

NEW STUDIES IN THE HISTORY OF PSYCHOLOGY AND THE SOCIAL SCIENCES

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Preface

Cheiron Europe was created in order to stress personal and scientific exchanges among people working in the history of the social sciences in European countries. Since its very beginnings, Eastern and Western European scholars found here a place to communicate, discuss, and above all meet and become personally involved in the task of strengthening European cultural links. While working from a plurality of theoretical points of view, they are building a new common spirit through personal friendship and participation in the same intellectual project.

The papers here presented were read at the 10th CHEIRON EUROPE Conference, held in Madrid (Spain) in the early days of September, 1991.

Two Spanish Universities, the Universidad Complutense de Madrid and the Universidad Nacional de Educacion a Distancia supported the conference. An important grant was also provided by the Comisión Interministerial para la Ciencia y la Tecnología (CICYT) of the Spanish Department of Education and Science, thanks to which these Proceedings are now being published.

It has taken CHEIRON EUROPE ten years to come to Spain. They were years of work and effort to promote historical reflection, but also years of enormous changes in our world that will likely give new support to Cheiron spirit.

Some 70 scholars came to Madrid from as many as 14 different countries. Particularly worth noting was the presence of researchers from Eastern Europe, including Hungary, Poland, Yugoslavia, Bulgaria and Romania. The high number of Spanish participants, on the other hand, clearly showed the great development reached by historiographical research in psychology in this country.

A wide variety of subjects was discussed. They may be grouped under the following general headings: 1) Issues in Psychobiology; 2) Historical Approaches to Social Problems; 3) Psychology in its Classical Period; 4) Historical Approaches through Psychological Journals; 5) Historiographical Issues; 6) History of Psychoanalysis; 7) National Trends in Social Sciences; 8) Skinner - A Reappraisal; and 9) Historical Approaches to Cognitive Issues.

Ten annual meetings have already taken place since the Society's foundation. If we are to judge from the vitality and quality of the papers here included, many more are still to come.

Madrid, July 1st, 1992

The Editors,

Psychohistory

The second front of scientific psychology

José Luis Pinillos

Universidad Complutense de Madrid (Spain)

Mr. Chairman, ladies and gentlemen!

It is really a great honor to have the opportunity to talk on this occasion. As you know, the topic I am going to talk about is psychohistory and its relations with psychology. To begin with, let me admit that psychohistory is not a science in the same way psychology is, or pretends to be. In my opinion, psychohistory represents an attempt to open a second front in a psychology which seems to be more appropriate for dealing with natural events than with the historical activities of man, so that the term 'scientific' cannot be applied to psychohistory in the same sense as it is applied to a psychology which claims to be a natural science. Having said this, it would be superfluous to add that most psychologists are reluctant to consider psychohistory as a serious discipline. This I can easily understand; but nevertheless reluctance is not a very impressive argument. Phrenology, for instance, was rejected, but so was *The Origin of Species*. Of course, time will decide if the case of psychohistory is going to be more similar to phrenology than to the theory of evolution, but meanwhile I would be inclined to give psychohistory the benefit of the doubt.

As all of you know very well, at the end of the last century, the epistemological *status* of the "new" experimental psychology was at stake.

The opinions of men like Wilhelm Wundt, William James, Wilhelm Dilthey, Franz Brentano, Hermann Ebbinghaus, Edmund Husserl, and many others, differed considerably among each other in regard to very important theoretical and methodological issues. All these discrepancies amounted finally to the famous *Methodenstreit*, where, among other things, the *pros* and *contras* of the application of scientific method to the study of psychology were discussed at large. On the one hand, the so-called tough minded psychologists, of which Ebbinghaus was perhaps the best example, did think that the scientific method should be identical for every science, because after all method was not affected by its contents. On the other hand, however, the more soft minded people - Dilthey, for instance - did believe just the other way around because, in their opinion, contents could be severely affected by method. According to this last view, scientific method should be adapted to its object, specially if the object could also be a subject, as was the case in human psychology. Summarizing, Ebbinghaus puts method in the forefront of science, whereas Dilthey

puts the object, i.e., the subjective object. At heart, the question was if method could be the same in the *Naturwissenschaften* (natural sciences) as in the *Geisteswissenschaften* (moral or cultural sciences).

Now then, if we take account of the fact that psychology deals not only with behavior, but with the ways organisms exist in their respective environments, we cannot ignore that these ways differ considerably in animals and men. The differences are so deep as to justify perhaps an epistemological shift. Animals live in very restricted "milieus", and their behavior is regulated basically by instincts. Men are 'ecumenic' beings, capable of coping in any place in the world; moreover, their actions are regulated to a certain extent by projects, not by instincts. Animals are natural beings reacting specifically in a similar manner to similar stimuli; for instance, the environment and the behavior of ants have not changed since almost four hundred million years. Still, men are historical beings in a continuous state of change: they continuously create new "worlds" which, in turn, modify their own psychological condition. Human beings live in a historical time; animals do not. Animals communicate; men write books on psychology. In short, it seems that Dilthey and his followers had some reasons to suspect that psychology, considered as a natural science, could be a sort of procustean bed, where human mind would be at the end distorted, when not mutilated or wholly ignored.

As early as the 17th century, Descartes was already afraid of this possibility. In his opinion, science should apply only to automatic movements of the body, but not to the operations of a spiritual entity as the mind; incidentally, that was the reason why he rejected the attempts of Gassendi to conceive the *cogito* in atomistic terms. However, this was what happened finally, when men like Hume or Condillac initiated association psychology. In a sense, this was only natural. At the time, Newtonian physics, the toughest of all natural sciences, was trying to efface any trace of mental concepts from its area of study. Inner experience, secondary qualities, final causes and value judgments were declared *personae non gratae*, and so, were exiled. Classical mechanics was entitled to deal only with the so called "objective representation" of reality: an experience of the world viewed exclusively from the perspective of calculable cause-effect relations of objects defined in terms of primary qualities, i. e., in terms of qualities which could be observed publicly and measured by different experimenters, and whose existence could not depend on being known by any subject. On the contrary, subjective qualities are called secondary because they exclusively have an intentional or mental existence in the mind of a subject, that is, they only exist in the act of thinking. For example, a fantasy of mine "exists" only for me, in my imagination and during the period of time I am imagining it. The trouble was, of course, that if the subjective qualities existed only in the private experience of individuals, then of the pretended science of mind could be said the same as the Romans used to say of one of their Popes:

Pio, per conservare la fede, perde la sede,

Pio, per conservare la sede, perde la fede.

In short, under the influence of modern science, the “mental” had to be segregated from the “physical”, in contrast with the physics of Aristotle, where mind and body belonged together, to the same “*physis*”. From the 17th century on, the mental and the physical belonged to two separate worlds. As you know, René Descartes divided the human being into two “things”, the mind and the body (*res cogitans* and *res extensa*), hypothetically united by the pineal gland, or by God. The object of the new physics was the world of extension, the world of the primary qualities and their causal relations: this was the “objective” world. The remainder -thinking, desiring, feeling, imagining and so on - formed the “subjective” world, of which no science was possible. At this point, once man was divided into two heterogeneous substances, the ways which remained open to psychology were basically three:

- i) To reduce itself to a philosophy of the mind (spiritualism, phenomenology, comprehensive psychology, etc.)
- ii) To apply scientific method to the study of mind (association psychology, association of ideas, mental philosophy, and so on)
- iii) Or to transform psychology into a science of behavior and its biological foundations (behaviorism, S-R association, reflexology, physiological psychology, and so forth)

Obviously, according to the criteria of objectivity and exactness handled by classical physics, this discipline could only deal with the automatic movements of the human body -reflexes-, but not with the immaterial operations of the mind: a mental science was necessarily a *contradictio in terminis*. Paradoxically, in order to be a mental science, psychology had to get rid of the mind, it had to de-mentiate itself. To be scientific, psychology either should operate exclusively with intersubjective observable phenomena, or should implement a special method to deal with subjectivity. This alternative was, of course, the road taken by the “mental philosophy” of the 18th century, by introspective association psychology. Nevertheless, the contradictions of this mental philosophy were so deep that finally it broke down. This represented the crisis of introspective psychology and the forthcoming of behaviorism, that is to say, association of ideas was substituted by association of stimuli and responses.

Anyway, what this story shows is that as a consequence of a metaphysical problem - dualism -, psychology split off into two branches: either it developed as an explicative science of behavior and its physiological foundations, or it took the way of a philosophy of mind. As I have already mentioned, association psychology was a compromise; it was supposed to be a physical science of the mental, but it ended in an “impasse”. Christian Wolff’s separation of *Psychologia empirica* and *Psychologia rationalis*, Dilthey’s distinction between *erklärende-* and *verstehende Psychologie*, and Husserl’s attempts to differentiate his *Phenomenologische Psychologie* from Wundt’s *Physiologische Psychologie* are but a few examples of the great impact which a metaphysical problem, a philosophical speculation - Cartesian dualism - did have and is still having on psychology.

Nowadays, it is a common place of the history of psychology that this battle came to an end with the triumph of the left wing of the 'party'. Among other things, because natural science was the only real science which existed at the time; then, if psychology wanted to become a science, it had to be the hard way. The decision was, of course, a sensible one; thanks to it, psychology exists today as an accepted social fact. But nonetheless, this practical success didn't mean that the basic theoretical problems had been properly solved. Actually, they were not. At the end, most of the important issues remained unsolved, undecided, and still do. One could say that the debate left an overdue account as a legacy.

Honestly, it must be said that the problem of mind hadn't any possible solution within the framework of classical mechanics. Only now, the possibilities of a solution begin to increase insofar as a modern concept of science has dispensed with many of the epistemological restrictions of Newtonian physics. Quantum mechanics, for instance, has softened Cartesian dualism, in considering that waves and particles are two functions of one and the same matter. Furthermore, Bell's theorem, the principle of no locality, the particles without mass, the implicated order of David Bohm, the Clauser-Freedman experiments and many other quantum phenomena strongly suggest that the fundamental processes of nature might operate outside the space-time continuum, an alternative which bestows a new scientific aura of rationality on the formerly exiled concept of mind.

As I have already said, an interesting way of exploring the possibilities opened by this new approach to a science of mind could be psychohistory, together with other neighbouring disciplines, like psychology of culture or the German *Historische Psychologie*¹. Certainly, I do not want to press the point, because for the moment psychohistory is more the promise of a science than a real science. Harry Lawton, for instance, has pointed out² that there is still "not a uniformly accepted definition for psychohistory", and in his well known critical bibliography on psychohistory William Gilmore has shown³ that the literature on this issue is indeed as copious as it is heterogeneous.

Nevertheless, in spite of its immaturity, or perhaps because of it, there is already enough agreement on central issues as to allow some speculation on the possibility that psychohistory could initiate a shift of paradigm in the field of psychological studies. In *The Psychohistorian's Handbook*, Lawton defines psychohistory as "the interdisciplinary study of why man has acted as he has in history, prominently utilizing psychoanalytic principles". Lloyd de Mause, in *Foundations of Psychohistory*,⁴ also believes that the task of psychohistory consists of tracing historical motives to their psychoanalytic sources. Essentially, this was also what Freud thought himself and what most people believe. Of course, psychohistory is nearer to psychoanalysis and its repressed conflicts than to standard scientific psychology. Really, this truism doesn't cast much light on the problem, but it helps to continue our lecture.

For many years, since its foundation by Freud at the beginning of the century, psychohistory was associated with a psychoanalysis where the *id* and biology had a

tremendous weight. According to this approach, psychohistory was mainly considered as a sort of applied psychoanalysis trying to see how deep unconscious motivations impinged on historical actions. In accordance with this premise, it happened that the energies of *id* were so powerful that, in spite of moral, scientific and aesthetic education, notwithstanding the high ideals of the Enlightenment, the pulsions of man always overwhelmed his reason, meaning that culture and history would fail once and again in their attempts to repress instinct. As Freud used to say, the repressed always return.

Gradually, this fatalistic perspective, which had its explicative *momentum* in deterministic biology, has been replaced by another more optimistic view, based on a psychoanalysis where *ego* and society have more weight than before. The influence of the Frankfurt School, and the democratic reception that America offered psychoanalysis in the thirties, succeeded in modifying the former deterministic interpretation, emphasizing the influence of history and personal biographies on the motives and phantasies of man. In the sixties, this is what Erik Erikson started doing. Under the influence of Anna Freud, whose lectures at the Maudsley I remember well, Erikson leaned on a cultural psychoanalysis of the *ego* where history played an important role in determining the stages of psychological development of the individual. To some extent, something similar is what Weinstein and Platt did try later,⁵ placing themselves in the point of view of a psychoanalytic sociology, where culture would be able to give sense to the instincts of man instead of limiting itself to repress them. Considered from this new perspective, the power of instincts to detain the progress of history was minimized through the personal mediation of the biographies of "great" leaders like Gandhi, Luther, Hitler or Stalin, and through the phantasies of historically conditioned groups.

Briefly, at the beginning of the present century, when innatism and heredity were almost almighty, psychohistory overemphasized the impact of instinct in history. Nowadays when environment leads the way in the social sciences, the influence of history on instinct is increasingly acknowledged. Instead of seeing culture as a mere instrument of repression of human pulsions, it is considered as a possibility of its redemption; there is now a tendency in psychoanalysis to consider culture as being capable of "historifying" instincts. However, behind this shift of perspective lies a series of crucial questions which deserve some comment. In a way, these questions are like the conditions of possibility of psychohistory, and their comment might help us to exemplify the type of problems we have in mind when talking of a shift of paradigm in psychology.

Of course, the first of these problems refers to the influence of general culture on science. As Stephen G. Brush indicated a few years ago, in opposition to the old-fashioned theory of superstructures, "no one would deny that the development of modern science has been a major factor in the recent history of civilization, *yet the relation between scientific theories and general culture is rarely given serious consideration*"⁶.

However, as Ilya Prigogine has pointed out in a conversation with Renée Weber,⁷ there is already clear evidence showing that science is not so much an autonomous activity as was usually thought, but is deeply rooted in social history, deeply contextuated by culture. "Western science - said this Nobel Prize winner in chemistry - originated at a time of absolute monarchies. The idea was that the monarch, like God and like the scientist, has an eternal wisdom, and therefore truth has to be eternal, unchanging. The universe had to satisfy eternal laws. What could be the meaning of uncertainty in the spirit of a scientist who is in a sense a representative of some superior knowledge?"

This fact is extremely relevant for our problem. In dealing with the historiographic problem of mental measurement, Paul Voestermans has argued not only that "psychological practice is the dimension of psychology that has a strong influence on the modernization of practices in the field of mental health, education, family life, and so on", but he has also stressed that "to give a historical account of this practical side of psychology is quite a hassle, since we all know that as a practical science psychology is quite vulnerable to ideological influences"⁸.

The question, I insist, is extremely important for the issue we are dealing with. If we think it over for a moment, we will realize that *de facto* - although without naming it - Bruce and Prigogine are talking about the influence of the *Zeitgeist* on science. Actually, we could substitute this term for others like world image, *Weltanschauung*, *Weltbild*, ideology, etc., but this change of name will not help much, as anybody can easily gather. The real question is that here we meet not only a conventional interdisciplinary problem, but a philosophical issue which surpasses by far the current metadiscourse of psychology. This question consists nothing less than of an expansion of the postulates of positive science, in order to open them to a dialogue with the humanities and with metaphysical ideas which do not have any empirical referent.

To acknowledge that culture has an effective influence on science, especially in the field of the human and social sciences, implies a rupture with the psychological paradigm actually in course. First of all, because this paradigm cannot recognize the influence of theory or ideas on observation, which nonetheless is what really happens in the case we are considering. As everybody knows, it is a well established psychological fact that human perception - and not only perception - is influenced by motivation, expectations, feelings, ideas and so on. Only that this influence is exerted in a special way that is foreign to natural sciences. For instance, if we believe that a comet is going to collide with the earth, our feelings and imaginations do not affect at all the course of the comet, but certainly they do affect our projects, our feelings and perhaps our individual and collective behavior. All this sounds perhaps like a truism, but I would add that it is a neglected truism, which notwithstanding makes a difference, a very important difference between the sciences of nature and the sciences of man. The problem does not lie exactly in the fact that mental anticipation of events affect our behavior and our thinking; this could happen also to higher animals. The question is that, in man, mental anticipations are being enhanced by language and conditioned by

culture. It obviously points directly to the historical condition of the human mind⁹, to the *Geschichtlichkeit des Seelischen*¹⁰, to the *Genèse [historique] de la conscience moderne*¹¹ or to the *histoire des mentalités*¹². As Richard Schweder has written recently, "cultural psychology is the study of the way cultural traditions and social practices regulate, express, and transform the human psyche..."¹³. Actually, in order to deal with these questions, psychohistory is forced to go beyond the old theoretical approaches and break the former epistemological frontiers of psychology. This proposal is not to be read as a disqualification of current scientific psychology, but should be understood as a necessary enlargement of its epistemological focus. Psychohistory has to study not only the impact of biological motivation in history, but also the influence of historical motives on mind: an issue which is definitely out of reach of a psychology within the classical frame of natural science, and which deals with problems that touch metaphysical frontiers. The admission of the influence of general culture on science implies also the influence of philosophy and, therefore, the rejection of scientism. Actually, the psychohistorical option leads to the repulse of the epistemological supremacy of natural science; it implies the rejection of scientism and casts serious doubts on the classical notion of objectivity, which incidentally was the main support of the supremacy of scientism. Moreover, in admitting the influence of *Weltanschauung* on science, psychohistory opens the door to relativism. Obviously it prefers the context of discovery to that of justification, and gives scientific validity to historical reason as opposed to the universal reason of the Enlightenment.

Now then, the fact that *Zeitgeist* has been acknowledged as a source of influence in science reminds me of the Spanish philosopher Ortega y Gasset, who once said that ideas are true if they agree with our image of life. But as our image of life is to a great extent determined by *Zeitgeist* in a subtle manner very difficult to grasp, this means that the influence of such an image might lead scientist to awkward decisions. Without a pressure of this sort, it would not be easy to understand that, in order to develop a science of mind, mental philosophers imitated a science which had no epistemological place for mental phenomena.

Episodes like this can be understood only in terms of subconscious beliefs - not only unconscious drives -, where complex historical and philosophical questions intermingle with deep repressed motivations: an issue which exceeds psychology. For instance, for more than three centuries it has been taken for granted that causation could operate upwards, from brain to mind, but not downwards, from mind to brain. But obviously this was a sort of historical dogma - not properly a theory -, which nowadays looks like an assumption of old materialism, which has lost its capacity of conviction. As Karl Popper has pointed out, if it was difficult to understand how ideas might influence brain processes, no less difficult was it to explain how a physical force can produce or "touch" an idea, etc., etc. As you know, there are many other examples which could be brought forward in support of my thesis, but scarcity of time will oblige me to reduce my references to a couple of convincing cases, I hope. First of all, a historical belief - and nothing more - is all that associates scientific method with

mechanism. As a matter of fact, the association of the hypothetical-deductive method with mechanism is an impressive example of an epistemological mistake which psychohistory, unlike conventional psychology, is not obliged to accept. This association of scientific method with a certain type of theory - mechanism instead of finalism - is a historical fact very fortunate in regard to classical physics, but is not an indissoluble marriage. Formally, the hypo-deductive method is as compatible with mechanism as with finalism. The essential condition of a scientific theory is that their hypothesis generate relevant predictions, capable of being compared with empirical facts by means of what Northrop called *epistemic correlations*. There are, of course, other conditions, but the accomplishment of the mentioned one entitled a discipline to operate scientifically. The nature of the theory is somehow indifferent insofar the predictions are apt to be 'confirmed' or, better, disconfirmed by observed facts. And that is a thing psychohistorians can do, at least up to a moderate degree of accuracy. Heidegger has insisted in Dilthey's view: the *Geisteswissenschaften* cannot be exact. Really, when one is dealing with history - or with psychohistory - scientific accuracy is not exactness; in the "moral" sciences, calculation is not necessarily measurement.

At the end, it is resistance to change, rather than rationality of choice, what explains the inability of many psychologists to explore a paradigm which offer more possibilities to human psychology than the old one. The problem here is that the resistance I am referring to is not a psychological one coming from defence mechanisms. This kind of resistance operates *too*, but is not the only one. One of the most pervasive and insidious mechanisms of delusion which operates inside human mind is the "transcendental illusion", so impressively analyzed by Kant in his *Critique of pure reason*. The trouble is that scientific psychology and human sciences are committed to a positivistic notion of science, which operates mainly with empirical concepts at the level of categories of the intellect, leaving aside feeling and imagination and disregarding also those which Kant called ideas of reason, i. e., those concepts which, although having themselves no observable referents, regulate however the rational use of observation and empirical concepts. As Kant has pointed out, these ideas without any adequate object of observation, without any direct referent at all, function as a rule or principle according to which we try to organize a whole body of empirical observations; but they in turn cannot be objects of experience. These ideas can only regulate experience, they do not make it possible; they help us in thinking and organizing a body of systematic knowledge, but they do not convey any specific knowledge. To believe that psychology could attain a scientific knowledge of mind was, indeed, the deadly blow, the unsurmountable paralogism of association psychology, the "rational", not the "categorical" mistake of the *mental philosophy*.

Obviously, to these kind of problems current scientific psychology is blind. Because of this lack of philosophical sophistication, most psychologists have taken for granted that the idea of finality has no use in science and that, consequently, psychological problems have to be explained mechanically and nothing else. Only that according to such an assumption, psychohistory should be regulated by the principle

of mechanism and would only pay attention to those features which were coherent with it. In other words, under these circumstances, psychohistory would consider exclusively those features that a mechanical definition of objectivity had previously postulated and therefore psychohistorians would be forced to accept causality as the only real force actuating in history: for example, that sex determinates culture and not vice versa, and so on and so forth.

Another unorthodox question that psychohistorians have to deal with is the demarcation of what is properly human. Some animals can almost become human, but at the end animals cannot stop being animals. However, man might become inhuman. Now this means that, in order to demarcate its territory, psychohistory needs a criterion to differentiate human from inhuman actions. And the question is that this criterion cannot be empirical, because empirically all man's actions are human. In this sense, if psychohistory wants to differentiate Eichmann behavior from Buddha's or Jesus Christ's, it is in need of an apriori moral concept, as Lloyd de Mause has openly acknowledged. And this certainly goes beyond and against orthodox practice in scientific psychology.

There are many other problems and situations that could have been analyzed. But those commented up to now convey the idea, I hope, that conventional scientific psychology still remains to a great extent in the orbit of classical mechanics, which keeps psychology away from a genuine study of human action, from historical experience and behavior. As Richard Schweder has said regarding cultural psychology, the mind "is content driven, domain specific, and constructively stimulus bound; and it cannot be extricated from the historical variable and cross-culturally diverse intentional worlds in which it plays a coconstituting parts"¹⁴.

