoanalítica de un gesto de Santa Teresa", <u>Archivos de Neurobiología</u>
1931	Garma A.	"La transferencia afectiva en el Psicoanálisis", <u>Archivos de Neurobiología</u>
1931	R. Lafora G.	"La educación sexual", <u>Revista de Pedagogía</u>
1931	R. Lafora G.	"La impotencia masculina y la neurastenia sexual" <u>Archivos de Medicina, Cirugía y Especialidades</u>
1932	Camargo C.	<u>La esencia del Psicoanálisis</u>
1932	Camargo C.	"Sobre un asunto de actualidad", <u>Revista de los tribunales</u>
1932	Garma A.	"Consideraciones psicoanalíticas sobre la vida sexual", <u>Archivos de Neurobiología</u>
1932	Garma A.	"La higiene mental en la infancia. Consideraciones psicoanalíticas", <u>Revista de Pedagogía</u>
1932	Garma A.	"Notas sobre psicoterapia en los psicópatas esquizoides", <u>Archivos de Neurobiología</u>
1932	Juarros C.	"Prólogo" a Vazquez R. Y Hidalgo H.: <u>Lo inconsciente y el crimen</u>
1932	Peinado J. y Jaen J.	<u>Psicología Pedagógica</u> "Lo subconsciente y la educación"
1932	Vazquez R. y Hidalgo M.	<u>Lo inconsciente y el crimen</u>
1933	Camargo C.	"El enjuiciamiento, los jueces y el enjuiciamiento de los jueces", <u>Revista de los tribunales</u>
1933	Garma A.	"Consideraciones generales sobre el inconsciente en Psiquiatría". <u>Siglo Médico</u>
1933	Garma A.	"La realidad y el ello en la esquizofrenia", <u>Archivos de Neurobiología</u>
1933	Garma A.	"Los sueños de angustia en la infancia", <u>Revista de Pedagogía</u>
1933	Solis J. y Alvarez F.	"Historia del desarrollo de las ideas psicoanalíticas", <u>Trabajos de la cátedra de Historia crítica de la Medicina</u>
1934	Camargo C.	"El "Complejo de Guzmán" o la antropología parricida en la antigua legislación española", <u>Revista de los tribunales</u>
1934	Garma A.	"Crimen y castigo", <u>Archivos de Neurobiología</u>
1934	Moragues J.	"El comienzo de la sexualidad en el niño", <u>Revista Médica de Barcelona</u>
1935-37	Camargo C.	"Psicoanálisis matemático", <u>Revista de los tribunales</u>
1935	Garma A.	"Paranoia y homosexualidad", <u>Archivos de Neurobiología</u>
1935	Jimenez de Asúa L.	"Psicología profunda y Derecho penal", <u>Revista de los tribunales</u>
1935	Jimenez de Asúa L.	<u>Valor de la Psicología profunda (Psicoanálisis y Psicología individual) en ciencias penales</u>
1935	Mira E.	<u>La Psicoanalisi</u>
1935	R. Lafora G.	"Interpretaciones psicoanalíticas de los celos", <u>Archivos de Neurobiología</u>
1936	Garma A.	<u>El Psicoanálisis, la neurosis y la sociedad</u>
1936	Pietro Vidal A.	"Reseña histórica de los métodos de curación psíquica", <u>Trabajos de la cátedra de historia crítica de la Medicina</u>

WILLIAM KINGDON CLIFFORD: DARWINISM,
NON-EUCLIDEAN GEOMETRY, AND MENTAL DEVELOPMENT

Roger A. Dixon*

Max Planck Institute for
Human Development and Education
Berlin, West-Germany

Summary

Although William Kingdon Clifford has had little lasting influence in psychology, and virtually no impact on developmental psychology, his single contribution to the study of mental development foreshadows recent advances in contextual developmental psychology. This paper serves as an introduction to Clifford, his life, his natural science orientation to ethics and religion, his evolutionism, his non-Euclidian mathematics, and his approach to the study of mental development. Clifford, an avid Darwinist, attempted to apply the principle of natural selection on both the individual and social levels of analysis. With regard to mental development, Clifford emphasized both continuous change and the influence of circumstances (or contexts) on the nature of change. Clifford argued that mental development, which is conceived as proceeding from the spontaneous action of the organism, requires a principle of both activity and plasticity. In this way his model is remarkably contemporary.