Ladies and gentlemen, to come to an end let me say that, in my opinion, Schweder's statements are coherent with the present scientific outlook. Nowadays, when so many basic concepts of hard sciences are being revised, the attachment to the old-fashioned paradigm looks slightly surprising. If only for the sake of shaking the restrictive prescriptions of classical psychology, psychohistory should be looked at quite sympathetically. Except that there are perhaps better reasons than mere sympathy to promote psychohistory. This is at least what I have meant to show tonight. Thank you!

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Three Portraits of Social Psychology in Europe

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It is a great honour for me to participate in the closing academic session of this conference. I want to thank, first of all, my friends and colleagues professors Carpintero, Ferrándiz and Lafuente, organizers of this conference, for having entrusted me with the responsibility of this last lecture.

I have chosen the topic for two reasons. To begin with, the way Social Psychology is portrayed has deep consequences for the identity of people who do research in Social Psychology. They need to know who they are. And who they are depends, to a great extent, on how they are perceived by significant others.

The second reason concerns us here in a more direct way. The fact that Social Psychology in Europe is portrayed in three different ways creates problems for Social Psychology. I think that historical research can help Social Psychology in solving these problems and achieving a less confused identity than it has today.

An American Portrait

In 1932 Fay Karpf published an important book with the following title: *American Social Psychology*. It had a subtitle: its origins, development, and European background. Karpf's book was written in a scholarly manner. It carefully documented the state of American Social Psychology in the thirties and considered at great length what she called its "European background". Well-known 19th century European sociologists like Comte, Tarde, Simmel and Spencer, as well as less known ones, like Ratzenhoffer and Gumplowicz, are given prominent places in her book, alongside with philosophers like Hegel.

Karpf took great pains to show that the original ideas of European thinkers had been adopted by American social psychologists, who transformed them and gave them empirical content. The message of the book was crystal-clear: let the background be European. But Social Psychology, in a strict sense, is an all-American development.

Karpf's book has not been very popular among social psychologists. It is seldom quoted, even in specialized literature. Yet, her message has been very influential. European Social Psychology is portrayed as a past, non-empirical, mainly rational enterprise, as a project whose fruits matured in American soil. This is the prevalent conception in mainstream Social Psychology, as illustrated by the major publication in the field: G.W. Allport's "Historical Background of Modern Social Psychology" (1968), as well as by some other minor works (Hendrick, 1977; Jones, 1985).

A European Portrait

But Social Psychology did exist in Europe by the time G.W. Allport published his controversial paper. In fact, in 1963, John Lanzetta, an American Social Psychologist, who spent long periods of time in Europe, organized the first meeting of European social psychologists in Sorrento, Italy. He was assisted by a Committee of three European social psychologists: Henry Tajfel, Mauk Mulder and Robert Pagés. Forty people attended the meeting. They represented almost 12 different European countries.

As recalled by Tajfel (1972), many of the attendants discovered, for the first time, colleagues from other European countries. A detailed account of this meeting, with special attention to the role played by important American social psychologists, was given by Jerome Singer last year in Budapest (1990), in his opening lecture of the Plenary Meeting of the European Association of Experimental Social Psychology.

The Sorrento meeting was followed by another one at Frascati, in 1964, where the decision was taken to create a European Association of Social Psychology. Eventually this happened two years later at Royaumont, when the First Official Committee was elected. The Association's main explicit aim was to create a supportive environment for the development of Social Psychology in Europe. It was assumed that the fostering of communication among different European countries was a necessary condition to this end.

Twenty years later, in 1986, Jaspars agreed that those pioneering European social psychologists were trying to articulate an independent forum for Social Psychology. National efforts were considered insufficient for the attainment of this goal, due to their radical inability to overcome national boundaries.

In order to pursue these goals, EAESP set forth several lines of activities: Plenary Sessions every three years, East-West Meetings to promote cooperation with the Eastern European countries, in a period when it was anything but easy, Summer Schools for graduate students. It also created its own Journal (*European Journal of Social Psychology*), its own collection of books (*European Monographs*) as well as a European Laboratory of Social Psychology (L.E.P.S.) in Paris.

As far as the organization of the different activities is concerned, the Association has been very successful. Twenty-five years have passed since 1966, when it was formally launched. In this period of time it has held 9 Plenary Sessions, more than 7 East-West meetings and more than 5 Summer Schools. In 1966 the Association had 40 members; 200 in 1978 and 452 in 1990. The *European Journal of Social Psychology* ranks fifth in the preferences of social psychologists, according to data published by Feingold in an issue of the *American Psychologist* of 1989. All this justifies Jaspars' conclusion that there is now a European forum for Social Psychology.

But, what about social psychological theory and research carried out in Europe during all these years?. Is it different from its American counterpart? Or, in other words, is it distinctively European?. Two important European social psychologists, Jaspars, in 1986, and Moscovici, in 1990, answer this question unambiguously in positive terms. European Social Psychology has, in Jaspars' opinion, three main characteristics, which separate it from the mainstream of American social psychology:

an orientation towards social problems (instead of an exclusive orientation towards theoretical issues); the consideration of the human being as a social agent, as a reflection of the society in which he/she lives, not as an abstraction of the isolated, asocial individual; the subordination of methodological problems to theoretical development, and not viceversa.

Jaspars himself recognizes that his conclusions are only tentative. His analysis draws upon two important publications: Tajfel's posthumous two-volume work "Social Dimension", known as the European Handbook, and Fisch and Daniels' 1982 comparative analysis of two European and one American Journal. But, of course, to reach such a general conclusion about European Social psychology as a whole a more thorough analysis would be needed.

Moscovici (1990) reaches, however, conclusions very similar to Jaspars'. He considers European Social Psychology as an "emancipated" social psychology. Emancipated from what? Emancipated from positivism. According to Moscovici, in the sixties, when European Social Psychology was in a period of consolidation, it had to face a choice dilemma: scientific respectability vs. social relevance. As evidenced by the publication of Israel and Tajfel's book "The Context of Social Psychology", it chose the second part of the dilemma. It was the right choice, for it allowed its emancipation from the chains of positivism and enabled it to understand the reasons and the dynamics of social change. So, Moscovici's conclusion parallels that of Jaspars.

Before leaving behind this second portrait of Social Psychology in Europe, three less positive facts about the Association must be mentioned: 1) national boundaries are still strong within the Association, as recognized by Jaspars (1980, p.422); 2) the participation of the different European countries in the activities of the Association is strongly asymmetrical: in 1980 four European countries accounted for almost 70% of the papers published in the EJSP; 3) an analysis of the first 7 volumes of the EJSP did not allow its author, W. Doise, to reach conclusions similar to those reached by Jaspars and Moscovici. Doise's analysis shows that individualism enjoys good health on both sides of the Atlantic Ocean.

As a final note, it is worth mentioning that the Association has created its own Archives. They are being kept at the University of Louvain. Currently a project of writing the history of the Association is under way, financed by the Association itself (Mikula, 1990, p.6)

A Pluralistic Portrait

When we consider the development of Social Psychology in Spain, we find a situation quite different from the one just described at the European level. My account is based on a paper by Peiró, a Spanish social psychologist. The paper was published in 1984 with the following title: 'Historical Perspectives of Work and Organizational Psychology in Spain'.

Peiró's historical research shows that scientific psychology emerged in Spain at the end of the Nineteenth Century, linked to the concern of progressive intellectuals with education. It was considered vital for the solution of social problems. Fostered by progressive forces, Psychology enjoyed a period of relative prosperity in Spain during the first three decades of Twentieth Century. Institutes of Professional Selection and Orientation were created both in Barcelona and Madrid, each publishing their own scientific Journals. Professional and scientific activities were very intense both at a national and international level, as is proved by the organization of two International Conferences of Psychology in Barcelona, in 1921 and 1930.

The Spanish Civil War of 1936 put an end to all these developments. After the War, both Institutes were closed down and most of the important psychologists were forced to go into exile. The new Spanish government introduced a rational, thomistic, prescientific psychology in the University.

Scientific psychology began to recover slowly outside University bounds. Ten years after the end of the war, in 1948, a Psychology Unit was created at the Council of Scientific Research. (Yesterday you attended the lecture of Professor Pinillos. Well, he was one of the very few members of that Psychology Unit). This was the first landmark of the struggle of Psychology to establish itself anew in Spanish society. The Spanish Association of Scientific Psychology was founded in 1952, and one year later, in 1953, a Professional School of Psychology and Psychotechnics was established. However, the previous, basic studies of Psychology were still lacking, which led a famous Spanish psychologist to say: 'in Spain, Psychology began to build the house by the roof'. To get a University degree in Psychology, Spanish students had to wait until 1969, 30 years after the end of the Civil War.

In his 1984 paper, Peiró makes a final assessment of the state of Work and Organizational Psychology in Spain. He denounces a split between the University and the real world, that is, between the Psychology being taught at the University and the psychological applications in industrial organizations. In my opinion, this is due to a gap between the University Studies of Psychology and the needs of modern organizations, as they exist in Spain.

Summarizing, historical research has shown three important peculiarities of Social Psychology in Spain; 1) scientific psychology in Spain has been linked to progressive political positions; 2) dictatorial governments have opposed scientific psychology, fostering instead its rational, thomistic version; 3) society has evolved at a faster pace than Psychology, due to the institutional handicaps Psychology had to overcome before becoming established. This explains the gap between academic Psychology and the real world.

Well, one could think that Spain is a very peculiar case in Europe. And that is true. But this close look at the evolution of Social Psychology in Spain has disclosed some important features which characterize also other European countries. In fact, what I personally have found is that the more one knows about the development of Social

Psychology in different European countries, the more one realizes the complex interplay between scientific and contextual aspects of the discipline. Let me elaborate on this a little bit.

I will consider briefly the case of Hungary. Here I will profit from Erös' work, and specially from his 1982 paper on 'Some Trends in the development of sociopsychological thought in Hungary before 1954'. What do we find in Hungary? Before giving an answer, please notice that I have to be very brief and that I cannot do justice to the richness of Erös' contribution.

In Hungary, psychosocial thought emerged in the midst of struggles with several reductionisms: the economicist explanations of History, on the one hand, and the Soviet criticisms of Psychoanalysis, on the other. The Hungarian Government did have an interest in Social Psychology, but only because it believed it could be utilized for preserving the existing power structures. As a result, sociopsychological thought underwent a severe distortion, reduced to a mere means of insuring psycho- and socio-technical efficiency and as a repertoire of characterological, national and racial criteria for personnel selection. Erös' summary of the history of sociopsychological thought in Hungary emphasizes the continuous conflict between the technological exploitation of the findings of Social Psychology and the humanistic ideas associated with it.

Another quite interesting account is given by Abma, a Dutch psychologist, in 1983. He focuses on the emergence of scientific psychology at the Catholic University of Nijmegen, the Netherlands, where there is an important Department of Social Psychology. Abma shows that the development of Psychology at Nijmegen before the II World War was determined, to a large extent, by its relation with the Catholic Church. He argues that this made possible the establishment of close links between psychology and the real world, or, in other terms, between psychology as a science and psychology as a profession. After the War, psychology at Nijmegen became more and more dependent on American psychology. This eventually led to a split, within psychology, between scientific and professional activities.

Further historical accounts of the development of Social Psychology in other European countries could be given: Apfelbaum and Lubek (see their 1983 paper, for example) have written several papers on the history of sociopsychological thought in France at the turn of the century. Munné (1985) has published a long and thoughtful paper on the history of Social Psychology in the Soviet Union. But I will stop here. It seems to me that the three illustrations I have just given (Spain, Hungary, the Netherlands) are explicit enough to indicate the core of my argument: a portrait of Social Psychology in Europe, if it has to be accurate, has to be done bottom-up. Otherwise, it runs the risk of becoming a very poor representation of the reality it wants to portray.

This does not mean that a top-down portrait is not valuable. I think it is valuable. It means, simply, that Social Psychology has been built, and still is being built, on the national level and has evolved in close relation with national events. If Social

Psychology wants to preserve its historical and collective memory, it cannot afford to forget this fact.

My conclusion is a very tentative one: Social Psychology has to decide which portrait is closer to the actual state of affairs. Historical research is the key to this decision.

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Ortega-y-Gasset's Mass-man psychology. An Interpretation.

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ABSTRACT

Ortega's analysis of Mass-man (The Revolt of the Masses, 1930) pointed to the rising of new antiliberal personalities. In his essay, Ortega synthesized the various traits of that personality in a two-word diagnosis: Mass-man may be seen as a "spoilt child". In so doing, Ortega was benefiting from the analysis carried out by Alfred Adler on the psychology of the 'spoilt child'.

Mass-man personality may also be related to the well-known psychological construct of "authoritarian personality" analyzed by Adorno and colls. many decades ago, as well as to Wilhelm Reich's The Mass-psychology of fascism (1933), or Erich Fromm's The Fear of freedom (1941). In a more or less convergent line of thought, these studies faced the phenomena of violence and antiliberal rejection of other groups.

All of these studies seem to have been promoted by a "Zeitgeist", clearly connected with many dramatic events that took place in Western history in our century.

The revolt of the masses, the well-known book published by the Spanish philosopher José Ortega-y-Gasset in 1930, has been frequently regarded as one of the most significant analyses of the great social changes that have taken place in our century in Western countries.

It has been noted that, after all, our time could be characterized as a new age, in which masses became the true actors acting out the historical drama.

Unfortunately, a too simplistic view of the book was dominant among its critics. It was suggested that, in writting the book, Ortega was against the promotion of the masses; sometimes, the book was read as an example of social criticism, with a more or less conservative mood, and suspicions arose that a deep rooted élitism was guiding his reflections.

On the other hand, one of the best interpreters of Ortega's thought, J. Marías, carried out a detailed analysis of the philosophical guidelines of the book, showing thus its connections with other metaphysical theories of its author that give it its ultimate meaning.

In our opinion, there is also a possibility of providing a "psychological interpretation", according to which, *The revolt of the masses* would be seen as containing the study of a personality type, the anti-liberal one. The analysis could be kept in line with those carried out by Adorno, or Fromm, in the post-war years.

Let us try to present here in some detail the content of such a contribution to social psychology, carried out at a very early time in our country.

About the author

José Ortega y Gasset (1883-1955), the first Spanish philosopher of this century, and one of our finest literary figures, developed a personal theory deeply rooted in his own circumstances.

According to his view, philosophy could be defined as a general knowledge about reality. Life is the fundamental reality, as all other realities must appear in it in one way or another. In the structural complexity of life two poles are always found: *I with things*, or *I am myself and my circumstance*, both in continuous interaction. Life must be seen as a concept analogous to those of "existence" and Heidegger's Dasein, whose analysis paved the way for a new metaphysics.

In his own life, every man tries to develop a project of himself, promoting self-actualization, according to the norms, values and beliefs of his society. He may fulfil his vocation (authenticity) or he may fail, carrying out an inauthentic project, that falsifies his life. "All life is the struggle, the effort to become oneself" (R., 72).

More specifically, the Mass-man kind of life may be considered as a sort of social pathology affecting Western societies, whose influence has been crucial in the central decades of our century.

The Mass-man

In accordance to his sociology, Ortega maintained that every society includes a minority and a mass. The minority's role is to guide, the mass' role is to be guided. Both are social functions or roles, not to be seen as dependent upon other characteristics or indicators (e.g., a physician's role is to be in charge of therapeutics, while a patient's role is to follow the indications prescribed by doctors; the doctor acts as a minority person, and the patient acts -or should act- as a mass person.)

Mass-man should not be confused with mass-person. The latter is a role, while Mass-man is a certain type or a human variety. "By mass... is not to be specially understood the workers; it does not indicate a social class, but a kind of man to be found today in all social classes" (R., 79). This kind of man is to be characterized in a number of dimensions.

"Masses - Ortega says (R., 15) - are today exercising functions in social life which converge with those hitherto apparently reserved to minorities... masses enjoy the pleasures and use the instruments invented by the select groups... they feel appetites and needs which were previously looked upon as refinements...". Pleasures as sports, competitions, boxing, bull fighting, are now but a small part of spectacles (opera, classical music, painting exhibitions) that were previously considered as élitist shows and are now attended by the masses.

This means that a general rise of the historical level has taken place. But while this change took place, "'good manners' no longer hold sway" (R., 54). Standards, norms, complex forms of education have been simplified, and personal will tends to get rid of problems through force and violence.

Ortega says: "Under the species of Syndicalism and Fascism there appears for the first time in Europe a type of man who does not want to give reasons or to be right, but simply shows himself resolved to impose his opinions" (R., 52)

This is, according to Ortega, a very important trait: Mass-man is against discussions and the free exchange of opinions. "Mass-man would feel himself if he accepted discussion... Hence the 'new thing' in Europe is 'to have done with discussions', and detestation is expressed for all forms of intercommunion which imply acceptance of objective standards, ranging from conversation to Parliament, and including science. This means... a return to the common life of barbarianism [*and to*] direct action" (R., 53).

Such a trait - described in 1930, long before the historical rising of the Nazi movement- points quite directly to some roots of violence that put an end to political parliamentary systems in many Western countries and, especially, in Eastern Europe. A sort of right "not to be reasonable" (R., 52) seems to have appeared for the first time in our civilization, striving century after century for higher levels of reason and humanity.

Mass-man is not defined by his ignorance, but by his attitude towards other people's ideas, neither accepted nor respected.

"If... the psychological structure of this new type of mass-man be studied, what we find is as follows: (1) An inborn, root-impression that life is easy... without any serious limitations; consequently, each average man finds within himself a sensation of power and triumph which, (2) invites him to stand up for himself as he is... not to listen, not to submit his opinions to judgment, not to consider others' existence... He will act then as if he and those like him were the only beings existing in the world" (R., 70). Moreover, "The mass...does not wish to share life with those who are not of it. It has a deadly hatred of all that is not itself" (R., 55).

This means a strong ethnocentrism, that excludes all liberal attitudes towards other people. Variety of values and norms, is not seen as a positive factor that enriches the world, but as a danger that menaces the position of Mass-man, and brings forth insecurity for the group. To get rid of these menaces, tendencies towards self-affirmation and negation of the others are systematically enhanced.

Such a strong egocentrism of Mass-man is not without consequences upon culture. Ortega stressed the fact that the new type of personality here described would have turned its back on civilization. "The type of man dominant to-day is a primitive one, a *Naturmensch* rising up in the midst of a civilised world"; and he adds: "the new man wants his motor-car, and enjoys it, but he believes that it is the spontaneous fruit of an Edenic tree... and does not extend his enthusiasm for instruments to the principles which make them possible" (R., 59). Instead of seeing civilization as the final product of many theoretical contributions, working under the direction of human reason, Mass-man seems to pay attention only to the effects, while forgetting the principles and the ideal basis upon which the whole building has been raised. The historical nature of sciences and techniques is forgotten; no debt towards the past is then acknowledged. So we are facing a 'man of nature' ("*Naturmensch*") and not a man with historical sense.

Mass-Man and the spoilt child

In his essay on Mass-man, Ortega finally arrived at a synthesis of the various traits of that personality in a two-word diagnosis: in his opinion, Mass-man could be seen as a sort of "spoilt child". He says that the two main characteristics of the "well-known psychology of the spoilt child" are these: "the free expansion of his vital desires... and his radical ingratitude..." (R., 41-2). In his life, caprice has no limits; he feels himself unconstrained by things and by other people. For this reason, according to Ortega, these men revolted against minorities.

The connection introduced by Ortega between Mass-man and the spoilt child has been deliberately chosen. "It would entail no error to use this psychology [that of the spoilt child] as "eyes" through which to observe the soul of the masses of to-day" (R., 42).

It is clear that, in so doing, Ortega was referring to the analysis carried out by Alfred Adler on the psychology of the 'spoilt child'.

As is well-known, Adler considered the neurotic style of life as the result of a deep attitude of retreat guiding the existence of the neurotic. These patients are suffering, but they are also incapable of sharing a "community feeling". Life, for these patients, is devoid of social interest and collaborative goals; normal exchanges with other people have turned out to be unbearable, and neurotics are secluded from the open society.

Adler also characterized some neurotic reactions as being typical responses of the spoilt (or pampered) child.

According to his view, the pampered child would become a fully neurotic person, as he has grown in a very special climate, in which all his caprices and desires have been satisfied by the pervasive influence of the mother. This is an abnormal world, from which no transfer to an open-air climate may take place. Normal interactions with other people have been interrupted, and the spoilt child is living alone in his ready-

made world. He will find many problems in solving the three major problems -social life, work and love- that each man has to solve according to his own life style.

It is not difficult to see the essential analogy appearing in both cases: both the spoilt child and mass-man have experienced an abnormal process of socialization, and they have missed the internal acceptance of common interests and motives as guiding forces for their lives. The "radical ingratitude" towards those factors that have made possible the ease of their lives seems to be a common trait in both cases, upon which Ortega seems to have built his view.

Spoilt child, Mass-man and their origins

Ortega's analysis of Mass-man has stressed the social origins of such a kind of personality. It is neither an effect of a style of bringing up, nor the result of unconscious forces acting upon the child. Instead of that, it is a sort of mechanical consequence of the social situation produced by techniques introduced in Western societies in the past century.

Ortega says: " This man full of uncivilised tendencies, this newest of the barbarians, is the automatic product of modern civilisation, especially of the form taken by this civilisation in the 19th Century" (R., 74). He has got all the goods and benefits that can be obtained from society; he feels no obligation towards the community in which he lives. He has been educated only in a technical sense, but he has not been raised under moral rules stressing the sense of responsibility and the duty of gratitude towards those that gave us the real possibilities of a better life.

Mass-man and authoritarian personality

The personality of mass-man may be related to the "authoritarian personality", the well-known psychological construct analyzed by Adorno and colls. many decades ago.

In 1950 Adorno and colls. presented the main results of their study in which a "fascist" type of man was described. It was characterized by anti-democratic tendencies, ethnocentrism and rejection of outgroups, impatience with subjective phenomena (anti-intrareception), general hostility and destructiveness, support of authoritarian aggression, use of stereotypes, conservatism and conventional adherence to middle-class values. Also anti-Semitism and exaggerated sex-repression tendencies were taken as characterizing this kind of personality.

Although not without theoretical and methodological difficulties, this personality pattern generated a large body of research on the interactions between psychological and social factors.

Closeness between authoritarian and mass-man personality should not be overlooked. Both show many common traits, such as conservatism, aggressiveness, proneness to violence, and ethnocentrism, as well as a broad rejection of liberal tendencies acknowledging or accepting values and ideas different from their own.

Ortega presented the fascist and the bolshevist as concrete historical examples of this mass-man type. But he also noted that, in Western societies, some very different kind of people, such as the average scientist, could also be included in this same category. "Bolshevism and Fascism... are... typical movements of mass-men" (R., 67), he wrote, as both are devoid of historic consciousness and try to become the only political accepted force, after destroying all the others. As for the modern scientist, Ortega says that "current man of science is the prototype of mass-man... He even proclaims it as a virtue that he takes no cognizance of what lies outside the narrow territory specially cultivated by himself, and gives the name of 'dilettantism' to curiosity for the general scheme of knowledge" (R., 80). It is always the same phenomenon: these kind of souls are devoid of broad impulses and aspirations towards wholeness, and they take *pars pro toto*, the part as though it were the whole.

CONCLUSION

It is not difficult to see that Ortega's contribution to the analysis of mass-man has suffered from various distortions, that have stressed partial interpretations in which the mass concept has been viewed in a quantitative manner, as denoting a certain social class whose ascent would have been opposed by Ortega. This is not the case. On the contrary, his study was pointing to the rising of new antiliberal personalities, whose historical influence should be viewed as a real menace to the free co-existence of groups and forces in Western societies, and whose acts and deeds in past decades have shown the extent to which Ortega was foreseeing our recent history.

It is true that other perspectives on these dramatic events of our recent history could be found in various works, such as Wilhelm Reich's *The Mass-psychology of Fascism* (1933), or Erich Fromm's *The Fear of Freedom* (1941), in which dynamic motivations and mechanisms were postulated to explain the submission of masses to totalitarian regimes. In a more or less convergent line of thought, these studies faced the phenomena of violence, antiliberal rejection of other groups, and some of the historical events described in the precedent pages.

And, most important, Ortega's analysis of Mass-man stressed, in our view, some social-educational mechanisms through which such a personality could have been produced, establishing the bases for the solution of the problem. Now that fascism, bolshevism, Spanish and Latinamerican conservatisms, among others, are in decline, the need for a humanistic education which may reinforce the will of freedom in our societies should not be overlooked.

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Le théâtre de la peur d'Alfred Binet

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ABSTRACT

*Like many French psychologists in the 19th. century, Alfred Binet had a double career: he was a pioneer of scientific psychology, and he wrote with André de Lorde horror dramas for "Grand-Guignol" theater. Although he separates a scientific Dr Jekyll from a literary Mr Hyde, there are close links between his two careers. For instance we can read some parts of the famous test and some "psychological vivisections" of his daughter in *L'étude expérimentale de l'intelligence* as small Grand-Guignol stagings. Therefore we may infer unexpected hypothesis about some fictionnal genealogy of French scientific psychology.*

Alfred Binet fut non seulement le psychologue que l'on sait, mais aussi le collaborateur d'André de Lorde, le spécialiste français du théâtre d'épouvante et d'horreur, avec lequel il signa plusieurs pièces de Grand-Guignol. Ce fait apparaît généralement comme anecdotique, et l'on se dispense de réfléchir sur le fait que Binet n'a pas été le seul pionnier de la psychologie française à mener une double carrière de savant et de littérateur. Quel lien peut-on faire entre les recherches d'un psychologue qui commença par pratiquer l'hypnotisme expérimental à la Salpêtrière, puis eut des entretiens avec des acteurs et des auteurs dramatiques et inventa un test célèbre, et d'autre part son activité de dramaturge?

Doubles carrières de psychologues

Plusieurs psychologues de la fin du siècle ont mené une double carrière. Jeune magistrat provincial, Gabriel Tarde publie en 1879 des *Contes et Poèmes*, dont il rachète et détruit ensuite les exemplaires, puis il envoie des articles, qui le rendent peu à peu célèbre, à des revues savantes. En même temps qu'il mène des recherches d'hypnotisme expérimental et de psychologie physiologique, Charles Richet écrit romans, nouvelles et fables, la plupart du temps sous le pseudonyme de Charles Epheyre. Henry Beaunis, directeur avant Binet du premier laboratoire français de psychologie physiologique, prend sa retraite et publie, en 1895, des *Contes physiologiques* sous le nom de Paul Abaur. Binet, quant à lui, au début de notre siècle,

avant sa mort en 1911, réactive un goût de toujours pour devenir le second d'un dramaturge.

A travers ces exemples, on pourrait donner statut d'apologue à un conte célèbre de Stevenson publié en 1886, et broser un portrait du psychologue fin de siècle en Docteur Jekyll savant et Mister Hyde littéraire, bien différent de la caricature de savant impersonnel et inhumain qu'on en a généralement. A l'image des sujets de prédilection de la recherche de l'époque, hystériques et hypnotisés à personnalités multiples, l'auteur se clive souvent entre deux identités et deux signatures de façon officielle, tandis que des liens multiples et importants se tissent subrepticement entre l'oeuvre sérieuse et l'oeuvre dite de "divertissement". Les écrits littéraires apparaissent comme l'autre côté du miroir des écrits savants. Romans, contes et drames sont hantés par les thèmes du savant possédé ou obsédé, de l'amour fou et fatal pour ces "instruments admirables" que sont les femmes somnambules, et de l'horrible expérience, vivisection ou autopsie qui transgressent les interdits moraux. C'est au prix d'un dédoublement complexe et retors du sujet savant, qu'a pu, pour une grande part, se constituer une psychologie objective. Il est donc important de comprendre que les travaux connus de Binet s'articulent à des travaux littéraires qui en révèlent certains ressorts laissés dans l'ombre.

Le psychologue, les dramaturges et les comédiens

Après avoir participé au théâtre réel de la Salpêtrière, Binet fait des gens de théâtre ses sujets psychologiques de prédilection, et enquête sur eux avec son ami Jacques Passy. Son intérêt pour la scène est aussi plus direct, car il a collaboré avec lui à un vaudeville, "Le pompier de Justine".

Après la mort de cet ami, de longues conversations et une correspondance le confrontent à l'auteur dramatique François de Curel, qui aboutissent à la publication dans le premier numéro de **L'Année Psychologique** de 1895, de "M. François de Curel (notes psychologiques)", et de "La nouvelle idole" -pièce sur cette nouvelle idole qu'est la science- publiée la même année par de Curel dans **La Revue de Paris**. Au travers du texte scientifique et de la dramatisation qui en est donnée par la pièce de théâtre, se dégage une joute. Dans l'article, de Curel résiste aux tentatives d'objectivation du psychologue; dans la pièce, se joue une lutte à qui sera plus psychologue que l'autre: le psychologue non médecin (à l'instar de Binet), la femme qui croit l'aimer, ou le mari, un grand médecin fanatique de la nouvelle idole?

Puis, toujours dans **L'Année Psychologique**, en 1897 et 1904, Binet mène une enquête sur les comédiens, et fait le portrait psychologique du dramaturge Paul Hervieu. Il s'agit pour lui de montrer, à l'instar de Stanislavski, que, contrairement à ce que soutient Diderot dans **Le paradoxe sur le comédien**, l'acteur qui joue des personnages est analogue à un hypnotisé ou à un possédé. Il s'agit encore d'objectiver, pour en faire un sujet, un individu, en l'occurrence Hervieu, rebelle non plus par une coopération excessive qui pourrait faire basculer les rôles d'enquêteur et d'enquêté mais par une trop grande soumission réservée.

“Un enfant qui a eu peur”. Binet et de lorde

Lorsque Binet va voir André de Lorde, une autre aventurier l'attend. Ce dramaturge est alors l'auteur fétiche du théâtre du Grand-Guignol, fondé en 1897 rue Chaptal à Paris. Après avoir accueilli des pièces naturalistes refusées par Antoine, cette scène se spécialise dans l'horreur. Binet ne peut faire d'André de Lorde son sujet, car il ressent, raconte-t-il dans sa préface au **Théâtre de la peur**, une telle communauté de vues et de passion pour le drame d'épouvante, qu'il franchit le pas d'imaginer et de créer des scénarios avec lui. Binet explique, à propos de celui qu'il présente comme son nouvel ami, qu'“il y a en lui un enfant qui a eu peur”, et il sous-entend qu'il y a peut-être là un point commun entre le dramaturge et lui-même. Ainsi pourrait s'éclairer après coup un intérêt pour “la peur chez les enfants”, qui fit l'objet d'une enquête dans **L'Année Psychologique** de 1896.

Le Grand-Guignol se situe dans la mouvance naturaliste inaugurée par Antoine dans son Théâtre libre. Ses thèmes sont réalistes et souvent inspirés par la science. Il y est question de l'hypnotisme, de la folie et de la médecine, sur un mode très noir, sous le signe de la mort. Le médecin imbu de la nouvelle idole, l'obsédé dangereux par sa lucidité, l'hystérique manipulatrice ou malheureuse, la folle ou le fou meurtriers jouent les grands rôles dans ces drames. Deux pièces, “Un drame à la Salpêtrière”, en 1908, et “L'horrible expérience”, en 1909, paraissent être une dramatisation rétrospective de l'activité d'un homme qui fut l'élève de Charcot et expérimenta sur ses deux filles. Dans la pièce de 1908, Charcot devient un personnage de savant aveuglé par sa morgue qui laisse faire sur une pauvre hystérique une horrible expérience. Dans la pièce portant ce nom, un père attaché passionnément à sa fille tente de la ressusciter par un appareil électrique de son invention, et succombe sur scène, étranglé par le cadavre tétanisé, tué par une morte.

Ce théâtre est réaliste, ou plutôt hyperréaliste, en un second sens, par le rapport qu'il tente d'induire entre le spectacle et le public. Dans le Grand-Guignol, il s'agit de provoquer l'angoisse sans distanciation, de sorte que le spectateur croie, comme dans un cauchemar, de façon hallucinatoire, à la réalité des scènes affreuses qui lui sont montrées. De ce point de vue, il y a congruence entre l'esthétique théâtrale de Binet et sa philosophie. En effet, dans **L'âme et le corps** en 1905, il soutient, à la suite de Taine, que “les images sont toujours réelles puisqu'elles sont perçues ou conçues”.

Vivisection psychologique et theatre de la peur.

Mais il y a aussi congruence entre cette dramaturgie hyperréaliste et le fameux test élaboré vers 1899 par Binet et Simon. Le théâtre de la peur peut éclairer rétrospectivement des éléments importants de l'épreuve psychologique. Certaines descriptions, demandées à partir de la présentation de gravures (p.43, 45, 47), suggèrent des scénarios à “faire pleurer les foules”, pour reprendre le sous-titre donné à l'une de ces images dans la revue **Lecture pour tous** d'où elles sont tirées.

Les histoires absurdes proposées aux enfants, ainsi que les récits à trou présentés aux adolescents, ont souvent des relents dignes du Grand-Guignol, et anticipent

parfois sur les dénouements mortels des pièces à venir, en forme de coups de théâtre imprévus, monstrueux et impensables succédant à des attentes angoissées. Par exemple le suicidé coupé en dix-huit morceaux du test (p.89) rappelle le cadavre qui tue de "L'horrible expérience".

Si une personne qui se promenait en forêt, s'est arrêtée très effrayée, si elle a couru chez le commissaire de police, "c'était pour l'avertir qu'elle venait de voir à une branche d'arbre un... Un quoi?" Au suspense ambigu de la question doit correspondre une "seule réponse juste" et une seule, dénouement univoque et affreux quelque peu arbitraire. Car après tout l'expression "voir à une branche d'arbre" est équivoque, et peut autoriser des réponses plausibles terrifiantes mais moins morbides, ou des réponses désamorçant le suspense en termes de peur imaginaire. Or Binet et Simon spécifient que l'adolescent qui dirait: "un voleur, un apache, un assassin", ou encore: "un dénicheur d'oiseau, un tronc d'arbre, une branche, une touffe d'herbe" aurait tout faux (p.107). Comme dans un problème scolaire, une seule solution est possible et, comme au Grand-Guignol, il n'y a qu'une seule fin, le plus inimaginablement horrible qui soit.

Il est instructif de rappeler que cette histoire de pendu, qui, selon les auteurs, "pique vivement la curiosité des sujets", et sur laquelle des générations d'écoliers français ont achoppé et frémi, renvoie à une anecdote que Binet recueillit antérieurement d'un instituteur et qu'il publia en 1896 dans "La peur chez les enfants": un écolier était tombé en convulsions à la suite d'une rencontre macabre analogue faite dans un bois. Le psychologue, dans cet article, recommande de ne pas effrayer inutilement les enfants. C'est pourtant ce qu'il cherche à faire délibérément dans le test. Ne vise-t-il pas ainsi à susciter une frayeur convulsive à dose homéopathique, garante de l'obtention d'une attention des sujets aux épreuves captivée et légèrement anxieuse? Ainsi le psychologue, en montrant, narrant, ou induisant images et scènes lugubres, est censé susciter un état atténué de peur et d'hallucination. Le sujet devrait donc être mis, en minuscule, dans une situation d'hypnotisé ou de spectateur de Grand-Guignol. Des évocations et des mises en conditions quelque peu angoissantes et brusques rendraient possible la bonne tenue d'un rapport de testeur à testé.

Lorsque dans le dernier quart du dix-neuvième siècle, l'hypnotisme triompha en France, il apparut comme un moyen de "vivisection psychologique", pour reprendre une expression popularisée par Charles Richet, permettant de manipuler in vivo le psychisme humain. C'est dans cette visée que Binet a expérimenté naguère sur les hystériques hypnotisées de la Salpêtrière. Même s'il abandonne l'hypnotisme, il garde le projet de faire des manipulations à vif sur le psychisme de ses sujets. Ainsi dans **L'étude expérimentale de l'intelligence**, en 1903, cherche-t-il à saisir impromptu les images mentales de sa fille: "Je lui dis un jour avec conviction, après avoir parlé de la mort de notre chien: **"C'est triste, tous les animaux meurent sans exception."** Je laisse passer dix secondes puis je demande brusquement: "Quelles images?" Marguerite sursaute, elle déclare qu'elle n'a rien imaginé; puis à la réflexion, elle découvre une petite image insignifiante, un insecte noir, immobile, recroquevillé" (p.56).

Dans cet interrogatoire, une vivisection au minuscule et au quotidien est extorquée par un jeu d'acteur qui vise à tromper l'attention du sujet pour mieux faire une coupe dans son psychisme par surprise. En parlant de la mort des animaux, et non de celle des humains, Binet enclenche une scénographie de Grand-Guignol édulcoré, à usage des petits: rappelons que le Grand-Guignol fut ainsi nommé parce qu'il devait être un Guignol pour les grands. Il cherche à attrister et angoisser Marguerite pour mieux induire chez elle un état psychique où elle soit, comme le spectateur de théâtre, tout à son chagrin et à sa peur, afin de lui extorquer par surprise les images mentales qu'elle aurait naturellement et spontanément eues. Ainsi à une mise en condition par l'angoisse succède un dénouement brusque et inattendu: on retrouve là l'un des ressorts dramatiques des pièces de Binet et de De Lorde.

Il n'est pas sûr du reste que le vivisecteur réussisse entièrement son coup, car sa fille ne lui livre qu'à la réflexion des images, et celles-ci s'avèrent "insignifiantes", probablement plutôt décevantes pour un passionné du théâtre de la peur. Plus loin, d'ailleurs, à propos d'une expérience d'évocation des souvenirs, le psychologue éprouvera le besoin d'interroger Marguerite sur la retenue qu'elle met à ne lui noter que des souvenirs récents et connus de lui.

L'ultime carrière de Binet comme auteur théâtral pourrait ainsi apparaître comme le contre-coup de son activité d'expérimentateur infatigable. Lui permit-elle, comme la tragédie selon Aristote, de faire la catharsis théâtrale de ses passions et de ses angoisses vivisectionnistes? Les pièces de Grand-Guignol exploitent en tous les cas une thématique médicale et psychologique familière au savant Alfred Binet. Surtout elles dramatisent hyperboliquement, sous la forme d'un jeu qui aurait des airs de cauchemar, certains des ressorts cachés par lesquels le psychologue se propose d'extorquer des réponses à un sujet.

En conclusion, l'on ne peut considérer comme purement fortuit que Binet ait collaboré avec de Lorde. L'analyse de la double identité complexe de certains savants français de la fin du siècle nous permet de comprendre sous un angle nouveau et inattendu un aspect de la genèse d'une psychologie scientifique en France avant la première guerre mondiale.

Paradoxalement, du moins pour qui s'en tiendrait à un récit historique autojustificatif et apologétique, l'objectivité paraît avoir ainsi résulté, pour beaucoup de pionniers, d'un clivage proclamé officiellement, doublé d'un rapport tenace et caché à la fiction, qui fit dire par exemple à Binet, à propos de sa collaboration avec de Lorde: "Après la vie, la littérature est peut-être l'expression la plus fidèle de ce que nous sommes". Faut-il généraliser ces analyses à d'autres moments et d'autres pays? Je laisse la question en suspens, sous réserve d'inventaires historiques plus précises...

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The origins of experimental social psychology

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In any field there tends to be a close connection between preconceptions concerning the essential nature of the object to be investigated and faith in the appropriateness of particular methods of investigation.

If experimentation is felt to be appropriate for the investigation of certain aspects of social life, that probably implies quite specific preconceptions about the nature of social reality. The choice of a methodology limits the kind of reality that can be represented in the products of scientific investigation. That raises the question of how methodology and ontology are related historically. Do certain preconceptions about the nature of the subject matter lead to the choice of certain tools of investigation, or do tools provide the basis for theories about the object, as Gigerenzer and Murray (1987) have suggested? What happens when a field of investigation like experimental social psychology first emerges? Why does a tool like experimentation get linked to an object like social psychology?

The systematic application of the method of experimentation to the solution of social psychological questions begins with some work that the German psychologist Walter Moede carried out in 1913 and published in 1920. Just prior to this work Moede had obtained his doctorate at the University of Leipzig. However, by the time that the young Moede began his scientific career, Wilhelm Wundt, was eighty years old and occupied with other matters.

Moede found his inspiration elsewhere. Two sources of influence are evident in his work, the first practical, the second intellectual. Practical interests certainly dominated Moede's career as a psychologist. After World War I, all his academic appointments were specifically in the area of applied psychology, as was his research. He edited the journal *Industrielle Psychotechnik* and chaired the organization of German applied psychologists for many years (Geuter, 1986).

Moede's practical interests are quite evident in his early experimental studies of social psychological problems. Many of these studies were carried out in schools with the benevolent co-operation of the school authorities. The general purpose of the studies was the comparison of psychological processes under individual and group conditions. The reactions of individuals would be compared when working alone and when working as members of a group. The centerpiece of Moede's investigations was

formed by an extensive set of experiments devoted to the Comparison of the performances of school children on school-like tasks.

Such studies would have been regarded as psychologically trivial by Wundt, though they had a precedent in the work of educators (Mayer, 1903) and constituted a natural extension of the work of the educational psychologist Ernst Meumann (1904) who had supervised Moede's doctoral dissertation.

Moede's work represents an example of a new faith in the possibility of holding up a mirror to real social life by means of an artificially created analogue. It is not surprising that a school provides the stage for much of the enactment of this faith. For the school had become an institution organized in accordance with a deliberate rational plan. It provided a social framework within which the old distinction between natural and artificial social forms was no longer very meaningful, where the artificial had become natural. On this territory a pragmatic, manipulative, interest in the rational organization of work could blend with a more general interest in the forces that operated within human groups.

But Moede did not see his extension of the experimental method to a new set of questions merely as a practical device to deal with problems of work efficiency. Although such considerations were clearly important to him, he also linked his experimental work to a very specific intellectual tradition, namely, that of "mass psychology." In fact, he identified what he did as "experimental mass psychology" and saw it as working over ground that had been prepared by the analyses of men like Sighele (1892) and Le Bon (1895).

These nineteenth century authors had become fascinated by the contrast between what they saw as the rational conduct of individuals on their own and the irrational behavior of masses or crowds. There had been speculation about the psychological processes through which the conduct of individuals could be drastically changed by impulses emanating from other members of a crowd. Moede's use of experimental methods in this context was based on a decision to create artificially the contrast between individual and group behavior that the crowd psychologists had believed to exist in natural situations. By this means some of the psychological speculations of crowd psychology might be subjected to direct investigation. For example, Moede tried to determine whether there was an involuntary transmission of movements among the members of a group.

At the time Moede conducted his experiments the sub-discipline of social psychology did not yet exist. What one finds is a colorful spectrum of quasidisciplinary projects, each one linked to the names of a small number of authors, that laid claim to territory perceived to have been left vacant by individual psychology on the one hand and the more established forms of social science on the other. Among these projects that of crowd psychology had a peculiar affinity with certain features of human experimentation. Three such features are worthy of note.

Firstly, experimentation was in practice limited to the investigation of relatively short-term effects. This made it uninteresting as a method for historically oriented

quasi-disciplines, like Wundt's *Völkerpsychologie*, that were concerned with long-term trends. However, crowd psychology had already defined its subject matter in terms of rapidly spreading processes like emotional contagion or suggestion. The exclusion of historically extended explanations by crowd psychology certainly removed one obstacle to the employment of experimentation in the study of social life.

A second feature of experimentation is closely linked to the first. Not only did experimentally studied segments of social life have to be relatively brief, they also had to involve a rather direct form of social interaction. One could not experiment on the network of indirect effects to be found in complex social institutions, in markets, in cultural products. Experimentation meant working with effects that were essentially local in character. Again, this converged very nicely with the interests of crowd psychology that were focused precisely on local effects operating among individuals present in the same place at the same time.

Finally, crowd psychology shared with the prevailing philosophy of psychological experimentation a common understanding of part-whole relationships. Experimentation meant decomposing complex phenomena into their components and then observing the effects of recombining components in accordance with a scheme imposed by the experimenter. Such a procedure might lead to explanations of complex phenomena as long as these could be regarded as an aggregate of separable components and their interactions. That was an assumption which clearly did not apply to the wholistic or organicist conceptions of social phenomena that were to be found in much of the social thought of the time. Crowd psychology, however, had performed an analysis of social phenomena in terms of a scheme that clearly fitted the requirements of the prevailing philosophy of experimentation. That scheme had reduced group phenomena to the temporary influence of one abstract individual on another. The crowd was constituted by separate individual components the sum of whose reactions manifested itself in the social phenomena of crowd behavior. But groups of this nature might well be assembled artificially, as "experimental mass psychology" proceeded to demonstrate. The only missing element was that of intense emotional involvement, but that was regarded merely as a difference of degree and not one of kind.