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It would be less than axiomatic to assert that William Kingdon Clifford (1845-1879) has had a lasting impact on developmental psychology. Indeed, it cannot even be stated with certainty that he ever had even the most ephemeral effect on the study of psychological development, his rather formidable essay, "On Some of the Conditions of Mental Development,"¹ notwithstanding. If, over 100 years after his death, he is remembered at all in psychology² it is

* The author appreciates the helpful comments of Paul Baltes and Richard Lerner on an earlier draft of this paper.

seemingly for his short-lived contribution of mind-stuff, the postulated fundamental element of mind, the standing of which no less a personage than William James (1842-1910) saw fit to refute.³ He is, however, remembered with rather more favor in the field of mathematics, which was, after all, the subject of his professional specialization.

Nevertheless, the apparent impertinence aside, it is argued in the present paper that Clifford's contribution to the study of mental development is rather more memorable than actually remembered. At the very least, it is deserving of a certain measure of critical attention by contemporary developmental psychologists, many of whom will find several of principles not entirely discordant with their own. To this end, the present paper is designed to serve as an introduction to William Kingdon Clifford the man, the scientist, the mathematician, the inchoate developmental psychologist. It is organized in three main parts. After devoting the first section to primarily biographical information, Clifford's natural science orientation to the pervasive intellectual problems of his day - evolution, ethics, religion - and to his particular area of mastery - mathematics - is discussed. Finally, in the third section of the paper, Clifford's somewhat implicit psychology - including his treatment of the mind-body problem, mind-stuff, but with an emphasis on mental development - is presented.

I. Who was William Kingdon Clifford?

The bare facts of Clifford's life are well preserved; the themes that weave these facts of life into a portrait of a vibrant nineteenth-century English intellectual we have on the good authority of Sir Frederick Pollack.⁴ For the purpose of the present paper a few brief biographical comments will suffice.

Clifford was born on May 4, 1845, in Exeter, England, and died on the Island of Madeira, where he had gone in a final hopeless attempt to regain his health, in his 34th year, on March 3, 1879. After distinguishing himself during his elementary education in Exeter, Clifford entered King's College, London, in 1860, where he

gained distinction in mathematics, languages,⁵ modern history, classics, English literature, and (Pollack notes with no small amount of verve) gymnastics. Already recognized for his precocity in mathematics, Clifford entered Trinity College, Cambridge,⁶ at age 18. Three years later, in his oration upon winning the College declamation prize - an oration devoted to William Whewell (1794-1866), late Master of the College, and noted English scientist - Clifford foreshadowed his own functionalist thinking:

Thought is powerless except it make something outside of itself: the thought which conquers the world is not contemplative but active.⁷

Like many people with active, creative minds Clifford was eccentric and independent. Possessing a voracious intellectual curiosity, Clifford chose to eschew the restrictions inherent in the Cambridge mathematics curriculum - to untether his pursuit of knowledge - and thus, came out a Second Wrangler in the Tripos of 1867.⁸ As continues to be true today, there is less than a perfect correlation between marks in school (which involved a good deal of intellectual conformity) and potential intellectual contribution (which entails somewhat more autonomous, imaginative thinking).

In 1868, at age 23, Clifford was elected a Fellow of Trinity College, and in 1871 he was appointed Professor of Applied Mathematics at University College, London, where he served the remainder of his career. Not yet wanting to be respectable, Clifford postponed joining the Royal Society of London until 1874, and then, as if to concretize his elevation to respectability, he married Lucy Lane on April 7, 1875. Characteristically, none of these achievements diminished his abiding sense of modesty.