There was therefore a remarkable fit between the way that late 19th century crowd psychology had defined the social psychological object and the features of social life that were amenable to investigation by means of social psychological experimentation. The model of social life constructed by crowd psychology defined the nature of the social psychoioquical problematic - the contrast between "individual" and group behaviour. This not only prestructured the design of Moede's experiments, but also the design of the better publicized experiments of F. H. Allport (1924) which were little more than replications of Moede's work. Allport shared the radically individualistic model of social relations that had been propagated by crowd psychology but extended it to all of social life. He was therefore able to change the name of the sub-discipline from experimental mass psychology to experimental social psychology. However, the fit between the assumptions and limitations of the method and the definition of the

object of investigation remained as tight in Allport's experimental social psychology as it had been in Moede's experimental mass psychology. The same congruence of method and object continued to characterize that tradition of social psychological experimentation which followed in Allport's footsteps.

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Typology and individuality

Heymans' characterology and its historical background

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INTRODUCTION

Typological classifications have played an important role in human thinking about individual differences since antiquity. The old conception of the *four temperaments*: the sanguine, the phlegmatic, the choleric and the melancholic, has, albeit with some modifications, survived well into the present century. Although the traditional temperamental theory was based on the biological assumption of four bodily humors (and for that reason enjoyed a great popularity in medical circles), this biological foundation played an insignificant role in the way the typology was used. People were classified in a common sense way according to their general behavior. Within the *naturalistic* thinking of the last quarter of the nineteenth century, a period in which there was a revival of typological thinking, a new element was introduced. Attention was paid not only to a lively and faithful description of the various types and sub-types to which a specific classification led, but also to the *hereditary* basis of character-traits. Belonging to a specific type implied a natural necessity which predisposed a person, given certain circumstances, to act in the corresponding way (Van Buuren, 1989). Following the example set by the psychological novel, characterology was expected to explain individual behaviour in the light of inherited dispositions and environmental influences.

In this paper we will analyze the function of typological classification in early twentieth century thought about individuality and ethical responsibility. The three-dimensional typology of the Dutch psychologist and philosopher Gerard Heymans is a very interesting case for this purpose, because of the way he related his characterology to his ethical and metaphysical views on individuality and personal responsibility. Although strong influences of nineteenth-century naturalistic and deterministic thinking can be traced in his ideas, he also transcended this style of thinking in his effort to surmount the constraints of individuality, following the new spirituality that began to manifest itself around the turn of the century.

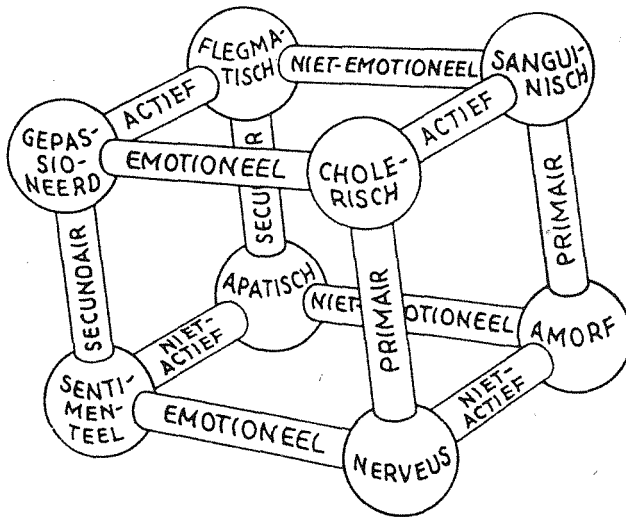
Heymans' typology and its ethical implications

Gerard Heymans (1857-1930) is generally seen as the pioneer of psychology in the Netherlands. He founded (in 1892) the first psychological laboratory in the Netherlands. He held a chair in philosophy, but - like many of his German colleagues - had turned to psychology in the search for an empirical basis for tackling the problems of consciousness and mental functioning which centuries of speculative thinking had been unable to solve. His empirical attitude led him to perform a series of laboratory experiments on optical illusions and the mutual inhibition of mental contents, which were aimed at reducing the complexity of mental processes to a few fundamental *basic laws* similar to the law of gravitation in physics. Considering his bold theorizing, we can say that he applied empirical methods to attain trans-empirical results (Hubbeling, 1983, p.33).

For a true understanding of Heymans' work we have to take his basic metaphysical conception, *psychical monism*, into account. His approach to the diverse subjects he investigated in the course of his career is pervaded by this conception, which he expounded in his *Einführung in die Metaphysik* (1905), and in a number of articles. Its central idea - formulated first in a clear way by Fechner - is that the material world is ultimately only the semblance of the spiritual world: what we experience via our senses as matter, is, in fact, mind (viz. Van Strien & Verster, 1987).

Beside his experimental and metaphysical interest Heymans also had a strong concern with *ethical* problems. The problem of the positive and negative forces behind human behaviour was a central theme in his Leyden doctoral dissertation (1880). He wrote articles on *Zurechnung und Vergeltung* (1883, 1884), and a scathing polemic in the cultural magazine *De Gids* about the place the findings of modern criminology (Lombroso c.s.) leave for personal *responsibility and accountability* (1901). We are inclined to see the large-scale inquiry on the *heredity* of psychological traits he undertook in 1905 in collaboration with his colleague in the chair of psychiatry, E.D. Wiersma (Heymans 1906-1918) against this background. We will leave aside the correlations between the character-traits of parents and children Heymans calculated to determine the degree of heredity, and proceed now to our main theme, Heymans' *typology*.

On the basis of a laborious correlational analysis of the ratings on the 2415 questionnaires that were completed - since all calculations had to be done by hand this took him several years! - Heymans developed a three-dimensional model of characterological differences. The first and second bi-polar dimension on which his typology is based - *activity* and *emotionality* - were taken from Kant's *Anthropologie*. The third was *primary versus secondary functioning* (Gross, 1902). By this he meant the degree to which previous contents of consciousness continue to influence the behavior of individuals. The eight types which represent the possible combinations of the extremes of the three bipolar dimensions can be represented by a cube (known as the "*Cube of Heymans*") which is shown in the following figure.



DE KUBUS VAN HEYMANS

As is clear from this figure, the old Galenean types of the sanguinian, the phlegmatic and the choleric temperament return in Heymans' classification. Heymans sentimental type comes closest to the Galenean melancholic.

Though Heymans had repeatedly stressed that his types were extremes, and that most people should be placed somewhere in the middle of the cube, he nevertheless assigned all persons who were described in his questionnaire to one of the eight types. In doing so, he conveyed to his readership the impression that everybody belonged to a specific type. This impression was enhanced by the second method on which he based his typology: the analysis of over 100 biographies (Heymans, 1908). In his popular book on "special" (=differential) psychology (Heymans, 1929) he illustrated the description of his types with many salient and anecdotal peculiarities of well-known politicians, artists and scientists derived from his biographical investigations.

In addition to character in the sense of his typological model (sometimes he uses the "old" term *temperament* here), Heymans also distinguished character in the narrow sense of core characteristics which form the *moral kernel* of personality. Character, in this more specific sense, is defined as the personal balance of the drives which determine our decisions (Heymans, 1901). He classified these drives into four groups: vital drives, egoistic drives, social, or altruistic drives and abstract or suprasocial drives (Heymans 1906-1918, cf. Heymans 1929). In some of his writings, especially the early ones (Heymans 1880, 1883), he speculated about a *character-formula* consisting of a quotient of the (evil) egoistic drives and the morally positive drives. The strength of someone's drives is, just like the degree of activity, emotionality and secondary functioning (of his typology) seen as *inherited* and *immutable*. Yet he believed that a person is responsible for his decisions. In his view the person *is* his character, and not something apart from it. The only grounds for moderation of our moral condemnation of a deed are *mitigating circumstances*. These include, on the one hand, intellectual impediments, which might prevent someone from fully realizing the

consequences of an action, and on the other hand characterological factors, such as a high degree of emotionality or a strong primary functioning, which might lead to a narrowing of consciousness, with the result that an immoral deed is a reflection of the situation rather than the basic character. 'Characterological' here has the broader meaning of "temperamental", as in his typology, and character in this sense functions as a *moderating* factor in our ethical behaviour.

It is precisely for this reason that Heymans attached a great *moral* value to self-knowledge and insight into human nature in general. Just as we have learned to cope better with the material world thanks to the advancement of our knowledge of the laws of nature, our increasing insight into the laws of human nature will enable us to regulate our behavior in the most morally desirable way. Insight into the limitations of their own temperament could help people to act more in accordance with their own character. People whose consciousness is narrowed by their emotionality and primary functioning, who are thus at the mercy of haphazard fancies and impulses, could practice postponing decisions. Parents and educators could teach this.

Because character, the factor that ultimately determines behavior, was considered by Heymans to be genetically based and therefore constant and immutable, the moral progress of humanity depends ultimately on *conscious choice of partner*. Even now, as he showed at the end of his *Einführung in die Ethik* (1914), morally positive traits such as compassion, tolerance, sincerity and reliability are found (as appears from the heredity inquiry) more frequently among married than unmarried people, while negative traits such as inconsistency, insincerity and slovenliness are found less frequently. If, he reasoned, we realize moreover that people aim for the morally superior in their choice of partner - evil too is attracted by the good! - we can expect a slight improvement from generation to generation. When the choice of partner is made on the basis of character traits - which we will learn to assess more quickly and more adequately thanks to psychology's progress - instead of on the basis of contingent social and economic criteria, and when the genetically inferior refrain voluntarily from procreation, as Heymans hoped, humanity will progress towards the ethical ideal. The concluding sentence of his *Ethics* reads: "When the Kingdom of God ultimately will come true, it will be the fruit of human effort".

As appears from the last sentences, Heymans took a *supra-personal* stance in his approach to the ethical aspects of character. The monistic idea of a common I of which each individual is a temporary separation, led him to conceive of the basic drives, which constitute the individual character, as part of a *hereditary chain*, the final fate of which is in our own hands (cf. Heymans 1905, p.341, and Heymans 1914, p.125). It is up to us to extinguish the selfish drives and to cultivate the unselfish ones, thus gradually improving the moral stock of humanity. In this way humanity will eventually overcome the limitations of individuality. In the same vein he saw our judgments of our own and other's choices as more than personal opinions. It is the eternal moral law which slumbers in each of us which enables us to distinguish between good and evil.

The grand perspective: Heymans' rectoral address

On September 20, 1909 Heymans addressed the academic community of Groningen on the occasion of his resignation as 'Rector Magnificus' of the university. In this oration, entitled *The Future Century of Psychology* (Heymans, 1909) he sketched his vision of a better world, a world improved through the benefits of psychological insight. In answer to the question how the 19th century, the age of the natural sciences and all the good they brought, could also have been an age of "utter pessimism", he distinguished three causes.

Firstly man had become *alienated from himself*. He had become a "labyrinth to himself" (op cit, p. 11). Without the protection of tradition man had become over-sensitive to the multitude of increasingly incoherent and contradictory influences. This had caused a disintegration of mental existence, which expressed itself in a general lack of constancy: the number of divorces had increased, many people changed profession, etcetera.

Secondly, people had become *alienated from each other*. "For the differentiation of individualities keeps pace with the complication of mental life" (op cit, p. 12): the more 'influences' people undergo, and the more divergent these are, the more people will become different, and the more social relations will disintegrate. This had lead to a marring of social life by myriads of misunderstandings, which was all the more serious because "few things are so fatal as the division of minds". The best way to overcome the *curse* of our individual boundedness was to contemplate the things that have been thought and wrought by people wiser and better than we (op. cit., p.13).

Thirdly, man had become *alienated from the 'foundation' of things*. In the past, religion had provided the background which connected the different parts and episodes of life in a meaningful whole. Nothing has replaced this function of religion yet.

Psychology, and characterological knowledge in particular, were seen by Heymans as the medicine that would cure the ills of disintegration and alienation. Of course, the typology was still imperfect, but as psychology progressed its beneficial effects would become apparent. These effects were two-fold: in the first place, insight into the typology would bring order in the chaos, reducing the apparent multitude of personalities to a limited number of types. This would allow prediction of one's own and other's actions and even thought-processes, and thus lead to more rational decisions at crucial points in one's life (marriage, occupation, etc.). In the second place, insight into the typology would make people aware of the fact that one's personality is but "a specimen of a particular, well-defined psychological group" (op cit, p. 16). This would cure the hubris of individualism once and for all.

By now it will be clear to the reader that to Heymans psychology was much more than a way of describing and explaining individual differences. To Heymans the variety of temperament and character was not an ultimate datum which humanity has to accept as a fact, but the consequence of the individuation of the human spirit. The *Future Century* ends with a description of the way in which the progression of

psychology, combined with self-imposed eugenics, will eventually enable mankind to overcome the limitations of individuality. And he concluded the third edition of his *Metaphysik* (1921), in which he has expounded his psychical monism, with the assertion: "In the certitude that we are all united in a common I, a deeper being, eternal and capable of continuous progress, lies the proper consolation of psychical monism."

The historical background of Heymans' thinking

Various authors in the historical and social sciences have described the changes in Western mentality following the Middle Ages in terms of an increasing preoccupation with human individuality. In the 18th and 19th century the awakening bourgeois consciousness, and the attendant instrumental approach to human capacities and personal characteristics had led to a growing preoccupation with the analysis of psychological "mechanisms", and with psychological classification (Jaeger & Staebule, 1978). The spectacular new developments in the natural sciences led in the second half of the nineteenth century to technological optimism, and the anticipation and thrill of ever new discoveries that would transform the world into a better place. There was a conviction that natural science would not only provide the insights necessary to get a firm grip on the material world, but would also show us the way to a better society.

This, however, is only one side of the nineteenth century. The other is that of an age of fragmentation and the loss of natural bonds. It is this face of the previous century that had caused the "utter pessimism" to which Heymans alluded in his oration. This aspect of the *Victorian era* can be characterized by the following four global themes (cf. Baumeister, 1986):

The first theme is a *fascination with fragmentation*, chaos and chance, and a continuous striving to master this instability with the help of systematization and classification. Sennett (1976) has attributed this attitude to secularization - a second theme. Faith in God as a uniting principle behind appearances was replaced by a faith in the meaningfulness of the phenomena themselves. The third theme is related to personal identity: The romantic idea of a deeper, hidden self behind appearances had lost its appeal. Instead, a conception of the *self as close to the surface of the person* emerged. Furthermore - and this is the fourth theme - with the inner so close to the outer world, whatever happens around one has a *direct influence* on one's personal identity. A stable environment was therefore regarded as essential for a stable character (this was the reason that such a great value was attached to the nuclear family).

The fin-de-siècle brought a new preoccupation with spiritual matters, and a longing for deeper meaning and fulfillment. From this vantage point the preceding materialism and naturalism seemed empty and shallow. A new sensitivity to the deeper aspects of reality came to the fore, which made the early twentieth century receptive to Freud's and later Jung's ideas about the hidden forces and treasures of unconsciousness, a sensitivity that manifested itself in the arts in the development from naturalism and impressionism towards art nouveau and symbolism.

Let us now return to Heymans and his typology. Heymans appears to have been a typical exponent of the transition from nineteenth-century positivism to the new spiritualism just described. He retained the nineteenth century trust in the methods and the reductionistic aims of natural science. He also retained its optimistic faith in progress. But the counter-motives of the Victorian era can be clearly recognized as well; especially when he directed himself towards a wider public: his articles in *De Gids*, and his oration. Aversion for chaos, complexity and chance and concern over the fragmentation of the inner world by the shattering influence of the outside world are central themes in these writings. His typological classification was intended as a means of bringing order into the complexity. Knowledge of one's own character and its heredity will, in the age of psychology, furnish man with wisdom in handling interpersonal relations and in making well-balanced, "objective" life-decisions. He emphasized that humanity needed a new compass in a world in which religion was loosing its grasp on human conduct - the theme of secularization - a world, gone adrift, in which family bonds no longer offered a safe heaven.

In Heymans' more reserved and even negative attitude towards the outer world, and his searching for a deeper meaning and sense, the late nineteenth century's transformation of European consciousness in the direction of anti-materialism and anti-naturalism manifested itself. Heymans wanted not only to *classify* individual differences, but ultimately to overcome the *limitations of individuality itself*. He longed not only for a pure, well balanced life, but for the ultimate *merging of individual consciousness into the World-spirit*.

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Character and sex - History of psychological classification and fixation in Germany

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ABSTRACT

Psychology of the Enlightenment, life-philosophy and psychoanalysis tried to define the relation between character and sex not only within a theoretical framework, but also with regard to its practical applicability. These continued efforts to define constants of personality are characterized by a formal classification, a fixation and a naturalization of gender-relations in its contents. The paper analyzes texts by German characterological authors of the 18th and early 20th century, and attempts to show that this has been a necessary and dangerous self-limitation of psychology in its scientific development to clinical professionalisation.

INTRODUCTION

Character is an important concept in clinical psychology today to which not only theoretical, but also practical relevance is due. In fact, in the 18th century it was already applied, but a systematic and precise formulation first occurred during the late 19th and early 20th centuries. Its establishment as a basic theoretical concept cannot be separated from its relative application, as Geuter has already indicated for characterology under National Socialism (Geuter 1984, p.144). Since the 18th century, formal and substantial classifications of personality constants have been founded on the concept of character, which have served as a conceptual instrument for, at first, a study of the soul, and, later, a clinical-psychological practice.

The establishment of the category of sex is closely tied to that of character. Since interpretations of general sex characters began to be developed in the 18th century, both terms have remained structurally interrelated. I would like to show in the following pages how enlightened, philosophical and psychological theories have attempted not only to decide conceptually the relation between character and sex, but also with a view to constructing a practical applicability. The concepts of character and sex should, indeed, comprehend and classify personality development in such a way as to be capable of being further developed, along with their theoretical implementation,

in clinical observation. However, directly as a result of the interaction between theory and practice, they produce a formal outline and substantial arrangement for individual development tendencies just as they encourage a naturalization of social relationships.

Idealization and physiognomy

In the 18th century, theories arose presuming naturally endowed sex characters, which, through idealization, contributed to a social and psychic standardization of femininity and masculinity (Ebrecht, 1991). Clearly separated, sexually specific characteristics were derived from physical, psychic and moral differentiations. In 1802, Brandes argues "that bodies of such different organization must bring forth... the greatest difference in the development of mental and moral disposition." (Brandes 1802, p.23) From the strong, male and the weak, female body structures, he develops the fundamental criteria for the particular sex characters:

"Devotion, gentleness, tender and deep feeling, refinement of the spirit are, in general, the predominant dispositions of women; strength of mind, as demonstrated in the association of sundry ideas, in the adherence to and the inferences from the associations of ideas, superior and more enduring play of the imagination, active fortitude of character are on the contrary the dispositions through which men distinguish themselves." (p.27)

The ascription to character of feminine gentleness and masculine strength together served as a basis for the legitimation of specific development goals. In the orientation toward specific ideals, women and men should develop a constant character. Women should approximate the aesthetic ideal of the beautiful soul, and men the moral ideal of the noble soul. With this psychologization of moral standards, an attempt was made, for both sex characters respectively, to derive and establish an assessable, continuous and unitary structure. And, because the concepts of enlightened reasoning and good fellowship called for an enduring mastery over emotions and desires, a substantial and formal assimilation by individuals to standards of character would have to occur. To be sure, men were granted a reasonable personality by nature. Masculine "soundness of character," writes Brandes in 1787, "is missing in a woman. But a certain stability, a harmony of character" could be developed by her (Brandes, 1787, p.46). It would be of value to women to develop the innately missing inner cohesiveness of character with the help of moral instruction and self-education. Ehrenberg has us consider that women, too, could mould within themselves a noble character "through prudent work on themselves": "Nevertheless, the charm of the female character is in great part the achievement of noble sentiments, of early training, of well-tried custom; but personal endeavor will thereby not become superfluous." (Ehrenberg, 1809, p.V.). A virtuous character should, however, not only be developed in the soul, but also manifest itself outwardly.

This significant relationship between physical manifestation and spiritual qualities was made by Lavater into the foundation for his character research in physiognomy

(Billmann-Mahecha, 1989). His idea, that qualities of character can be decoded from their physical expression, anchored a maximum of moral-psychic control in social contact. The body becomes a betrayer of the mind and is declared the organ of control over its moral composition. Lavater's expressed goal in 1776, to elevate the "physiognomy of feeling" to "physiognomy in the spirit of observation" (Lavater, 1776, p.16), contributed toward making the formerly preconscious perception of emotional agitation conscious, intensified and conceptually classifiable through observation of the body. This pre-scientific approach could be used to standardize the psychophysical regulation (of sex ratios also) of oneself and others through its immanent reference to practice.

An anonymous author in 1776/77, in the magazine *Deutsches Museum*, refers to the connection between physiognomy in theory and in practice:

"The application belongs to the understanding of the thing itself... Physiognomy... must be in a state to enable anyone, without his being at the same time a born expert on the subject, to acquire theoretical understanding and artful application of the thing." (Anonymus, 1776, p. 75)

The connection between soul and body becomes comprehensible with a system of signs through which the decoding should aim "more at universal views, than at catechized instruction in detail." (p. 350) The demand already suggested here is for a practical, manageable system of psychophysical classification and typification, which should later determine the development of characterology into a science relevant to practice.

Naturalization of gender-relations

The enlightened formulation of constant sex characters from moral, psychic and physical qualities was expanded by Bahnsen in 1867 into characterology as an independent science. Bahnsen credited ideas from Schopenhauer's philosophy of life with the connection between moral philosophy and the study of the soul: The Will stands for him as "the foundational core of the whole personality" (Bahnsen, 1932, vol.1, p.13) and is useful for the division into "character strengths", which maintain themselves, and "character weaknesses", which accommodate themselves to external factors (p.422). Attached to this polarization is Bahnsen's definition of sex characters: moral "strength of character" should be conjointly "the specific noble attribute of the man - as 'manliness'" and would have its cause "not so much in a certain degree of strength or energy of will, but is, on the contrary, the expression of unity in this direction and... under all circumstances remaining the same." "Unity" and "wholeness" should be decisive attributes of the male character (Bahnsen, 1932, vol.2, p.423). Bahnsen derives from the "Teleology of Mother Vocation" an antimoral and discontinuous basic character for the female sex. Women, in contrast to the rational being, "man", should be seen, merely on the basis of their physiological requirements, to be more oriented by feelings and, therefore, incapable of any unitary morality. They

should only be able to experience injustice in a concrete individual, but not as an abstract, continuous principle of justice. Two lines of argument by Bahnsen changed the further development of characterology into a practice-oriented science: one, his interpretation of character as (ethically determined) wholeness, and, the other, the sexually specific distinctiveness of reason- versus feeling-oriented character.

In the context of the philosophy of life at the turn of the century, characterology was scientifically and practically able to further establish itself. Character was now defined as the "special feature" of the indivisible, organic individual (Häberlin, 1925) (Klages 1928, p.12f.), as "unitary, enduring structure" or "resolute, individual unity" of the soul and body (Utitz, 1925, p.15) (Baumann, 1910, p.44) (Müller-Freienfels, 1935, p.13) as well as "relatively permanent qualities" and "whole framework for forms of experience" (Lersch, 1938, p.23). These definitions make reference to the vitalistic view (held by Driesch for instance), in which the organism should be an autonomous, entelechial union of body and soul, which should evolve toward an immanently idealistic goal. The individual should evolve towards his ideal structure from within himself, i.e. from within his own nature. A naturalization of reason and social determination has by now taken place here. Indeed, the philosophy of life still comprehended character as the psychic variant of the individual, immaterial and autonomous principle of life. However, by functionalizing its concept of organism, it could later, under National Socialism, be declared the hereditary, physical disposition of superindividual wholeness.

An establishment of characterology into the natural category of "sex" was attempted at the turn of the century, especially in the discussion over Weininger's book "Sex and Character", published in 1903. Weininger sees in the relation between male and female in his day the "main principle of all scientific characterology" (Weininger, 1903, p.71). In that "woman", in contrast to man, should possess neither memory nor will nor continuity and logic nor standards (p.109ff.) but only "incomprehensible feelings" (p.245), he denies her soul, character and ego. Characteristic of Weininger's writing is its inflexible means of argumentation, which endeavor to make each new step appear severely logical. This pseudorationality no longer allows for the sex characters as an aspiring developmental goal, but as given by nature, necessary and universally visible.

In a time when the first women's movements began to articulate their demands, this appears to have been consistent with men's requirement for clear and indisputable dominion over relations between the sexes. In fact, Weininger calls for the "legal equality" of man and woman (p.342). In that woman, however, in her natural passivity should always orient herself towards man, she should be her own "ultimate enemy in the emancipation of women" (p.447). As absurd as Weininger's schematic characterology appears today, back then it was a great success. Apfelbach, for instance, praised Weininger as the first "to attempt to rationally resolve the character problem" (Apfelbach, 1924, p.1).

Blüher also maintains in "The Domestic and the Mental Antifeminism" of 1916: "We know since Otto Weininger, that the emancipation of women is meaningless, since it is the essence of the woman to want to be bondage." (Blüher, 1916, p.4) Blüher derives the sex ratio from characterology:

*"If it is correct that physiognomy means **anything** at all, if the shape of a face actually indicates substance, then it is also correct that the substance of man in its essence is completely different from that of woman." (p.41)*

Only man could be mental in a creative sense. The essence of woman, however, should be eros; she should be "dull" (p.6). Blüher accordingly calls for women to be excluded from men's associations and male society as the "only productive forms of social togetherness" (Blüher, 1919, p.89ff.). Characterology needed nothing more at the turn of the century than such a clarification to establish, scientifically and practicably, a socially standardized practice among the sexes. It served even more to strengthen and defend the status quo against women's struggle for emancipation. The social requirements for the masculine definition of civil society were logically formalized and substantially naturalized in the theories about fixed sex characters.

The concept of character was further narrowed and simplified in the biological foundation for the philosophy of life. Physical qualities were no longer viewed as expressions of individual and variable, spiritual states, but in reverse, with physical structure inferred from spiritual composition. Characterological typologies, like those by Kretschmer and Jaensch, produced then only classifications which organized those qualities seemingly found in nature and utilized them for a destructive practice. Kretschmer declares in 1925 to have furnished the "master key" to the "central question of medical and psychiatric clinics" with his typology of body structures and temperaments (Kretschmer, 1925, p.2). His association of constitution with race, however, prepared for the utilization of characterology under National Socialism.

Characterology under National Socialism proceeded entirely systematically and undifferentiated in that it generalized the individualistic concept of wholeness in the philosophy of life and shifted away from the soul to the body. Character was declared the hereditary singularity of a pureblooded society. Schering, for instance, plans a characterology of wholeness which proceeds from a common people and race (Schering, 1937, p.25ff.). He also determines from this the sex ratio: "It recognizes the man as a warrior in life, as comrade and useful member of his own kind, the woman as mother, companion through life and center of the family." (p.54) The theory-practice relationship has become strengthened here unilaterally: the definitions of character and sex have consolidated into a theoretical form, which no longer further develops in its respect to practice, but delivers only the naturalized basis of legitimation for an especially deliberate and destructive conception of practice.

Psychoanalysis and character

Today in psychoanalytical diagnostics and therapy for neurotical illnesses, character neuroses are demarcated from symptom neuroses. The view prevails that character neuroses would not exhibit "typical neurotic symptoms" but rather envelop "the whole personality" (Hoffmann/Hochapfel, 1987, p.10). In 1923 Freud deduces "that the character of the ego" should be "a result of abandoned object cathexis" (Freud,

1923, p.257). In that the male or female character shapes itself from within, according to Freud, during the Oedipal stage in conflicts with the father, the type of character depends upon the super-ego or ego-ideal at the given time. From which, in 1925, Freud reasons,

"that the niveau of customary standards will be a different one for the woman. The super-ego will never be as inflexible, as impersonal, as independent of its emotional origins as that which we demand from the man. Character traits, for which criticism has reproached the woman all along, such as her showing less sense of justice than the man, less inclination to yield to the greatest requirements of life, her more often allowing tender and hostile feelings to guide her decisions, would find adequate grounds in the above derived modification of the super-ego formation." (Freud, 1925, p.29f.)

This view seems to proceed with the attributions which concerning the sex characters had evolved since the 18th century through, first, social and, then, clinical practice in character research.

While Freud granted character no systematic significance in his theory, his students extracted a critical dimension from it. Character increasingly received a function in psychoanalysis as mediator between social influences on and psychic total development of the individual. By 1925, Reich establishes that psychoanalysis should no longer be only "symptom therapy" but develop "instead constant to the therapy of character." (Reich, 1925, p.6) Reich distances himself from the view that neurotic characters would not suffer from symptoms of disease. He maintains instead "the periphrastic neurotic symptom would be directly consistent with the **fragments...** while the neurotic character is always an expression of the... corresponding **general attitude.**" (Reich, 1925, p.17) The neurotic symptom, therefore, is based on a neurotic character. It is not seen, then, as a foreign body but as an integral component of the complete personality and its social reference.

Fenichel for example defines character in 1931 as the "function of constants, hence, organized elements of the ego" whose habitual manner" is "coming to terms with the inner and outer world" (Fenichel, 1931, p.136). In as far as the ego, according to Fenichel, receives the function of mastering reality, the "mediation between the instinctual world and the outer world" (p.137), character forms a switchboard between social reality and psyche. Fenichel stresses that attempts, such as Freud's, to place the significance of the super-ego for character in the center seek "to reduce the empirical differences in male and female character to differences of the male and female super-ego formation" (p.147). If, however, the characteristic differentiations occur more likely in the ego than in the super-ego, then they are to be transferred out of the Oedipal phase and into earlier forms of object relationships. This criticism opens up the perspective for understanding character and sex as a special processing of social forms, gender-relations and forms of identity and individuality (Jäger & Staebble, 1978). A field would thereby be made accessible for psychological characterology beyond schematic arrangements and naturalization.

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The phenomenological nature of Skinner's radical behaviourism

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ABSTRACT

The purpose of this article is to show that the main difference between the perspectives of methodological and radical behaviorism is that the former functions -really, practically- within a 'phenomenological' framework, whereas the latter assumes a conceptual framework of a 'representational' (Cartesian) nature. This a-dualism inherent to the phenomenological perspective is what allows radical behaviorism to eradicate both the mentalist and physicalist prejudices where methodological behaviorism becomes entangled because of its representational perspective.

This article aims to show our concept of the real sphere of sense of Skinner's Radical Behaviourism. The main idea we intend to develop is that radical behaviorism functions in practice as a *phenomenology of behavior*; this implies that it embodies in fact, an *a-dualist* perspective, which allows for the eradication of both the physicalist and mentalist prejudices characterizing most psychological trends.

The outline of the inevitably succinct exposition of this argumentation runs as follows: In order to achieve our purpose, the essentials of the perspective or argumentation of Radical Behaviorism will be expounded, in the first place, the difference of sense between Radical and Methodological Behaviorism being taken into account. This will permit a consideration of both the representational and phenomenological perspectives from which psychological work can be faced. In the second place, after a brief statement on the phenomenological nature of all psychological field, we intend to show how Skinnerian radical behaviorism bends in practice towards the phenomenological nature this field evidences.

Radical behaviorism as a counterfigure/counterimage to Methodological Behaviorism

In order to show the radical lack of symmetry existing between Radical and Methodological Behaviorism it will be helpful to point out the two ways in which we

think the study of behavior has been developed within psychological science: one might be characterized as representational; the other will be characterized as phenomenological. The former stresses an alleged methodological similarity with physical-natural sciences; from this perspective, behavior is merely viewed as a datum granting the 'scientificity' of psychology. The latter, on the other hand, demands sticking to the study of behavior, and justifies its position by referring to reasons *directly* related to the thematic and material contents of the psychological field, insofar as that these contents are specifically and exclusively acknowledged as behavioral.

Hence, methodological behaviorism adheres to the first type of argumentation, while Skinnerian radical behaviorism remains attached to the second. In the case of Methodological Behaviorism, behavior has a *slanting* and *abstract* role oriented towards a purely nominal 'scientific homologation'. Therefore, we lose sight of the fact that behavior is the *specifically psychological content of the psychological field*. It must be pointed out that this abstract and representational consideration regarding behaviorism is not exclusive to neobehaviorism, but has its roots in Watson's initial proposal as well.

As a matter of fact, Watson's works are boosted by a scientificist voluntarism which aims to obtain the corresponding scientific homologation by assuming physicalism (Fuentes, 1991); but this aim does not allow, in fact, the access of the psychologist to any thematical content of the psychological field itself. The aim of Psychological Neobehaviorism precisely was to delimit the psychological field by embodying 'other factors' placed in a different dimension from the strictly behavioral -(independent) stimulus factors and (dependent) response factors. These other factors are the depository factors of that specific thematical content and explain, at the same time, the functional relationships between stimuli and responses present in the behavioral dimension. But the appeal to other factors should fulfill Watson's (physicalist) methodological requirement in order to maintain the alleged range of scientificity; hence, the methodological step of reinserting these other factors in the plane -a second time- of allegedly physicalist behavioral data was needed. As is known, logical positivism lends the epistemological alibi needed for the fulfilment of that step (Fuentes, 1989).

We will now briefly contrast Skinner's Radical Behaviorism perspective to this methodological conceptualization of behavior.

To start with, the whole argumentative logic of radical behaviorism is based on the following thesis: it is precisely by an experimental control of environmental (independent) factors that the explanations of behavior are *in fact* made. The assumption of this fact is what allows us to understand the sense environmental (independent) factors have in the organization of the psychological field itself. So that the significant nucleus presiding the net of radical behaviorism is placed in the effective *locus of control* from which the organization or construction of behavior and, therefore, its explanation is made possible.

From Skinner's point of view, indeed, behavior may be only explained when it is *experimentally built or produced*, and this is only possible through the experimental control and manipulation of environmental factors this behavior is a function of. Thus, as may be readily perceived, the radical behaviorist concept *eradicate the embroidered conceptual network* necessarily linked to the *circuit of oscillation* between behavioral (dependent and independent) factors and hypothetical factors with which methodological behaviorism got inevitably and characteristically entangled. If, in fact, hypothetical factors are conceptualized from the beginning as though they were able to contain (thematical) contents different -as a rule- from behavioral ones, What is left of this claim if they are required to be introduced into the behavioral plane? This claim -as Skinner himself noticed (Skinner, 1950)- is an empty methodological voluntarism: insofar as hypothetical factors are destined to revalidate their thematical width in a behavioral plane it is of no account and, consequently, the use of such hypothetical factors becomes *unnecessary*.

Hence, Skinnerian criticism of methodological behaviorism emphasizes the following idea: no matter what kind of mentalist or physiological drapery is used to disguise hypothetical factors, when one assumes in practice that behavior is produced and, hence, explained by the experimental control of independent environmental factors, these alleged other factors, precisely for being 'drapped' with a pretended non-behavioral costume, become entirely unnecessary. Taking this into account, Skinner says, it may be acknowledged that the functional relationships of control, i.e. the behavioral relationships themselves, constitute the whole of the psychological field on their own right. All this implies that Skinner's argumentation is characterized by its essential pragmatist and functionalist nature -which constitutes the true texture of the psychological field as well (Fuentes & Robles, 1991).

As may be seen, up this point we have restricted ourselves to expound the essential characteristics of the argumentative logic of methodological and radical behaviorism, and also to highlight the radical lack of symmetry between both perspectives. As already expounded, radical behaviorism is based on the pragmatical statement that, in day-to-day psychological research, in order to establish behavioral relationships it is enough to stick to the control of independent environmental factors, all appeals to other non-behavioral factors that might organize or account for behavior appearing correspondingly unnecessary.

Therefore we may ask ourselves for the reasons why this is so, i.e.: How and why, in psychological work, does the mere control of environmental factors organize and account for behavioral relationships?

In our opinion it is possible to understand this insofar as we realize that psychological work and, hence, the control of environmental factors is done in a phenomenological plane -which necessarily structures the psychological field.

The Phenomenological Structure of Radical Behaviorism

The representational perspective in Psychology here exemplified by Methodological Behaviorism is, in fact, centered in the "world's prejudice" inherent

to Cartesian epistemology (Robles, 1991a); i.e., the representational perspective includes the assumption of the existence of a physicalist plane that is structured according to some modality of the Cartesian 'distinction' and 'clarity', and stands as a ultimate depositary of scientific authenticity. Once this prejudice has been acritically assumed, behavior appears as a mere methodological instrument: it is understood that behavior is open to public observation and that this kind of observation is an access to the physicalist plane characterizing all physical-natural sciences. In our opinion, however, to assume that immediate and direct observational contents -behavioral contents- have a physicalist character is a mistake; to us, on the contrary, these behavioral contents have a phenomenological character.

In order to make a quick characterization of the consistence of the phenomenological threshold inherent to psychological field, we will refer to an instance in the history of psychology. Traditional Gestalt Psychology may be said to have placed phenomenological experience in its natural locus by identifying it with ordinary, spontaneous, natural experience -that is, an experience that has some meaningful object as its immediate content. Hence, Gestalt Psychology brought about the idea of the need of differentiating both planes of experience: the physicalist and the phenomenic; according to this view, the physicalist contents of knowledge are 'constructions', not 'data' of immediate experience. This perspective allows for a more accurate identification of the above mentioned mistake all representational psychologies make: Watson and the Neobehaviorists take as granted that the behavior of others, as an immediate content of public observation, is physicalist and therefore objective; nevertheless, we can now understand that the behavior of others, precisely as an immediate content of the psychologist's observation, is merely a *phenomenic* experience datum for any observer, especially the psychologist, not a physicalist construction.

The main features of behavioral *phenomena* constituting the psychological field will be now considered. For this purpose, we will briefly refer to the notion of *operant intentionality*, which was conceived in the philosophical sphere by phenomenologists and will enable us to perceive more clearly the *phenomenical and pragmatical* character inherent to the psychological field. As is commonly acknowledged, phenomenological methodology mainly consists in trying to describe phenomena referring to what appears in them. From the phenomenological point of view, the main feature of experience is *intentionality*, that is, the character of being directed towards an object. By making intentionality (i.e., the *referential* character every experience has) the focus of analysis, one does not have to refer to those two poles Cartesianism had as assumptions -*res cogitans* and *res extensa*-, which produced the characteristic confusion in representational psychologies.

Now, this intentionality is an intentionality adjusted to the world, it is an operant intentionality as was defined by Husserl (1954) and Merleau-Ponty (1945). In *operant intentionality*, world is offered as an amount of practical-behavioral *references*; i.e., in operant intentionality we are given an *operatory context* where relationships or references prevail over the elements characterising the physicalist plane assumed by representational psychologies.

Thus, all phenomenonic experience implies in fact, a context with a referential texture; it is thus necessary to analyse in more detail this referential texture or suture of the phenomenological plane which necessarily structures the psychological field. In our opinion, this suture has a particular contingencial nature which we will express by the notion of '*operatory imminence*' (Robles, 1991b). In order to clearly understand the nature of this notion, some of the arguments developed by Aron Gurwitsch in his well-known book *The Field of Consciousness* (Gurwitsch, 1979) will be used. Gurwitsch considered that every certain experience *overlaps itself*, as it necessarily refers to a modulate system of possible experiences. Thus, for instance, perception as a particular presence of a 'material' thing referentially expresses other aspects of the thing that are not in fact 'present'. Therefore, we can state that experience actually and necessarily structures a configuration of *simultaneity of presence and absence*. It is this configuration of presence-absence what is being specifically referred to by the term *imminence*.

Now, this notion of imminence is close the praxes implied in the experiences it reveals; hence, the imminence adheres to the related operations or activities of each situation. The different modulations of the imminences interrelated within action - operatory imminence- make up a situation as an articulated whole that is *characterised by a non-representational behavioral meaning or sense*.

Now, this phenomenological plane structuring the psychological field is the plane towards which Skinner's radical behaviorism is inclined in practice. In Skinner's praxis we find a manipulation of the psychological field through the control of the environment; it is thus contingencial to this field. That is, the behavioral control of the behavior of others performed by radical behaviorism is carried out through the manipulation of operatory imminences that suture the psychological field.

When adopting and *puting into practice* this phenomenological perspective, radical behaviorism achieves a real reconstruction of the behavioral contents of the psychological field, since it follows the *continuity of sense* revealed in the presence-absence that is inherent to the psychological field. Therefore, radical behaviorism can explain the actual behavior of an organism insofar as it can rebuild this behavior through the course of operatory imminences to which the organism has been exposed. Because of this, moreover, it can behaviorally modify the behavior of an organism insofar as it controls, by means of an adequate course of operatory imminences - phenomenonic experiences interrelated with action-, the behavioral perspective of the organism itself. Modifying a behavior is, in short, to establish a new behavioral horizon, linking the action of the organism to a new field of behavior which, however, does not offer ruptures regarding the previous behavioral field. Therefore, Skinner's archetypical model of '*puting an organism in situation*' makes use of operatory imminence by means of shaping; this means that there are no elemental unities - stimuli and responses- or any other factors that might explain behavior; the point is, as Skinner says, *to shape* behavior on the basis of effective behavior forming in fact a continuity of sense. It may be seen that behavior is in fact liable to be controlled

insofar as there is a possibility of indetermination -operatory imminence- which structures behavior as a *behavioral continuity of sense* (Robles, 1991b). This is the possibility on which radical behaviorism operates, insofar as it does not goes beyond the phenomenological plane in which behavior occurs.

Skinner's words below illustrate this main idea: "Operant conditioning shapes behavior as well as a sculptor shapes clay... Nothing ever arises which is basically different from what preceeded it; the final product seems to have a special unity or integrity of design, but we cannot discover the point where, suddenly, this integrity appears... 'raising your head' is not separate unity of behavior. It is not, we could say, a separate packet... However it can easily be demonstrated the continuous connection between this operant behavior and pigeon's general behavior. Actually, it is the base of a practical procedure to establish a complex response... Reinforcing a series of shaping -sucessive approximations- we raise a rare response to a highly probable response. This is an efficient procedure because it *acknowledges and uses the continuous nature of a complex act.*" (Skinner, 1981, 121; authors' italics).

Thus, we maintain that behavioral continuity liable to be used and recognised in operatory imminence provides an access to the phenomenological meaning of the psychological field and, hence, that it allows to develop true psychological explanations. If this is so, as we believe, we must acknowledge that the main question in the radical behavioral argumentation is not wrong. This question has two sides very closely linked together, one positive and another negative or critical. The former holds that it is *necessary and sufficient* to take into account the control of environmental factors in order to explain -produce or build- a behavior; correlatively, the negative or critical consequence that follows is that every possible appeal to any other alleged factors not present in the behavioral dimension (i.e., in the dimension of environmental factors), the control of which forms the base of the construction of behavior, is entirely *unnecessary*. From what has been said here we are thus forced to acknowledge that this is so because the dimension from which the control of environmental factors that construct or shape every behavioral modification is carried out -the production itself- is the phenomenological dimension.

Skinner's negative or critical requirement of not going beyond the behavioral dimension should be understood, then, as a requirement of *not going beyond or remounting (behavioral) phenomena*; the positive reason on which this requirement is based (Skinner presented it as a matter of practical fact) should be understood as an statement of the practical (practical-behavioral) fact that it is from a phenomenological framework that the behavior of others is generated and controllled through our own behavior.

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Emilio Mira y López. Un scientifique espagnol "transterrado" en Amérique Latine

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En 1949, José Gaos écrivait: "Les espagnols, nous ne nous sentons pas expatriés, mais simplement "transterrados". Nous avons fait une nouvelle découverte de l'Amérique. Nous connaissions l'Amérique espagnole, mais comme il est différent de 'vivre' son immensité et sa diversité dans le présent, sa profondeur et sa complexité par le passé et à la fois sa jeunesse, sa fermentation de formation, et pour ces trois choses sa pléthore de possibilités de futur". En 1947, Juan Ramón Jiménez faisait allusion à une expérience similaire, cette fois-ci en utilisant le terme "conterrado" (Abellán, 1989). Emilio Mira y López (1896-1964), psychiatre et psychologue espagnol de grand prestige international, fut également l'un de ces espagnols "transterrados". Pendant un quart de siècle, il va réaliser une intense activité professionnelle en Amérique Latine. Au dire de R. Ardila (1969) ce fut probablement le plus prestigieux psychologue latino-américain.

Au terme de la Guerre Civile, marqué par la défaite militaire de la République, il se produit la plus grande émigration espagnole jamais connue. Une grande partie des scientifiques qui s'étaient formés au prix de tant d'efforts au cours des décennies que précédèrent le soulèvement militaire de 1936, abandonnèrent l'Espagne en même temps quelques six cents mille exiliés. Les professeurs universitaires, les chercheurs et les professionnels qui parvinrent à franchir la frontière française furent internés dans les camps de concentration où ils furent tout d'abord retenus par les autorités françaises; puis, non sans difficultés, ils purent traverser la ligne qui les empêchait de gagner les villes plus peuplées du nord de la France. D'autres embarquèrent dans les ports de Valence et d'Alicante à destination des pays d'Amérique, et tout particulièrement du Mexique. D'autres encore s'échappèrent par des moyens moins conventionnels (passage au Portugal, fragiles embarcations à destination d'Alger, etc...) ou connurent les camps de concentration ou les prisons espagnols avant de pouvoir gagner l'exil (García Camarero, 1976).