During this period he devoted himself exhaustively to his teaching and to his writing, sometimes working through the night on the latter and performing his duties for the former untiringly the next day. The products of his writing remain as testimony to the vividness and fluency of his tinkering; many were written only after he had delivered them extemporaneously. The products of his teaching are, of course, less concrete, but Pollack attests to

Clifford's uncanny insight and skill in the didactic art. In addition, he was an active member of several learned societies (e.g., London Mathematical Society and the Metaphysical Society), as well as less formal groups that met frequently for long discussions in his home. During this brief period of active scientific productivity he also found time to write poetry and a collection of children's fairy tales called *The Little People*.⁹

Clifford, apparently believing he was training his body to versatility, was disinclined to attend carefully to the conditions of his health. Further, perhaps he had "inherited a constitution in which nervous energy and physical strength were unequally balanced."¹⁰ In any event, symptoms of pulmonary disease appeared in the spring of 1876. Still, after spending six months in Algiers and the south of Spain he had returned to a moderate level of health. Then, after his father's death in early 1878, as well as a concomitant surge of time-pressured professional commitments, his health broke seriously, and in April he left again for the Mediterranean. In August of 1878 he returned to England for the last time, suffering a relapse the following month. Facing imminent death with, Pollack says, intrepidity,¹¹ Clifford sailed for Madeira in early 1879, where he died on March 3.

Much can be made of the brevity of life - of any person's life - and the temptation to do so for the life of Clifford is great. Macfarlane, for example, writes with a certain plaintiveness: If he had lived we might have known something.¹² In the following sections Clifford's most salient intellectual contributions, wrung at great cost, are described. Emphasis is given to that portion of his oeuvre of most relevance to the history of developmental psychology.

II. Clifford's Natural Science Perspective

In his intellectual history of the nineteenth century Mandlebaum observes:

It is generally agreed that one of the most distinctive features of nineteenth-century thought was the widespread in-

terest evinced in history. The manifestations of this interest are not only to be found in the growth and diversification of professional historical scholarship, but in the tendency to view all of reality, and all of man's achievements, in terms of the category of development.¹³

The category of development implies the method of historicism. That is, an adequate understanding of any phenomenon (biological or philosophical) requires that it be considered in terms of its position in the present situation and its role in a continuous developmental or historical process.

In general, two major fronts influenced the emergence of this historical or developmental thinking, this attempt to understand human and natural phenomena in a temporal context. On the philosophical front, this attempt involved a dynamic, dialectical theory of history, such as the idealist philosophy of G.W.F. Hegel (1770-1831) and the materialist social philosophy of Karl Marx (1818-1883). On the scientific front the interest in evolution and development was manifested in two mutually influential areas of investigation. On the one hand there were the notions of natural history informed by research in geology; of particular note in this regard was the uniformitarian method of Sir Charles Lyell (1797-1875). On the other hand, there were the varieties of a biological theory of evolution, especially those of Jean Baptiste de Lamarck (1744-1829), Charles Darwin (1809-1882), and Herbert Spencer (1820-1903).

Partly as a result of the publication of his *Origin of Species* in 1859, Darwin came to be the exemplary component of the emergence of historicism in the nineteenth century.¹⁴ Darwinism influenced psychology inasmuch as it brought the study of mental activity firmly into the realm of the natural sciences. Mind or human behavior, as much as features of human anatomy and physiology, were seen to have had evolved gradually and as a function of the environment to which the organism was adapted. The process of natural selection operated upon mental as well as physical qualities of organisms; that is, both mental and physical qualities served in

the continuing struggle for survival. Darwin's contribution to developmental psychology should not be underestimated, but it is not at issue in the present context.¹⁵ The Darwinian contribution to the thinking of William Kingdon Clifford is both germane and pervasive.