Divers pays d'Europe, tels que la France, l'Angleterre, l'Union Soviétique accueillirent les intellectuels et les scientifiques, mais dans la plupart des cas leur séjour européen fut de courte durée, et la grande majorité de nos scientifiques et de nos penseurs partirent finalement à destination des pays d'Amérique latine. L'accueil

réservé aux scientifiques espagnols dans les différents pays latino-américains était dû, en grande partie, à leur prestige international, à leurs activités en tant que conférencier ou enseignant dans ces mêmes pays avant la guerre, ou encore aux liens d'amitié qui les unissaient à des scientifiques d'origine ou à des scientifiques espagnols déjà installés dans ces pays... ou bien était organisé par des institutions spécialement créés à cet effet, tel que le *Comité Técnico de Ayuda a los Españoles en México*.

Nous pouvons distinguer trois étapes dans la vie professionnelle d'E. Mira y López: une première étape de formation et de réalisation professionnelle en Espagne qui va jusqu'en 1939; une seconde de pérégrination internationale qui va de 1939 à 1945; et enfin une étape brésilienne, de 1945 jusqu'à sa mort en 1964 (Ribeiro da Silva, 1964).

E. Mira quitte l'Espagne en 1939, alors que les troupes franquistes avancent sur Barcelone, la cause républicaine, qui était la sienne, était perdue. Il espère trouver du travail en France, mais les autorités françaises le reçoivent avec froideur et le confinent dans un petit village proche de Paris. Il reçoit tout de même le soutien de psychologues français tels que H. Wallon et H. Piéron. Il demande une aide à la Sorbonne pour un projet de recherche qui devait être une première approche de sa grande contribution à la psychologie appliquée: le test psychodiagnostic Miocinétique (PMK) et que synthétise en partie l'expérience qu'il avait acquise lors de la sélection des pilotes de l'armée républicaine ainsi que d'autres études.

Sa demande n'ayant obtenu aucune réponse, et compte tenu de la mauvaise situation financière de la famille, il part pour l'Angleterre, où le Prof. Myers de Londres lui obtient une bourse d'études en tant que "Research Fellow" de la British Society for Protection of Science and Learning. Ceci va lui permettre d'effectuer ses recherches sur le nouveau test au Maudsley Hospital.

Le 12 Octobre 1939 il présentera le résultat de ses travaux à la Royal Society of Medicine dans une communication officielle intitulée: "El psicodiagnóstico Miocinético, un nuevo recurso para detectar las tendencias conativas de la personalidad". L'exposé est publié en 1940 dans la revue *Proceedings of the Royal Society of Medicine*. Le test PMK, qui devait le rendre célèbre dans le monde entier et qui, au dire de Mira lui-même, devait être sa meilleure contribution à la Psychologie, venait de voir le jour.

E. Mira et sa famille restent en Angleterre jusqu'en Novembre 1939, mais c'est de nouveau leur situation économique précaire qui le pousse à accepter l'invitation qui lui est faite de travailler à Buenos Aires. Sa famille embarque sur le "Highland Monarch", à destination de la capitale argentine, tandis que lui part pour New York pour donner des cours et des conférences, et obtenir ainsi les ressources économiques dont il avait tant besoin. C'est sans doute là, la raison pour laquelle Mira multiplie de façon spectaculaire ses activités en tant que conférencier. A titre d'exemple, il suffit de signaler que pour la seule année 1940 on compte jusqu'à 30 conférences, six cours, neuf articles et un livre (Iruela, 1988, p. 131).

1. Les Etats-Unis (1939)

E. Mira arrive à New-York en Novembre 1939, et durant le reste de cette année il va donner des conférences et des cours aux Universités de New-York, Princeton, Harvard, Yale, Chicago et Washington, dans lesquels il aborde la "Psicología y psiquiatría de guerra".

L'expérience acquise par Mira pendant la Guerre Civile suscitait un intérêt tout particulier parmi les américains, c'est d'ailleurs l'un des problèmes qu'il avait déjà abordé lors de son "Psychiatric experience in Spanish war". Les contributions de Mira lors de la première tournée aux Etats-Unis, ainsi que lors de celle qu'il réalisera en 1942, seront d'une grande utilité pour les psychologues de l'armée américaine pendant la Seconde Guerre Mondiale.

2. Cuba (1940)

En Janvier 1940, E. Mira donne une série de conférences à La Havane sur divers sujets traités aux Etats-Unis ainsi que sur d'autres sujets d'orientation professionnelle:

"Psicohigiene de la ira", à l'Université de La Havane,

"La Psicología de la Guerra", à l'Ecole Libre de La Havane,

"Psicoanálisis: su teoría y su práctica", à l'Instituto Hispano-Cubano de Cultura, et

"Qué es la orientación profesional, su finalidad", au Consejo del I.C.M.

3. L'Argentine (1940-1944)

E. Mira se livre à une intense activité en Argentine. Il travaille en tant que conseiller à la clinique "La Chapelle" de Buenos Aires, puisque, n'ayant pas validé son diplôme de psychiatre, il ne peut s'occuper directement des patients. Il enseigne à l'Université de Buenos Aires et collabore avec le Prof. B. Houssay au Laboratoire de Physiologie. Il est invité à prononcer plusieurs conférences et cours dans diverses institutions médicales, psychologiques et pédagogiques, dans tout le pays, mais également au Chili et aux Etats-Unis. C'est ainsi qu'au cours de l'été 1941, il est professeur à l'Université du Chili et qu'en 1942 il accepte l'invitation qui lui est faite et voyage à New York pour participer aux "salmon Lectures", organisées depuis 1931 par l'Académie de Médecine de New York, à la mémoire de l'éminent psychiatre nord-américain Th. W. Salmon. Cette invitation représente un très grand honneur puisque jusqu'alors, seuls trois psychiatres étrangers avaient reçu cette invitation (Ribeiro da Silva, 1964, p. 12)

En Novembre 1942, il prononça la conférence intitulée "Psychiatry in War", qui sera recueillie dans un volume publié par les Editions Norton de New York en 1943. Au cours de ce nouveau séjour en Amérique du Nord, Mira fut également invité par les Universités de Houston, de Nouvelle Orléans, de Yale, de Princeton, le Wassar College, le Bard College, le Bellevue Hospital, l'Institut of Living, la Société Psychiatrique et Neurologique de Philadelphie, le St. Elisabeth Hospital de Washington et l'Ecole de Médecine de l'Aviation de Randolph à prononcer diverses conférences.

En Octobre 1942 il est chargé d'organiser la prévention et le traitement des maladies mentales dans la province de Santa Fe. Il va y rester de Février 1943 à Mars 1944, en tant que directeur de l'Hôpital Psychiatrique, considéré alors comme étant le plus moderne d'Argentine. Son contrat était de deux ans, mais la situation politique se dégrade et Mira pressenti une autre dictature, se démet. Il obtient du travail à Montevideo, où il se rend en Avril 1944.

De nouveau la situation politique obligeait Mira à émigrer, alors que sa situation économique et sociale commençait à se stabiliser. Il faut dire à ce propos que le contrat qui le liait au Ministère de la Santé Publique de la province de Santa Fe lui accordait des conditions très avantageuses (Iruela, 1988, p. 148). Pendant toute cette période, il poursuit ses travaux sur le PMK et publie de nombreux articles sur ce sujet ainsi que sur d'autres sujets dans des revues spécialisées. Nous présentons ci-dessous une liste chronologique des livres publiés:

Problemas psicológicos actuales (1940). Buenos Aires: Ateneo

Manual de Psicoterapia (1941). Buenos Aires: Aniceto López

Psicología del Niño y del Adolescente (1941). Rosario: Ruiz

Manual de Psiquiatría (1943). Buenos Aires: Ateneo. Il s'agit d'une seconde édition du *Manual de Psiquiatría* publié à Barcelone, en 1935, à peine modifié. En 1946, il publie une troisième édition qui n'est en réalité qu'une réimpression, puisqu'elle ne modifie en rien la deuxième. Il est raisonnable de penser que ces deux éditions de son oeuvre répondent davantage à des motifs économiques qu'à des raisons scientifiques d'approfondissement et de mise à jour (Miralles, 1985).

Intantáneas psicológicas (1943). Buenos Aires: Bajel

Fundamentos de psicoanálisis (1943). Buenos Aires: Americales

Psychiatry in war (1943). New York: Norton. Traduit en espagnol: *La Psiquiatría en la Guerra*. Buenos Aires: Médico Quirúrgica, 1944.

Higiene mental del mundo de postguerra (1945). Buenos Aires: Mundo Atlántico

4. L'Uruguay (1944-1945)

Du premier Avril 1944 au 30 Septembre 1945, Mira séjourne en Uruguay, engagé par le Ministère de l'Education pour faire des recherches sur le normotype de l'écolier et de l'adolescent uruguayen. Il s'agit là d'un projet complexe qui englobait les aspects médicaux, psychologiques, pédagogiques et sociaux d'une vaste population. Le *Boletín de Psicopedagogía "Sebastián Morey Otero"* publia l'étude dans un volume de 630 pages intitulé "Investigación de los normotipos de los escolares uruguayos".

En Uruguay, Mira poursuit également ses activités en tant que professeur et conférencier. Mais l'élément biographique le plus important de cette époque est sans aucun doute sa rencontre avec Alice Galland, avec laquelle il se mariera après s'être séparé de sa première femme, Pilar Campins. Alice Galland était infirmière diplômée à Genève et avait suivi un cours de "Public Health Nurse" à l'Université de Toronto.

Elle avait travaillé à l'organisation de la première Ecole d'Infirmières de l'Université de Rosario de Santa Fe. Vu la situation politique en Argentine, elle renonce à son poste et elle est invitée par le Servicio Cooperativo Interamericano de Salud Pública d'Uruguay à organiser la formation technique des premières équipes d'infirmières sanitaires qui devaient travailler dans les centres de santé récemment créés. Alice Galland décrit en détail sa rencontre avec Mira (1974). Il s'établit ainsi une relation personnelle qui les amènera à se marier à México en 1945, et sur le plan professionnel, cette amènera Alice Galland à se spécialiser dans le test PMK, auquel elle apportera d'intéressantes contributions.

5. Le Brésil (1945-1964)

En 1945, Mira est invité à pronocer une série de conférences dans diverses institutions de Sao Paulo et de Rio de Janeiro, sur la santé, l'hygiène mentale et l'orientation professionnelle, qui suscitent un énorme intérêt dans les milieux spécialisés. C'est la raison pour laquelle, un mois après son retour à Montevideo, il est de nouveau invité par le Departamento Administrativo del Servicio Público pour donner, pendant un an, un cours sur la sélection, l'orientation et la réadaptation professionnelle et les problèmes qui s'y rattachent. Le cours qui commence le 10 Octobre 1945 eut une profonde influence sur les professionnels et les étudiants.

Pendant près de vingt ans, E. Mira va être le grand animateur, l'organisateur et le représentant de la psychologie brésilienne en sa qualité de directeur de l'ISOP, Instituto de Selección y Orientación Profesional (Lourenço Filho, 1969; Seminerio, 1973, 1978). Cette institution s'occupait de divers domaines de la psychologie expérimentale et appliquée, en particulier de la psychologie éducative et de la psychologie du travail; elle conseillait des organismes, organisait des cours et des conférences pour les professionnels et prêtait des services d'orientation et de sélection bien au delà des frontières brésiliennes. C'est ainsi qu'en 1948, il organise un cours de formation de psychotechniciens et que des représentants de la plupart des pays d'Amérique du Sud et d'Amérique centrale y participent.

En 1949, la revue *Arquivos Brasileiros de Psicotécnica* est fondée en tant qu'organe officiel de l'ISOP, elle devient bientôt la revue la mieux qualifiée et la plus autorisée de la psychologie brésilienne. Mira constitue également, en collaboration avec Lourenço Filho, l'Asociación Brasileña de Psicotécnica (Lourenço Filho, 1969). Cette association va jouer un rôle capital pour la psychologie brésilienne, et, grâce à son influence, la loi 4.119 qui réglemente la formation et la profession de psychologue, loi pionnière en Amérique latine, est promulguée le 27 Aouût 1962 (Ardila, 1969, 1971).

En 1959, Mira est secrétaire général du Sexto Congreso Interamericano de Psicología, qui a lieu à Rio de Janeiro sous les auspices de la Sociedad Interamericana de Psicología y la Asociación Brasileña de Psicología Aplicada. Le congrès constitue la reconnaissance internationale du développement atteint par la psychologie brésilienne. Il faut également signaler que Mira participe à plus de 10 congrès nationaux et

internationaux, en Europe et en Amérique, comme représentant le mieux qualifié du Brésil.

Pendant cette étape brésilienne, E. Mira exerce une influence considérable sur la psychologie au Vénézuéla, il contribue à l'organisation de l'Escuela de Psicología de l'Université Centrale, et donne des conférences et des cours dans divers centres. Il nous est impossible de citer ici le grand nombre d'articles (118), de conférences (200) et de cours (120) correspondant à toutes ces années (Galland de Mira, 1964). Nous mentionnerons ci-dessous les livres par ordre chronologique.

Manual de Orientación Profesional (1947). Buenos Aires: Kapelusz.

El niño que no aprende (1947). Buenos Aires: Kapelusz.

Cuatro Gigantes del Alma (1947). Buenos Aires: Ateneo.

Psiquiatría Básica (1948). Buenos Aires: Ateneo.

Cómo estudiar y cómo aprender (1948). Buenos Aires: Kapelusz.

Psicología Militar (1950). Rio: Biblioteca del Ejército

Le Psychodiagnostic Miocinétique (1951). Paris: Centre de Psychologie Appliquée.

Trad. *Psicodiagnóstico Miokinético (PMK)*. Buenos Aires: Paidós, 1957.

Manual de Psicotécnica (1953). Rio: Científica.

Psicología Experimental (1953). Buenos Aires: Kapelusz.

Guía de Salud Mental (1956). Buenos Aires: Oberon.

Compendio de Psiquiatría (1958). Buenos Aires: Ateneo.

Factores Psicológicos de la Productividad (1961). Buenos Aires: Ateneo.

Hacia una vejez joven (1961). Buenos Aires: Kapelusz.

La mente enferma (1962). Montevideo: Roche.

Las vocaciones (1963). Buenos Aires: Siglo XX.

Doctrinas psicoanalíticas (1963). Buenos Aires: Ateneo.

Psicología de la vida moderna (1963). Buenos Aires: Ateneo.

Emilio Mira y López fut psychiatre, psychothérapeute, psychologue, professeur, conseiller professionnel. Il connut la souffrance humaine dans les salles de psychothérapie et vécut dans sa propre chair la réalité inhumaine d'un camp de concentration. C'est sans doute la raison pour laquelle il s'enthousiasma à ce point, vers la fin de sa vie, pour le courant existentialiste de la psychothérapie. Cependant il maintiendra toujours une position éclectique par rapport aux diverses idéologies psychologiques. Mira y López pouvait parfaitement ressentir la Philosophie existentielle, celle qui sait exister, qui sait aimer la vie, qui sait apprendre à aimer la vie (Ribeiro da Silva, 1964, 16).

De toutes ses secondes patries, le Brésil fut sans aucun doute la plus chérie. C'est là qu'il refit sa vie et fonda une seconde famille. Alice Galland écrit: "A sa mort, mes enfants et moi, nous avons recouvert son cercueil de trois drapeaux. Sur la partie supérieure, le drapeau de la République espagnole, qui symbolise les pensées et les sentiments de Mira; sur le corps, le drapeau du Brésil, apys dans lequel il résida pendant 20 ans, après une longue pérégrination, et où naquirent nos quatre enfants, et aux pieds, le drapeau de Cuba, où il était né. Les drapeaux étaient le symbole de

l'universalité, de l'oecuménisme, de l'envergure et de l'éclectisme de toute la vie d'Emilio" (Iruela, 1988, 174).

Dans le titre, nous avons mentionné le terme "transterrado". E. Mira utilise un concept à peu près semblable, lorsqu'il écrit: "Beaucoup d'entre nous savent combien il est douloureux de se voir éloigner de sa patrie et de devoir recommencer à vivre dans des conditions déprimantes et adverses. Mais, pour ce genre de malheur, il est bon d'appliquer la célèbre phrase: 'Wherever is an Englishman, there is England', c'est à dire: 'Là où il y a un Anglais, l'Angleterre est là'; en effet, celui qui aime vraiment ce doux rassemblement de souvenirs, les emporte avec lui et, par conséquent, impose ses caractéristiques là où il pose le pied... C'est pourquoi la détresse de l'expatrié, du réfugié ou du désolé a un remède simple: sa consolation réside précisément sur son nouveau sol, sur lequel il doit transplanter sa personnalité entière" (Mira, 1970, 100).

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Biological determinism and social determinism in early 20th century U.S. psychology

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ABSTRACT

The radical environmentalism of Watsonian behaviourism co-existed at the beginning of 20th century in the U.S. with hereditarian theories, which defended a genetic determinism of human nature. The heredity-environment debate was based upon the background of the socio-political implications of each position.

Contrary to the opinion that extreme environmentalism meant a reaction against the ideology of hereditarianism, I will maintain that, at least as far as Watson's behaviourism is concerned, there was not such a reaction in an important sense because both determinisms responded to the technical needs dictated by their social context, favouring at the same time the process of professionalization of psychology. Although both theories adapted their methodological lines of research to different "technical imperatives", thus making room for their theoretical divergencies, the similarity of their structure and function can be identified by a number of significative signs common to both, such as reductionism, scientifism, the importance given to their technological components or the placement of the causes of human behaviour beyond the control of the individual.

At the beginning of the 20th century an intensive development of psychology took place in the U.S. The origin of behaviourism has been placed in 1913, which is also the year when Goddard is invited to Ellis Island in order to control the entry of European immigrants, with the consequence of a rise in deportation of 350%. Radical hereditarianism, embodied in mental tests fever, the proposals of eugenic measures and the restriction of immigration in order to promote improvements in social conditions, with defenders like Goddard, Terman, Yerkes, Brigham..., co-existed with an equally radical environmentalism defended by J.B. Watson and his behaviourist psychology.

These two apparently opposing ways of understanding human nature were developed in North America at the beginning of the century. Their divergency does not seem to lie only in the contents of their theories, but also in the socio-political consequences implied by each one of them.

On the basis of historical, philosophical and pragmatic reasons, contemporary philosophers of science (e.g. Longino, 1990; López Cerezo & Luján López, 1989: chap. 2, etc.) argue that scientific theories (their origins, as well as their contents and technological components) cannot be adequately understood without taking into account the historical and sociopolitical context in which they take place.

Thus, the fact that the biological determinism of hereditarian doctrines and the environmental determinism of classical behaviourism, two contrasting theories, belong to the same sociocultural and ideological context, should have some type of implication. The most obvious would be that both come up against the same problems which appeared in the U.S. at the beginning of the century with different political ideologies, therefore offering opposing solutions. This is the conclusion that can be derived from Nicholas Pastore's study (1949), who found an "almost perfect" correlation between the point of view sustained in the nature-nurture controversy and the political ideas of the 24 scientists of the early 20th century who he analysed. Geneticists, except in the case of Terman, normally adopted conservative points of view, whereas environmentalists, apart from J.B. Watson, held more "liberal" or "radical" positions. The emphasis on environmental determination could therefore mean a decisive reaction against dominant geneticism. Logically, hereditarians rejected environmentalist doctrines: McDougall's accusation against Watson and behaviourists of being "Bolshevists" (quoted in Pastore, 1949: 173) would not be more than a mere example of the revolutionary potential which environmental doctrines contained as motors of social change. Nevertheless, there are other ways in which both theories may be related to their context and which reveal greater similarities between them than those that are usually pointed out.

López Cerezo and Luján López (1989: chap.2) introduce a model of scientific development according to which the contents of theories are conditioned, to a great extent and indirectly, by the ideological context in which they arise.

This context generates definite *technical imperatives*, that can be defined as "the normative concretion of economic and political interests in technical guidelines for the manipulation of the social or natural environment" (López Cerezo & Luján López, 1989: 195). These technical imperatives determine the convenient methodological selection for scientific research that tries to respond to them. The methodology chosen this way will directly influence the contents of the resulting theories.

Basing myself upon this approach, I will defend the thesis that hereditarian theories as well as behaviourism tend to favour the same ideology, but on adapting to different technical imperatives -although dictated by similar needs or interests-, the two have developed apparently opposing methodologies which give rise to incompatible theories that, nevertheless, converge on their social function.

At the beginning of the century in the U.S. the great boom of psychology was partly due to the necessity for solving the problems caused by the rapid economic and industrial growth. The generalised faith in scientific progress made society turn to

science to find the solutions to the problems which science, itself, had originated. And one of the greatest difficulties which American society confronted was accelerated demographic growth with its consequent problems of poverty, unemployment and the need to rationalize the abundant "human resources" available. Psychology was directed towards applied areas: education, work, publicity... (vid. O'Donnell, 1985).

The hereditarian theory was successful in this context because it supplied a marvellous excuse for justifying the existing social order, offering a "scientific" explanation of inequality of wealth, culture, occupation, etc. (vid. Gould, 1984; López Cerezo & Luján López, 1989; Samelson, 1979). Each person occupies his or her corresponding place according to the intelligence genetically transmitted by his or her parents. Besides, the biological determinism of these hereditarian doctrines was armed with a valuable instrument: mental tests, legitimating the proposal of application of "social technologies" such as eugenics, or the optimization of human resources (vid. López Cerezo & Luján López, 1989: 176-183). However, the hereditarian theory not only offered an explanation to intelligence, also to a lot of "interesting human types of behaviour" (to use Gould's expression), due to the fact that intelligence was related to morality or social value (vid. *Kallikak's* story in Gould, 1984). A really useful theory... for those who were interested in the status quo.

The behaviourist theory presented by Watson did not really deviate in the objectives sought by the hereditarians. Watson explicitly declared that what interested him was not to understand human behaviour, but its control and prediction. Behaviourism also supplied an easy explanation to human problems: these arose due to people's maladjustment to their environment. The solution could be tackled in two different ways: transforming the environment or transforming the individual (Watson, 1913: 159). The latter was habitually chosen, for obvious reasons. In the world shaped by the "behaviouristic freedom" (Watson, 1924: 304) that Watson conceived in "Should a child have more than one mother?" (1929), there are no problems of adaptation because society is ruled by behaviourist scientists who make the existence of government useless, since they succeed, thanks to their methods, where political leaders fail (vid. Morawski, 1982).

Behaviourism responded to the imperative of controlling, foreseeing and modifying individual human behaviour; the hereditarian theories of intelligence responded to the imperative of "reaching an educative stratification which would promote social stagnancy" (López Cerezo & Luján López, 1989: 79), and this is merely another way of assuring individuals adapt to social order. When adjusting to these two types of "social needs", both theories differ in their methodological choices. The hereditarian theories were constructed upon the proposal of the irrelevance of environmental variables, and with a methodology which responded to the differences between groups, using I.Q. tests as instruments and designing a type of experiment with well-defined theoretical expectations. On the other hand, Watsonian behaviourism assumed the irrelevance of "internal" variables and assembled its theoretical network upon the study of the universals of behaviour, adopting experimental designs and

proceedings of comparative psychology. These different methodological engagements generated theories with opposing contents.

Nevertheless, although their methodological principles differed, they were analogous in their function and structure, in the same way as their technological imperatives were. Thus, four basic points may be highlighted in which, regardless of their evident divergences, both explanations of human nature converge. In these intersections their respective dependencies on the ideological and sociopolitical context in which they were developed (and on its technical necessities) are shown:

- (i) The interest of turning psychology into an “authentically scientific” discipline (Watson, 1913: 169). The representatives of the hereditarian theory adopted the methods of quantification, statistics, etc. Watsonian behaviourism, on the other hand, looked for “scientificity” making the “observable” the only object of psychological science. Watson’s environmentalism did not react against geneticism and its ideology, but rather against introspectionism and the whole philosophical burden of mentalist psychology. Science was what society demanded and science was what both Watson and Goddard offered.
- (ii) The quest for immediate and applicable practical results. Society not only demanded “science”, but also “applicable science”. Mental tests demonstrated their “applicability” in World War I and behaviourism did the same, perhaps, with the success of Watson’s job in the advertising agency called J. Walter Thompson, although from 1913 he had been trying to “sell” behaviourism through “advertising” the benefits its use and therefore its “utility” may involve (Watson, 1913: 168). Watson presented both behaviourism and test psychology as “applicable science” but not as “applied science”. To call upon “pure science” meant to justify at the same time its affirmations and its use.
- (iii) Reductionism: technological and political goals, and consequently technical imperatives promoting outer control, held sway exclusively over epistemic utilities as explanatory power or empirical adequacy.
- (iv) Both theories placed the causes of human behaviour “outside” the individuals. “Outside” because they were not controlled by them, since the causes were as much “external” (in the case of behaviourism) as “internal” (in the case of hereditarianism). By creating an individual incapable of being “master of his destiny”, both theories transferred that power to “foreign” factors or instances, making intervention possible intervention through social technologies -as eugenics or behaviour modification-, which adapted individuals to social necessities. Thus, the professionalization of psychology (favoured by the demonstration of its usefulness) facilitated the state control by supplying the “scientificity” needed to legitimate “technocratic” practices.

The four points mentioned above are clearly interrelated. The keys of convergence of both determinisms lie, on the one hand, the demand of North American society at the beginning of the century for quick and effective applied sciences, and on the other hand, in the need to maintain a social order in which each individual occupies the place

he or she has been assigned to within the hierarchical and bureaucratic structure, always to the benefit of economic and industrial development—or, in other words, “progress”. As Samelson says (1979: 155), both theories tried to adequate individuals to their social roles, justifying and legitimizing their manipulation and control.

Nicholas Pastore’s analysis (1949) of political engagements and theoretical positions of scientists implied in the heredity-environment controversy at the beginning of the century, seems to be an important obstacle to the interpretation expressed here about the meaning of the radical environmentalism of Watsonian behaviourism. Nevertheless, the fact that Watson is the only environmentalist that Pastore identifies as conservative may not be pure coincidence. Watson is also the only radical behaviourist of the scientists analysed by Pastore. The Watsonian position did not represent, at least not consciously, any “revolutionary” alternative for the transformation of social order. To the contrary, in his “political utopia”, Watson (1929) conceived his behaviourist scientists as taking care to maintain the most traditional social norms (vid. Morawski, 1982). On the other hand, for the majority of the psychologists who Pastore qualifies as environmentalists (e.g. James Mckeen Cattell, W.C. Bagley, F.N. Freeman, G.D. Stoddard), the emphasis on environmental factors clearly means a rejection of the social implications of hereditarianism, and none of them sustained the social determinism of Watsonian behaviourism.

This convergence in social function and in the form that research in both fields adopted, also became a convergence in contents as time went by. The radical biologicism of the psychology of mental tests relaxed in the thirties. Now, contemporary hereditarians defend the existence of a 20% or 30% of environmental determination. On the other hand, Skinner, taking over from Watson, call on what he named *survival contingencies* to account for those types of behaviour that his environmental determinism left unexplained. According to Skinner human beings are, after all, biological beings. Both determinisms continue interested in “how much” heredity and “how much” environment we are made up of. Contrary to this, a really interesting distinction does not seem to be “biological determinism vs. social determinism” or “determinism of any kind vs. human singularity”, but rather that pointed out by S. J. Gould (1977): “determinism vs. human potentiality”.

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The development of schoolpsychology in the Netherlands

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INTRODUCTION

In the 1950s schoolpsychology became a new psychological profession in the Netherlands. Diagnosing children and advising schools and parents with respect to allocation, developing treatment programs, therapy and schoolguidance, became some of the subject matters of newly developed schoolpsychology (Van den Broek, 1964).

In the Netherlands schoolpsychology developed rather late, compared internationally. According to an international survey by UNESCO (1948) schoolpsychologists did not exist in the Netherlands at that time. This in contrast with countries such as the United States, Britain, Sweden, Denmark and Switzerland (Wiegersma, 1958).

But nationally too, its late development is in contrast with the early introduction of psychology within trade and industry, where Dutch psychologists had been able to create a significant niche for themselves during the 1920s and 1930s.

Within the field of education, however, developments seemed less clear-cut and roles of psychologists less exclusive than in trade and industry. Schoolpsychology operated on the verge of other cognate disciplines, such as social medicine, pedagogics and child-psychiatry.

In this paper two questions are posed: a) why were psychologists hardly able to practice their testing on a large scale within the Dutch schoolsystem in the first half of the 20th century; and, b) why could psychologists not make exclusive claims on testing and diagnosing schoolchildren after the Second World War? In this paper some tentative answers to these questions are given.

Schooldoctors as psychologists

During the process of industrialization from the second half of the 19th century onwards, the child became the object of political and scientific debate. Main issues were the upbringing of children and school education. The school should educate

children to become diligent and obedient subjects (Mulder, 1982). After the enforcement of the Compulsory Education Act in 1901, the diversity within the schoolsystem grew for both 'normal' and 'abnormal' children. This caused serious trouble for the struggle for unification of the system.

This lack of unity called for political measures. Although these problems were acknowledged by the Government and in spite of the work of half a dozen state committees for educational reform, politics refrained from taking measures, afraid that the 'school-struggle' about educational liberty and equal funding of non-denominational state schools and religiously orientated private schools within the Dutch pillarized society would flare up again (Bolkestein & Menkveld, 1978; Dodde, 1981).

Two problems were acknowledged. The first was the allocation of 'abnormal' children into the special education system. The second was the poor connection between primary and secondary education. It was the allocation and the connection problem which became the concern of city-councils, school-doctors, psychologists and pedagogues. Several reports were published on the subjects mentioned (Révész, 1926; Van Veen & Kohnstamm, 1928; Bolkestein e.a., 1935).¹

From the end of the 19th century onwards, school-doctors got interested in the physical and mental condition of the child (Nicolai, 1988). Contagious diseases, which occurred frequently in those days, were a constant threat for public health in general, and for the children attending the primary state schools for the poor, particularly. Because the Government had ordained compulsory education, doctors and some teachers as well held public authorities responsible for the health of the children. The advocates of social hygiene, i.e. doctors, teachers and public authorities, argued in favour of the appointment of school-doctors.

In spite of broad programmatic ideals the daily practice of school-doctors consisted of periodical medical examinations for the early recognition of contagious diseases and other physical and mental aberrations, such as feeble-mindedness. Crude standards on healthy versus unhealthy, and mentally normal versus abnormal, were developed.

To give a firm basis for the study of and discussion about schoolsurveillance, the medical and mental care of schoolchildren, a small group of school-doctors set up the 'Dutch Association for School-doctors' in 1903. The newly established association also was to defend the interests of the new profession. A journal was founded to stimulate discussion on various medico-educational topics.

For the final decision to admit the child to the most suited type of special education and the admittance into the secondary school special attention was given to the intellectual capacities of the child. Therefore, adaptations of the Binet by the Dutch schooldoctor, J. Herderscheê, and the American Army alpha tests became important tools for the schooldoctors. With the psychological test school-doctors were able to enrich their rather dull and uninspiring occupation of periodical medical examinations. Offering solutions to serious educational problems, it enhanced the social

prestige of the profession. For a long time the medical profession kept a strong foothold mainly in the allocation of backward children within the special education system by psychological and physical examination, and, to a lesser degree, the psychological assessment of normal children for further education. In fact, (school)-doctors were the first psychologists in the Netherlands who used the psychological test as a scientific tool.

In spite of the strong position of physicians, this did not mean that psychologists were not interested in issues related to school and education before the Second World War. Although on a modest scale, professional psychologists were enlisted by both, school organizations and parents to test children for the admittance to special and secondary education facilities. Some close, enduring relations were built up between psychologists and schools. Often these relationships were of a pillarized nature. For example between the Catholic deaf institute in Sint Michelsgestel and psychologists of the Catholic University of Nijmegen,² and between Protestant schools and the psycho-technical laboratory of the Protestant Free University in Amsterdam. Though psychologists were active in the educational field schoolpsychology as a special discipline still did not exist.

Schoolpsychology after the Second World War

In accordance with the ideas of the 'Mental Health Movement' and the rising popularity of Social Medicine as a medical specialism, attention shifted from the 'negative' (i.e. curing and preventing diseases) to the more 'positive' (i.e. promotion of the health of the human being). Ideas of the 'Mental Hygiene Movement' and Social Medicine were rooted already in the 1920s. In the climate of economic and social recovery and the building of the welfare-state after the German occupation, ideas on mental health got more popular in the Netherlands and were taken up by pedagogues and psychologists and other intellectuals.

The human being was considered a psycho-somatic unity, embedded within social life. When the social environment was rewarding, a physically and mentally healthy child would normally become a healthy adult.³ As a part of public health care these ideas on mental health were essential for the reorganization of the school-health-care-services and the forthcoming school-guidance-services. Amongst others, scholars such as physicians, pedagogues and psychologists worried about the bad mental condition of the population after the war. Within the more complex postwar society, traditional standards of the upbringing of children were challenged. A mentally healthy social education was held to be crucial for the formation of loyal democratic and responsible citizens (Nieuwenhuis, 1954a; 1954b). The school and the school-health-care-system should strongly contribute to this endeavour.

After the Second World War the school-health-care-system was still splintered. Municipal Health Organizations ('GG & GD's), municipal education inspections and religiously inspired Cross Societies for Family Care provided the health care for

schoolchildren. The already existing Pedological Institutes and the Medico Pedagogical Bureaus were also part of this 'system'. It was within these organizations in which schoolpsychological work was started and service departments were formed.

In the organizations mentioned above physicians traditionally held the main positions. Psychologists, pedagogues and school social workers only gradually got affiliated with these organizations. Towards the end of the 1950s and the beginning of the 1960s several reports and counter-reports were published to improve the school-health-care-system.⁴ Attempts were made to define the role of the state, to delimitate professional roles and to reorganize the infrastructure (Wilmink, 1967). Because so many disciplines were involved already, teamwork, the idealistic programme of the Mental Health Movement, was considered necessary and inevitable to improve the physical and mental health of schoolchildren. In spite of many different points of view, it was generally considered that the role of the school-doctor as a 'psychologist' came to an end. He had to confine himself to the purely medical examination of schoolchildren again. In spite of his regret he felt that professional restriction was necessary because of the enormous case-load. Moreover, schooldoctors for the first time had to cope with the problems connected with working in a team in which they had to co-operate with equally educated colleagues.

On the other hand, both psychologists and somewhat later, pedagogues, had to operate very carefully within these organizations, which were still dominated by medical people. The only way these newcomers were able to start their work was in co-operation with the medical staff (Wilmink. o.c.).

For the optimalization of the school-careers of children new forms of schoolassistance were established in the 1960s. Multi-disciplinary School Guidance Services should contribute to the innovation of education, which became an important political issue in the 1970s.⁵ School-doctors were excluded from these services. Psychologists and pedagogues became the most important figures within these services. They had to contribute to the psychological and educational well-being of the schoolchildren with the help of psychological diagnoses and, if necessary, therapy.

Meanwhile, the breakthrough of schoolpsychology, or at least its official recognition, was realised by a Royal Decree in 1949. This coincided with the extension of the special education system with a new schooltype: the 'schools for children with learning and educational problems'. These new schools were created for a large group of children who were not mentally defective, nor feeble-minded, but whose learning disabilities and behaviour caused serious trouble for the normal education system. Supported by the Dutch Association of Psychologists and the well-known female pedagogue W. Bladergroen, it was the catholic Minister of Education and psychologist himself, F.J.Th. Rutten, who introduced a bill, ordaining that the decision to admit a child to these special education facilities should be based on sound psychological findings of a psychologist, together with the opinion of the headmaster and the schooldoctor.

Against this background of the reformation of the school-health-care-system, the development of the school-guidance-services and the official governmental recognition of schoolpsychology, schoolpsychologists, in an attempt to elucidate their work, claimed their share in the enterprise. Besides newly written books and pamphlets, articles were published in scientific and popular journals (Van Gelder, 1953; NIPP, 1964; Wiegersma, o.c.). In compliance with the mental health perspective, the prevention of future troubles for children and their parents was an important issue in these publications. The early tracing of children with possible psychological disturbances by means of periodical examination of the whole schoolpopulation was idealistically appreciated as the most effective way to contribute to the mental health of children. This would enable psychologists to be independent of possibly resistant teachers in applying pupils for testing. An ideal, by the way, which was never realized.

In spite of the different perspectives of the authors, who would be either pedagogically or psychologically orientated, the general idea was that psychological diagnoses was the main task of the schoolpsychologist, though pedagogues challenged the position of psychologists on this field. From 1967 onwards a pedagogue with a special test endorsement was officially competent for psychological testing.

EPILOGUE

Up to the late 1940s a lot of 'school-psychological' work was carried out by school-doctors to solve allocational problems, in particular in the field of special education. Because of the firm position of schooldoctors psychologists were hardly able to enter the educational system structurally, in spite of all efforts they made. After the war they succeeded better in entering the schoolsystem by means of the institutionalization of the school-health-care- and the school-guidance-services. For the medical profession it meant that it lost its psychological territory. Because of the demands of co-operation with competing cognate disciplines, such as pedagogues, schoolpsychologists were not able to create an exclusive niche for themselves and their expertise.

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NOTES

- ¹ See also the report of the municipal council of The Hague: *De aansluiting van lager en voortgezet onderwijs* (1928). Verslag van de commissie van onderzoek en advies inzake de aansluiting van lager en voortgezet onderwijs, benoemd door burgemeester en wethouders van 's-Gravenhage, bij besluit van 4 november 1924, nr. 25563. Groningen/Den Haag: J.B. Wolters.
- ² Interview with J.Th. Snijders and N. Snijders-Oomen 6 May 1991 on tape. In: Archives of the Foundation for Historic Psychological Materials (HMP), Groningen.
- ³ See S.M.C. van Veen (1954) *Ontwikkeling en vernieuwing van de sociale kinderhygiëne*. Assen: Van Gorcum and J.T. Buma (1954) *De sociale kinderhygiëne in Nederland - Haar organisatie en haar toekomst*. Assen: Van Gorcum.
- ⁴ See for example the catholic report: *Schoolgezondheidszorg. Rapport van de commissie-Souren* (1958) 's-Gravenhage: Centrum voor Staatkundige Vorming. See also the *Interim-rapport schoolgeneeskundige diensten* (1961) and other governmental reports, and articles in several Dutch journals on social medicine and mental health care, such as the *Tijdschrift voor Sociale Geneeskunde* and the *Maandblad voor Geestelijke Gezondheid*, mentioned by Wilmink (1967).
- ⁵ The socialist Minister of Education, J.A. van Kemenade, published a report in order to stimulate discussion: see the *Discussienota Schoolbegeleiding* (1975) 's-Gravenhage: Staatsuitgeverij.

Historical foundations of the computational theory of mind

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ABSTRACT

In this paper it is suggested that there is a strong historical continuity between the 17th century study of mind and modern 'conventional' cognitive science, as exemplified in the computational theory of mind (CTM).

First Laudan's model of science, in which the concept of a research tradition plays a prominent part, is reviewed, followed by a short survey of the main characteristics of the CTM. The basic idea of the computational theory of mind is that the mind is a computational information processing system. Ontologically, symbols are posited referring to things in the external world and part of a structure that is of causal importance to the behaviour of the organism containing them. These symbols interact in a formal way. Methodologically the mind can be studied independently of its relations with its surroundings and/or its physical basis.

These basic assumptions are first formulated in the 17th century. Descartes first postulated the mind as an inner domain of ideas. Conceptual work by several theorists gradually led to an interpretation of ideas that is comparable to the modern interpretation of symbols with respect to their structure, their function, and the way the mind processes them. Methodologically and empirically some interesting parallels are indicated.

In conclusion it is suggested that as a consequence of this 'heritage' of the CTM, those who adhere to it have difficulties in understanding and respecting the criticisms of those who do not base themselves on this 17th century research tradition. An analysis of the underlying research traditions is necessary in order to understand and constructively evaluate many recent controversies in the study of mind.

In this paper I will pay some attention to the question as to whether the basic assumptions of the symbolic or so-called 'conventional' cognitive science, as expressed in the computational theory of mind

(CTM), can be seen as elaborations of a consistent set of ideas which were developed in the 17th century.

The starting point will be a short review of Laudan's (1977) model of science, followed by an equally short survey of the main characteristics of the CTM. In the main part of this paper I will try to show that there is a strong historical continuity between the 17th century study of mind and conventional cognitive science. The CTM has reopened the kind of conceptual and empirical problem solving in the mental domain that originated in the 17th century. I will use Laudan's notion of research traditions to analyze this continuity. My contention is that in the 17th century a research tradition developed within which modern conventional cognitive science is still working. In conclusion I will suggest that as a consequence of this 'heritage' of the CTM, those who adhere to it have difficulties in understanding and respecting the criticisms of those working in a different tradition. Knowledge of the involved underlying research traditions is necessary for making any substantial progress in these matters.

One more remark might be in order. In this paper, rationalists and empiricists will turn up in an intermingled fashion. I think this is in accordance with the strong connections between thinkers in the 17th century. A simplistic divide between rationalists and empiricists as if the two camps were separated by a great watershed does no justice to the fact that they all worked together within a certain common conceptual frame which I will try to describe below.

LAUDAN'S MODEL OF SCIENCE

In the following I will draw heavily on the concept of research traditions (RT's). According to Laudan (1977) a research tradition is

"a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain."
(Laudan 1977, p.81).

A RT specifies certain guidelines for the development of specific theories. Some of these guidelines provide an ontology which specifies in a general way the fundamental entities in a domain and the ways in which they can interact. Specific theories within a RT solve empirical problems in the domain by 'reducing' them to the ontology of the RT.

The function of a RT is to provide empirical and conceptual means to solve problems, and partly to delineate the problems and their importance in a given domain. Methodologically a RT specifies how theories about the ontologically indicated entities should be constructed and tested.

Laudan's model has some interesting characteristics: First, a RT consists of a family of historically and conceptually related theories, *any two of which can be mutually contradictory*. Second, a RT can *evolve* through the gradual replacement of old assumptions by new ones. There is thus no strict, unchangeable hard core as in Lakatos' (1970) research programmes. Relevant is furthermore that Laudan sees the importance of conceptual work in addition to empirical problemsolving. This conceptual work takes the form of answering questions about the well-foundedness of the conceptual structures which have been devised to solve empirical problems. Below I will concentrate mainly on conceptual matters.

In the following I will describe the ontological and methodological assumptions, and the main conceptual and empirical problems essential to the 17th century and the conventional cognitive approach to the mind. I will, in accordance with Laudan's model, pay no attention to the fact that these assumptions may have played different roles in different theories.

THE COMPUTATIONAL THEORY OF MIND

The basic idea of the computational theory of mind (Simon 1981; Newell 1981; Pylyshyn 1984) is that the mind is an information processing system. Mental life consists of the processing of symbols. The symbols are the basic ontological entities with which theories should explain mental phenomena. Symbols are representational structures, they are about things in the external world and they are part of a structure that is of causal importance to the behaviour of the organism that has them. The representational system has a complex, constituent structure; complex representations are built up out of simple ones in a structured manner.

Another ontologically important notion is that these basic entities interact in a formal way: the human mind can be seen as a formal system. The processing of the symbolic structures takes place according to strict rules that can be studied separately from the medium in which these rules are instantiated. It is the form or syntax of the symbols that makes them accessible to the rule-based operations.

These basic ontological ideas have led to several empirical and conceptual questions. Most profound are the problems regarding the representational and causal powers of symbolic systems. Questions are raised as to whether an artificial computational symbol system could really function in the real world, and (apart from that) whether such an artificial system should be regarded as truly mental. Should we say that it can think? Would it be conscious? Should it be treated as a person?

Methodologically the mental domain is set apart both from the external world and from the underlying physical machinery. It is studied without regard to its surroundings. A major methodological tool is the development of computer programs which try to simulate mental phenomena.

THE 17TH CENTURY

Conceptual matters

Ideas

Although much attention is normally paid to Descartes' ontological dualism, a much more important development he started is the 'way of ideas'. In the workings of his mind-substance ideas played a substantial role. He tried to give the concept 'idea' a new interpretation (away from the scholastic, rather Platonic, use of it), but could not find a single unequivocal explication for it. This shows among other things the many ways in which a mind can relate to its ideas: the mind can form or construct them, it can grasp, find, notice or perceive them, bring them out of its consciousness, ideas can present themselves, and the mind can look at them (Kenny 1967). The conceptual problems surrounding Descartes' new ontological entities dominated the thoughts of the main thinkers of the 17th century.

A very important ambiguity in Descartes' use of the concept 'idea' has led to an interesting controversy between Malebranche and Arnauld. Sometimes Descartes seemed to interpret 'ideas' as acts of the mind, whereas at other times he seemed to mean by them the objects or contents of the mind.

Arnauld adhered to the act version of ideas whereas Malebranche hung on to the object interpretation. It should be noticed however that Malebranche still interpreted ideas in a Platonic way (Lovejoy 1923). These true and, with respect to the essences of things, representative units exist in the mind of god, and are therefore no 'modifications de l'âme'. The effects of objects on our senses cause movements in our brain which lead us to postulate an existing thing as their cause, instead of representing it. There are, therefore, no representations in our minds: god gives us the true ideas or true essences.