Frederick Pollack, the author of the aforementioned biographical essay on Clifford, reports that Clifford was for several years the leader of an enthusiastic group of Darwinians at Cambridge. Natural selection, according to these advocates, was to be the "master-key of the universe."¹⁶ Clifford believed that the doctrine of evolution, as applied to biological phenomena by Darwin and to other areas of human knowledge by Spencer, could be applied equally well to all natural processes. More than that, the doctrine of evolution was:

a living spring of action, a principle to be worked out, practised upon, used to win victories over nature, and to put new vigor into speculation.¹⁷

Thus Clifford applied the principle of natural selection on both the social and individual levels. His view of ethics, beliefs, and religion were informed by this principle.¹⁸ For example, he asserted that morality is less a function of individual action than of social identity of solidarity.¹⁹ That is, citizens are ethical (courageous, prudent, etc.) insofar as their actions contribute efficiently to the functioning of the community.²⁰ In this way, however, although the emphasis on individual action (rather than environmental "action") as the source of variability is thoroughly Darwinian, the ascendancy of the social self (and its dominance of the individual self) was in contrast to Darwin's own view of morality.²¹

Clifford's scientific agnosticism is well known: "it is wrong always, everywhere and for any one, to believe anything upon insufficient evidence."²² Although, as a young man, Clifford had been a High Churchman and well-schooled in Catholic theology, his later adoption of an immoderate agnosticism cost him his faith and brought him into conflict with such contemporaries as William

James, who attacked Clifford from a common (Darwinism, Peirce's logic) intellectual base, and James WARD (1843-1925), who set upon him from still another point of view.²³ From the perspective of Charles Sanders Peirce's (1839-1914) pragmatic maxim of logic, it is questionable whether religious statements (as for example, about the existence of God) may stand the pragmatic test of meaningfulness. This, Clifford recognized at a deeper (or at least more literal) level than did James in his "The Will to Believe." In contrast, and perhaps self-contradictorily, James sought to justify faith as a policy and to preserve a connection between faith and knowledge, a connection that even in his later *Pragmatism* (1907) he constructed somewhat outside the pale of Pierce's pragmatic theory of meaning.²⁴ For James (but not Clifford) truths of metaphysics and religion could be established by sentiment, rather than logic or experience. For both James and Clifford the search for truth was conducted steadfastly. However, James was more tolerant of potential errors than was Clifford, who admonished that belief is warranted only on the basis of persuasive logical or experiential evidence.

Clifford had developed his philosophy of science in such essays as "On the Aims and Instruments of Scientific Thought,"²⁵ and "The First and the Last Catastroph."²⁶ Admiring, but eschewing much of the Kantian position,²⁷ Clifford built his philosophy on two foundations: the theory of evolution and non-Euclidean geometry. It is to this latter foundation of Clifford's thought that the attention of this paper now turns.

Clifford and non-euclidean Geometry.

Clifford referred to Nikolas Ivanovitch Lobatchewsky (1793-1856), the Russian originator of non-Euclidean geometry, in exalting terms, calling him the Copernicus of Geometry.²⁸ Lobatchewsky had argued that Euclid's geometry (which was over 2.000 years old) represented neither an absolute truth nor a necessary mode of human perception.²⁹ In his paper of 1870, "On the Space-Theory of Matter," Clifford advanced the arguments of Lobatchewsky and Géorg Friedrich Bernhard Riemann (1826-1866), who had studied with Johann

Friedrich Herbart (1776-1841). As would be expected, part of his method involved appeal to experimentation. That is, the assumptions of Euclid and other ancient geometers were "practically exact", more exact, in fact, than experimentation, but exact only for the finite things within our practical reach. The sum of the angles of a gigantic triangle stretching across the curvature of space, Clifford argued, may very well not be 1800. That is, Clifford argued for a theoretical relativity in physical measurement: the ancient laws of geometry may not be valid on the curvature of space. Clifford writes:

Riemann has shown that as there are different kinds of lines and surfaces, so there are different kinds of space of three dimensions; and that we can only find out by experience to which of these kinds the space in which we live belongs.³⁰

And:

The axioms of plane geometry are true within the limits of experiment on the surface of a sheet of paper, and yet we know that the sheet is really covered with a number of small ridges and furrows, upon which (the total curvature being not zero) these axioms are not true.³¹

Clifford's work in non-Euclidean geometry (his physics) and that of Riemann, has been identified as a precursor to such developments in twentieth-century physics as Einstein's relativity theory of the gravitational field.³² Clifford's work in mathematics and physics was also influential for Karl Pearson (1857-1936). Stripping claims of absolute exactness from mathematics and physical science and yet questing for truth, relying on the method of experimentation and yet overawed by the potential for error, these typify the strands of Clifford's thought that are representative of both Darwinism and his non-Euclidean geometry. Had he lived longer the seeming contradictions might have melted into cohesiveness. His interpretation and advancement of Riemann's geometry, creative and thorough as it was, might have become powerful enough to push (rather than nudge) physical theory ahead.

In the following section another aspect of Clifford's work is addressed, his psychological theory. For the moment, it should be noted that his psychology did not stand apart from his philosophy or geometry. However, the way in which his psychology is connected to other realms of his thought - via the theory of mind-stuff, which suggests that all things, from particles to metaphysics, are manifestations of mind - is less than satisfactory.

III. Clifford on Mental Development.

Writing in 1888, in patently excessive intonations, Grant Allen expressed enthusiasm for the intellectual good that the Darwinian revolution had wrought:

There is no department of human thought or human action which evolutionism leaves exactly where it stood before the advent of the Darwinian conception. In nothing is the fact more conspicuously seen than in the immediate obsolescence (so to speak) of all the statical pre-Darwinian philosophies which ignored development, as soon as ever the new progressive evolutionary theories had fairly burst upon an astonished world.³³

As alluded to above, Clifford's view of mental development partook explicitly of the Darwinian conception, and was thus one of the earliest efforts at a categorical evolutionary-based developmental psychology. Further, as we shall see, it is a conception that clearly foreshadows recent developments in contextual developmental psychology³⁴.

The 23-year-old Clifford began his discourse on mental development³⁵ by establishing the pervasive fact of flux, of mental change, that throughout the day one does nothing but change one's mind. This change, however, is in some way superficial, for it is not tantamount to development and does not necessarily bespeak parallel change in underlying "character" or "general attitude". According to Clifford the mind can be known only in reference to

its changes; and "character," which is the law of those changes, is also subject to constant change, albeit much more slowly.

Circumstances (or contexts) leave their mark on the definition and, thus, the trajectory of character change.

Do you not speak of the character of a child growing into that of a man: of a man in new circumstances being quite a different person from what he was before?³⁶

Because every circumstance leaves some "mark" (however infinitesimal) on the mind, character represents both the "sum" of these marks and the life history of the individual. On the strength of the analogy of planetary notion, Clifford argues that:

the character which will *roughly* represent the law of a man's actions for some considerable time, will not *accurately* represent that law for two seconds together. No action can take place in accordance with the character without modifying the character itself; just as no notion of a planet could take place along its orbit without a simultaneous change in the orbit itself.³⁷

Citing the historical observation of national resemblances ("general types", "family likenesses") as a function of the "Spirit of the Age," Clifford argues for multi-level change: individual minds change in the context of a changing culture. However, multi-directionality of individual trajectories is maintained, with every variation from the average character (Spirit of the Age) pursued. Again arguing by analogy, Clifford proposes that, just as with organic change, there are three kinds of change occurring in individual minds:

- a) change in the form of *growth*, e.g., acquisition of new knowledge, remembering and forgetting;
- b) change of *structure*, e.g., changes in the aggregation, shape and arrangements of the "parts" of mind;
- and c) change of *function*, e.g., the acquisition of new faculties through the growth of the child or through practice in the adult.

The issue of developmental assessment or measurement of mental improvement was addressed by Clifford along the two dimensions of differentiation vs. integration and organism vs. environment relations. The poles of the former dimension (differentiation and integration) are what make up the processes that result in development. To assess improvement Clifford proposes an intra-species (and intra-individual, or longitudinal) method, with six subsequent, temporally-related criteria:

The parts of the organism get more different.

The parts of the organism get more connected.

The organism gets more different from the environment.

The organism gets more connected with the environment.

The organism gets more different from other individuals.

The organism gets more connected with other individuals.³⁸

Thus through differentiation and integration is a mental concept formed, the "positive thing made up of contradictory negations".³⁹

Like the modern contextual approach to developmental psychology, activity is fundamental to - Clifford says the "first condition of" - development. Mental development:

is a process of simultaneous differentiation and integration which goes on in the parts of consciousness, between the mind and external things, between the mind and other minds.⁴⁰

Development - proceeding from the spontaneous action of the organism (activities) rather than by the direct action of the environment (passivities) - requires two conditions for its continued progress or upward extension, one positive and one negative. The positive condition is that the mind should be active, creative, and inventive rather than assimilative and acquisitive. Hailing the value of pure research or inquiry, Clifford continues to argue that the thing the mind creates need not be immediately useful. The negative condition is plasticity, the avoidance of (a) permanent ideas ("except such as lead to action"), (b) stagnation, (c) biases, (d) crystallization, and (e) being fixed in opinion and mode of thought. Plasticity - or "an attitude of absolute receptivity; admitting all, being modified by all, but permanently biased

by none"⁴¹ - is an essential characteristic of mental life; it embodies the power of adaptation to context.

IV. Conclusions

Although William Kingdon Clifford's life was brief the frenetic pace at which he worked resulted in an unusually wide range of scientific output of an impressively forward-looking nature. In particular, in spite of his ill-begotten concept of mind-stuff, Clifford's early effort at applying Darwinian methods to the study of mental development can be seen as a forerunner to contemporary theoretical developments. His omission from historical accounts of developmental psychology is, if not a serious oversight, then at least worthy of rectification.

Notes

- 1 This paper was delivered in 1868 at the Royal Institution, London, and published in 1885.
- 2 Most of the influential histories of western psychology do not even cite Clifford, e.g., Boring (1950), Flugel (1933), Hearst (1979), Misiak & Sexton (1966), Murphy (1929), Wolman (1968).
- 3 See James (1890), especially Chapter Six; see also Royce (1881) and Wittaker (1881).
- 4 See Frederick Pollack's biographical contribution (originally written in 1879) to William Kingdon Clifford (1918). See also Alexander MacFarlane (1916). The following biographical notes have consulted these sources. Thus, because MacFarlane drew heavily from Pollack, their veridicality rests primarily on Pollack's account.
- 5 During his lifetime Clifford mastered French, German, Greek and Spanish, was competent in Arabic, and entertained an interest in Sanskrit and hieroglyphics.
- 6 The college which had also trained other eminent British mathematicians such as Arthur Cayley (1821-1895) and Augustus De Morgan (1806-1871); see MacFarlane (1916).

- 7 Quoted in Pollack (1918, p. 9).
- 8 Not to worry: Other mathematics Second Wranglers of the period were such luminaries as the aforementioned Whewell, James Clerk Maxwell (1831-1879), and Sir William Thomson (Lord Kelvin) (1824-1907).
- 9 This was a diversion not completely unlike that of his contemporary mathematician, Charles Lutwidge Dodgson (1832-1898), who is better known as Lewis Carroll. Pollack writes that Clifford was quite fascinated with children and would do virtually anything to entertain them: "A children's party was one of Clifford's greatest pleasures" (p. 12).
- 10 MacFarlane (1916, p. 81).
- 11 Pollack (1916, p. 15) observed that Clifford fulfilled Spinoza's view of liberty: *Homo liber de nulla re minus quam de morte cogitat* (A Free human thinks about nothing less than death).
- 12 MacFarlane (1916, p. 82), as quoted from one of Clifford's own volumes.
- 13 Mandlebaum (1971, p. 41).
- 14 Of course, Darwin was clearly not without intellectual antecedents; see, e.g., Glass, Temkin, & Straus (1968), Manier (1978), Ruse (1979), Toulmin & Goodfield (1965); see also Darwin (1958).
- 15 See, e.g., Darwin (1872, 1877); see also Dixon & Lerner (in press).
- 16 Pollack (1916, p. 19).
- 17 Pollack (1916, p. 19). Pollack notes that Spencer's broad application of the concept of evolution to all natural phenomena was similar to Clifford's views.
- 18 See Clifford (1918), especially "Right and Wrong: The Scientific Ground of their Distinction", "The Ethics of Belief", "The Ethics of Religion".
- 19 His delineation of morality is, of course, more specific than this, involving an appeal to piety, rather than altruism, as the basis of morality. As Mandlebaum (1971) notes, it is not

entirely consonant with Darwin's view of morality. Nevertheless, Mandlebaum argues, it was compatible with a prevailing Darwinistic perspective, viz., that the form of an individual's action should be evaluated in terms of its contribution to social survival.

- 20 See Clifford's "The Scientific Basis of Ethics."
- 21 Darwin (1871); Mandlebaum (1971); Richard (1982).
- 22 Clifford, "The Ethics of Belief". In this he was allied with his contemporary Thomas H. Huxley (1825-1895).
- 23 See James (1948), especially "The Will to Believe"; Ward (1911). Ward (p. 413) argues (using James): "One tale is good till another is told: so all this antagonism is true and trite so far as logic goes. But there is more to life than logic, and it is just its primary factor that agnostics overlook".
- 24 Peirce, visiting England in 1875, had met Clifford at Cambridge and at meetings of the Royal society. Citing Fisch, "A Chronicle of Pragmatism", H.S. Thayer (1968), reports that after this meeting, Clifford supposedly said that "Peirce was the greatest living logician, the second man (the other being Boole) since Aristotle to add significantly to the subject" (p. 70).
- 25 Delivered in 1872 in Brighton to the British Association.
- 26 Delivered in 1873 as a response to a discourse by James Clerk Maxwell given in Bradford to the British Association.
- 27 Macfarlane (1961).
- 28 Bell (1937); Boyer (1968) describes a line of influence on Clifford deriving from C.S. Peirce and George Boole (1815-1864). Peirce agreed with his father, Benjamin Peirce (1809-1880), that mathematics is the science of necessary connections, but argued that whereas mathematics is hypothetical and conditional, logic is categorical.
- 29 Bell (1937).
- 30 Clifford, "On the Space-Theory of Matter."
- 31 Clifford (1968), which is a reprint of the first edition of 1882.

- 32 Bell (1951, pp. 203ff.). In another context the same author, Eric Temple Bell (1945) writes that, although Clifford's speculations were bold and accurate, they were generalized anticipations rather than directly programmatic ones.
- 33 From Grant Allen's *English Worthies* (1888), as quoted, with qualification, in Merz (1965, pp. 607-608).
- 34 See, e.g. Dixon & Nesselroade (1983), Lerner (in press), Lerner, Hultsch & Dixon (in press).
- 35 Clifford (1885).
- 36 Clifford (1885, p. 2).
- 37 Clifford (1885, p. 3); emphasis in original.
- 38 Clifford (1885, p. 9).
- 39 Clifford (1885, p. 11), a rather "dialectical" portrayal.
- 40 Clifford (1885, p. 12).
- 41 Clifford (1885, p. 14).

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