Arnauld thought this separate existence of representative units, apart from individual perceptions, to be impossible. He used 'ideas' in the Cartesian sense and took them to be acts of the individual human mind. In short, the Malebranche-Arnauld controversy boiled down to the question whether ideas were mind-independent objects or mind-dependent acts (Cook 1974), but the debate knew no clear results.

Locke combined Malebranche's object interpretation with Arnauld's positioning of ideas in the human mind. He viewed ideas as entities or objects in the mind. As objects in the mind they take the place of external objects which the mind doesn't get into direct contact with (Ryle 1933). Locke tried to give a mechanistic explanation for the workings of our minds in order to justify our claims to knowledge, something he is blamed for by Rorty (1979). The identification of these two different matters can be put down to the wax-tablet metaphor gone astray, and shows that Locke attributed a dual role to ideas: a causal role in the behaviour of the organism and a representational role with respect to the outside world.

Locke is furthermore important for accepting the idea of thinking matter as a possibility (1690, II, xxvii, 17; IV, ii, 6), thereby diminishing the importance of Descartes' ontological dualism.

Another important 17th century project was Leibniz's *characteristica universalis*. Leibniz thought that human reasoning could be seen as the mechanical process of performing combinatorial operations on characters. He tried to develop an alphabet of human thinking, the basic elements of which consisted of simple ideas or first notions. These basic ideas were combined in a methodical way, through a kind of calculating procedure (Spruit & Tamburrini 1990). This is in effect the idea of the constituent structure of symbols that is of crucial importance to conventional cognitive science (Fodor & Pylyshyn 1988).

Locke, by the way, also developed the thought that ideas have a complex constituent structure. Simple ideas are the building blocks out of which complex ones are built (1690, II, xii, 1). He suggested however not a rule-based way of combining the basic ideas into complex ones, as Leibniz did, but an associationist way. Fodor & Pylyshyn (1988) see his line of thinking reflected in modern connectionist models, much to their dismay, for an associationistic way of combining simple elements into complex ones doesn't allow for systematicity and productivity which, according to them, are essential characteristics of human cognition. Conventional computationalists and Leibniz suggest rule-based structuring in opposition to connectionists' and Locke's associationism.

Locke was one of the first to see the semantic problems the way of ideas brought in its wake. According to Locke an utterance derives its meaning from the idea in the mind, of which it is a perceptible sign. The task language has to perform is to transmit ideas from one mind to another (Locke 1690, III, i, 1-2). Locke understood very well that ideas couldn't derive their meaning from a kind of resemblance to objects in the external world (something which Berkeley and Hume failed to appreciate). Instead of this he suggested a kind of covariance theory (1690 III; Cummins 1989). Our simple ideas represent because they covary nomologically with specific events in the outside world. In other words; an external object produces an idea in our minds, and that object is the meaning of the idea. It is precisely this line of thought that has been heavily investigated by recent computationalists like Fodor (1987; 1990) and Dretske (1981).

Formalization

As said above, Laudan explicitly states that a specification of the way basic entities interact is a part of the ontology of a RT. A basic assumption in the CTM is that the interaction between representations is formal. Mental processes take place according to formal rules which apply to representations in virtue of their syntax. The same basic assumption can be found in the 17th century.

For instance Hobbes' famous dictum: "By ratiocination, I mean computation" (Hobbes 1656, I, 3) can be analyzed into two claims: (1) thinking consists of symbolic

operations, thoughts are special tokens in the brain which he called 'phantasms' or 'thought parcels', (2) thinking is most clear when it follows methodical rules. In other words; thinking is a mechanical process, mental units are being pushed back and forth according to certain rules like beads in an abacus (Haugeland 1985).

The same line of thought can be found in Leibniz who strove towards the development of a thinking calculus in which propositional expressions could be manipulated in a formal, truth-preserving manner. He pointed out that the instructions of the calculating procedure can be seen as the prescriptions of operations on symbolic, and not just numerical, expressions in general. He also called attention to the fact that symbolic expressions function through their form instead of their content, with the implication that only minimal intellectual capacities are required to process the symbols.

After Leibniz however things came to a standstill for almost a century and a half. This was due mainly to a lack of mathematical abstraction, which continued until the work of George Boole in the middle of the 19th century (Kneale & Kneale 1962).

Thoughts about formalization were formulated not only with respect to thinking, but with respect to perception as well. Descartes explained perception as a mechanical process of bumping and colliding. External objects cause a shock to sense organs after which this shock is 'transported' into the brain and ultimately, though unexplainable for Descartes, to the mind. According to this view the mind could only notice the conditions its senses were in. In true perception the mind judged, on the basis of the effects an external object had on the senses, which was the cause of these effects it noticed. So, in perception, there are mental operations on the basis of raw sensory material (Reed 1982).

Malebranche took this idea a lot further. He realized that there were unmistakably active judgement-like elements in what he thought were fundamentally passive sensations. He ascribed these active and rational operations to a divine intervention (Malebranche's occasionalism) which also helped him in bridging the Cartesian gap between the body and the mind. It is very interesting however that god's continuous intervention is not at all divinely free, but on the contrary, strictly bound to rules. As Malebranche says:

"God always acts in consequence of the same laws, always according to the rules of geometry and optics, always dependently upon the knowledge of what takes place in our eyes compared with the situation and motion of our bodies, always in consequence of an infinity of instantaneous inferences" (Malebranche 1674, I, 9; 1980 p.47).

In modern terms one could say that god's intervention could be automatized or computerized (Meyering 1989, p.103-104).

In conclusion of this section one can say that the concept of ideas, postulated by a Cartesian ontology, formed the central part of the most important 17th century

theories of mind. The most important conceptual problems centred around what ideas exactly were, how they interacted, how they played a role in thinking and reasoning, how they could represent anything, and how they fulfilled their causal role in the behaviour of the organism. Many different theories were devised to answer these questions, which were all part of one RT.

Methodologically, the mind was studied in a special way, through introspection (Lyons 1986). All through the 17th century the mind was seen as a self-contained (but not necessarily ontologically irreducible), introspectively approachable domain.

EMPIRICAL MATTERS

Not only with respect to conceptual matters are interesting parallels between the 17th century and symbolic cognitive science to be found. In the empirical domain fascinating analogies can be seen as well.

The 20th century saw the rise of powerful 'thinking' machines, but already in the 17th century machines that actually captured at least a little bit of the flavour of the predicate 'thinking' emerged. In 1617 von Napier built a simple mechanical calculating machine made out of ivory. In 1623 Wilhelm Schickard built a digital calculator. Some 20 years later Blaise Pascal produced his calculating machine, which was improved later on by Leibniz. Also, at the turning of the century, machines who could imitate animal and human behaviour were being built and heatedly discussed, e.g. a duck, a flute player, and 'the turk', the first chessplaying machine (albeit based on deceit).

Not surprisingly the 17th century also saw the emergence of an explicit comparison between machines and organisms. Especially clocks and animals figured largely in these collations. Descartes ventured the opinion that animals were machines (1637, 1: 115-117) which led to a heated discussion about the 'bête-machine' (Rosenfield 1940; Draaisma 1990). Within a century humans were included in the comparison. Finally La Mettrie (1748) wrote outright that humans too could be seen as machines. He stated that there was nothing more to the human soul than an intricate organization of the body and brain.

But in this area progress slowly came to a standstill during the 18th century. For although many ingenious mechanical machines were still built in the 18th century, discussions about the implications of these machines for human thinking petered out rather quickly (Draaisma 1990). New developments were only started by Lady Lovelace after Charles Babbage developed his plans for his analytical engine around 1833.

Locke was one of the first to realize that the 17th-century way of ideas led to problems with respect to personal identity, and he dedicated a chapter (1690 II, xxvii) to the problem. After him Hume continued the struggle, but in the end had to accept that the problem seemed unsolvable. He called attention to the fact that talking about

objects in the mind presupposed an observer, which had to be explained in turn and therefore lead to circular reasoning. It was only after the development of computers that a solution to this problem (which proponents of the CTM trace back to the 17th century and has become known as 'Hume's Problem') could be proposed (Dennett 1978).

CONCLUSION

In a rather cursory way I have tried to show that there are important parallels between 17th-century thought and the CTM. The theories of Descartes, Malebranche, Hobbes, Leibniz, Locke and others in the 17th century, *as well as* the 20th-century theory of mind, are all members of one family, belonging to the same research tradition, despite their individual differences.

Ontologically the RT of the 17th century and the CTM can be characterized as positing elementary thought-particles which interact in a formally specifiable way.

Methodologically, introspection was replaced by computer simulation. Yet, the methodological point of studying the mind in itself, and thus abstracting from its surroundings and underlying (physical) basis is crucial for the 17th century as well as for the CTM.

Within this 17th century RT several theories were developed, which show conceptual and empirical similarities with the 20th century CTM.

Conceptually important is the 17th century view that the human mind could be seen as an inner domain of ideas. All through the 17th century one tried to answer questions concerning the dual function of ideas: how ideas could be about things in the external world, and how ideas could have a causal influence on behaviour. The rules according to which ideas are being processed were not thought to be ad hoc or arbitrary, but in principle accessible to formalization. Complex ideas were thought to have a constituent structure.

Empirically important are the comparisons that were made between machines, animals and human beings, and the questions that were raised about whether a system of ideas would do full justice to several 'marks of the mental' (consciousness, self-identity).

All this can be connected to a notable peculiarity of many of the strongest advocates of the CTM. Especially AI-workers tend to be more or less immune to reactions and criticisms that are not formulated from within this 17th century context. As a short example Searle's (1980) is very illustrative. In this article Searle developed his well-known 'chinese room' thought experiment, which shows, according to him, that physical symbol systems like computers cannot truly be called mental, because they miss a certain understanding or consciousness of what is going on. Searle purported to show that solely formal interaction between symbols is not enough for mentality. It is very important in this context that the cogency of Searle's argument

depends largely on the intuitive 'feel' it provokes in its auditors (Dennett called it an 'intuition pump') (Dennett, 1987).

Now Searle can be seen as working in a phenomenological (maybe even Heideggerian) tradition (Dreyfus 1982, McIntyre 1986), and this may very well explain the tone of many reactions from CTM-scientists working within the 17th century tradition. Hofstadter, for instance, said:

"This religious diatribe against AI, masquerading as a serious scientific argument, is one of the wrongest, most infuriating articles I have ever read in my life" (Hofstadter 1980, p.433; see also a.o. Rorty 1982).

That since then the two points of view have not come nearer to each other can be gathered from the fact that John Searle in a recent article states that he thinks his 1980 argument was 'quite decisive' (Searle 1990, p.585), whereas out of the AI-community the same old brusque reactions can be heard, this time through AI-expert McDermott who finds this new article by Searle "wrongheaded from beginning to end" (McDermott 1990, p.617).

Searle tackles in his (1980) article the very foundations of the cognitive RT, the ones that go without saying in their research and articles. Searle questions this very foundation which explains the sometimes vehement reactions to it. This also explains why the controversy can't be solved in any direct way.

Part of the reason for the seeming impossibility to discuss this disagreement in a more constructive manner might stem from the difficulty in discussing one's intuitions. However, if our intuitions about what is mental are indeed nothing more than "our readiness to fall in with a specifically philosophical language game" (Rorty 1979, p.22) than an elucidation of the involved research traditions may prove very helpful. It is to be expected that an analysis of the philosophical traditions underlying the different intuitions in question could clear up a lot of the difficulties proponents of the CTM have when facing fundamental criticisms. It is only after the involved RT's are taken into account that any substantial progress in this matter can be made.

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Psychologie zwischen Moderne und Postmoderne

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ABSTRACT

In my approach to the contemporary discussion on modernity and postmodernity I have tried to find some new motives. My task was to reconstruct the relationship between psychology and modernity/postmodernity, which meant to discuss psychology within a framework not very familiar in its self-reflexion. In the first step I discussed contributions of the modern age to the development of psychology as a science. By promoting the subjectivity principle in the whole social life, the modern age constituted the subject of psychology (N.Elias). On the other side the first methodological equipment of psychology (introspection, experiment) has also its social origin in the conditions of modern life (E.Zilsel). The results of the discussions on psychology and modernity led to a conception of a historical psychology which must follow the transformations in psychological mental and behavioural structures taking place within the context of broader social changes (L.Vygotski, N.Elias). The second theme I discussed by explaining the latent psychological assumptions lying behind postmodern figures - singularity, dissens, paralogy, patchwork etc. (J.-F.Lyotard) The striking similarity between these postmodern figures and the phenomenology of psychopathological experience has been analysed concerning the lack of communicative procedures in both models. By using developmental psychological arguments (J.Piaget) I tried to show the inevitability of a shared experience (the other, language, norm) for the constitution of a possibility to become singularity or to get acceptance for a dissens (J.Habermas) Previous discussions should confirm the following thesis: in the same way as postmodernity is founded in modernity, postmodern psychology is inevitably dependent upon modern psychology - in both cases without being aware of it.

Wenn ich mich heute in die Diskussion über die Postmoderne einschalte, bin ich mir bewusst, einer Modeerscheinung zu folgen, die oft sogar die Züge einer Knechtschaft trägt. Dennoch entspringt gerade diesem Bewusstsein ein Anspruch auf die Rettung

einiger emanzipatorischer Motive, die -so glaube ich- auch diese Herr-Knecht-Beziehung in sich birgt.

Die hier gewählte Perspektive enthält von Anfang an eine Distanzierung gegenüber der Postmoderne zunächst durch die Beibehaltung der Moderne als eines noch gültigen Paradigmas. Diese Zwischenstellung zwischen Moderne und Postmoderne als Diagnose des gegenwärtigen Zustandes ist aber erst eine Hypothese, deren Überprüfung dieser Aufsatz gewidmet ist.

Was die Psychologie betrifft, so ist sie mit dieser Hypothese in eine ihr selbst fremde Gesellschaft hineinversetzt, wobei die Fremdheit an den Selbstreflexionsleistungen der Psychologie gemessen wird, d.h. aus deren Fehlen hervorgeht.

Den begrifflichen Triangel Psychologie, Moderne, Postmoderne -in eine theoretische Beziehung umzuwandeln, das ist eine Aufgabe, die auf die Explikation von impliziten Voraussetzungen und daraus folgend auf die Rekonstruktion sowohl der Theoriegeschichte als auch der Sozialgeschichte der Psychologie hinzielt. Der heuristische Gewinn lässt sich vor allem auf der metatheoretischen Ebene ablesen. Dass die metatheoretische Kompetenz zum Bestandteil des wissenschaftlichen Werkzeuges gehört, ist eine Aussage, die in einem postempiristischen wissenschaftlichen Paradigma wohl schon eine Anerkennung findet.

Nach diesen vorläufigen Bemerkungen methodischer Art begeben sich an die Rekonstruktion der Psychologie innerhalb des Spannungsfeldes Moderne - Postmoderne.

Psychologie - Moderne

Die Aufgabe, auf die Beziehung zwischen Psychologie und Moderne zu reflektieren, ergibt sich schon aus einem einfachen empirischen Grund: aus der Simultaneität -allerdings nicht absoluter, aber doch in einem hohen Masse - der Entwicklung von Psychologie als Wissenschaft und der Moderne als psychosozialer Formation.

Die Moderne wird hier als die neuzeitliche Epoche verstanden - vor allem in Form der neuzeitlichen sozialen Welt und Gedankenwelt. Auf diese Weise nimmt die Moderne ihren Ursprung in jener Umbruchsphase, in der sich die mittelalterliche feudale Welt auflöst und auf deren Boden sich die Keime neuer ökonomischer, sozialer, kultureller Strukturen herauszubilden beginnen - z.B. im Zuge einer Verweltlichung des Lebens entstehen Städte als neue Zentren (statt mittelalterlicher Klöster), zu denen immer mehr Menschen, die ständischen Bindungen durchbrechend, strömen und dort als freie Unternehmer leben, neue Bedürfnisse entwickeln und neue

Verhaltensweisen pflegen. In Folge dieser umfangreichen Umwälzungen werden soziale Bedingungen geschaffen, unter denen der einzelne Mensch als Subjekt hervortritt. Dementsprechend wird das Subjektivitätsprinzip epochal inauguriert. Typische neuzeitliche Figuren sind: der Mensch als Täter der Geschichte, als homo faber, als rational denkendes Wesen, als seinen Körper entdeckendes (und malendes), als solo singendes Wesen usw.

In dieser Epoche in der nun schon fortgeschrittenen Moderne - ist auch Psychologie als Wissenschaft entstanden. Die Entstehung der Psychologie lässt sich nicht einfach auf die Gründung des Wundtschen Instituts zurückführen. Sie umfasst vielmehr all jene langfristigen Prozesse, in deren Folge dem Menschen als Subjekt, als Träger unter anderem auch von psychischen Strukturen mit sozialer und theoretischer Aufmerksamkeit begegnet wird. Erst als der Mensch als individualisiertes Subjekt die Selbstverantwortung übernimmt (und nicht mehr wie im Mittelalter als ein undifferenzierbarer Bestandteil eines ihn übergreifenden Gefüges auftritt), erst dann beginnen auch seine psychischen Prozesse eine neue soziale Bedeutung zu verlangen und zu gewinnen. Sie bekommen eine reale soziale Verankerung, aus der sich zunächst eine lebenspraktische und später dann auch eine theoretisch artikulierte Behandlung entwickelt.

Dieser skizzenhafte Umriss sollte hier nur einen groben Hinweis auf die neuzeitliche soziale Verankerung des Gegenstandes von Psychologie geben.

Zur Grundlage der Psychologie -wie jeder Wissenschaft- gehört auch ihre methodische Ausstattung. In Wundtscher Auffassung werden der Psychologie zwei Methoden zugesprochen: Introspektion und Experiment. Zur Völkerpsychologie gehören nach Wundt zwar auch einige andere Methoden (Analyse von Kulturprodukten), nicht aber das Experiment.. Allerdings hatte die Wundtsche Völkerpsychologie nur eine Randbedeutung in der weiteren Entwicklung der Psychologie; ausschlaggebend war die sogenannte physiologische Psychologie und ihre Methoden.

Zur notwendigen Revision der psychologischen Methodologie gehören deshalb sowohl die Aneignung von methodischen Errungenschaften der Völkerpsychologie als auch die Überprüfung des theoretischen Status von Methoden der experimentellen Psychologie. Die hier vorgeschlagene Revision wird in Richtung auf eine Historisierung und Sozialisierung sowohl der Introspektion als auch des Experiments vollzogen werden.

Ein relevanter Hinweis: die sozialen Ursprünge des Experiments finden sich in jenem Gefüge, in dem einerseits die Handarbeit an Bedeutung und positiver sozialer Bewertung gewinnt, andererseits aber die sozialen Träger dieser Arbeit bestrebt sind, sich auch zusätzliche wissenschaftliche Kompetenzen anzueignen. "Die beiden Komponenten der wissenschaftlichen Methode waren noch bis 1600 getrennt intellektuelle methodische Ausbildung war der höheren Klasse gelehrter Leute vorbehalten, den Universitätsgelehrten und den Humanisten, das Experimentieren und die Beobachtung waren mehr oder weniger plebejischen Arbeitern vorbehalten... Schliesslich brachen die sozialen Barrieren zwischen den beiden Bestandteilen der wissenschaftlichen Methode zusammen, und die Methoden der höheren Handwerker wurden von akademisch ausgebildeten Gelehrten übernommen: die Wissenschaft war gebore". (Zilsel, E./1976/, S.59-60).

Die Introspektion war als wissenschaftliche Methode lange umstritten, wurde aber nichtsdestoweniger auch befürwortet und für die Psychologie als unersetzbar gehalten. Mir geht es hier nicht um den alten Methodenstreit. Zur methodischen Aufgeklärtheit möchte ich von einer anderen Seite beitragen, nämlich von der Seite einer Sozialgeschichte der Introspektion.

Die Introspektion hat zur allgemeinen logischen und realen Voraussetzung die vollzogene Internalisierung psychischer Prozesse. Damit werden wir wieder auf den neuzeitlichen Zusammenhang verwiesen. Gerade in diesem Zusammenhang werden die Lebensformen einer Individualisierung und daraus folgend auch einer Internalisierung unterworfen. Durch einen offenen Spielraum für individuelle Tätigkeiten aller Art werden neue Verhaltens- und Denknormen in Gang gesetzt, die durch eine wachsende Psychologisierung gekennzeichnet sind.

Gerade der Trend von Vielen zu Einzelnen und von Aussen nach Innen kennzeichnet jenen Zivilisierungsprozess, der der abendländischen Geschichte seit der Renaissance eine einzigartige Prägung gibt. Die moderne Welt bilden sowohl die neuen Institutionen (zentralisierter Staat, marktorientierte Produktion, massenhafte berufliche Ausbildung usw.) als auch die veränderten subjektiven Eigenschaften, ohne die das gesamtgesellschaftliche Gefüge nicht hätte zustande kommen können. Zwischen den gesellschaftlichen und den individuellen psychischen Strukturen besteht kein Vorher-Nachher-Verhältnis, es geht vielmehr um ihr Ineinandergreifen.

Wenn man dieses Ineinandergreifen aus der Perspektive der Bildung von psychischen Strukturen betrachtet, hat man den Gegenstand einer historischen Psychologie bestimmt. Ihre Aufgabe wäre es, zu untersuchen "wie Fremdwänge sich in Selbstzwänge verwandeln, wie in immer differenzierterer Form menschliche Verrichtungen hinter die Kulisse des gesellschaftlichen Lebens verdrängt und mit Schamgefühlen belegt werden, wie die Regelung des gesamten Trieb- und Affektlebens durch eine beständige Selbstkontrolle immer allseitiger, gleichmässiger und stabiler wird." (Elias, N./1969/, S.313).

Erst auf dem Grund des gesellschaftlichen Zwangs zum Selbstzwang können sich differenziertere psychische Strukturen entwickeln - eben das was das Innere des Menschen ausmacht: seinen Gedankenwelt, sein Triebleben, seine Gefühlsarten usw. In, diesem Zusammenhang ist auch die Entstehung der modernen Introspektion zu sehen. Was diese von der Augustinischen Introspektion unterscheidet, ist eben ihr weltlicher, universeller Charakter und ihre funktionelle soziale Verankerung. Man könnte sagen, dass die Introspektionsfähigkeit und -praxis zum Bestandteil eines allgemeinen modernen Sozialcharakters gehört (wenn man den Frommschen Begriff auf diese Weise verallgemeinern darf).

Diese Eliassche historische Psychologie könnte man metatheoretisch als eine historische Konkretisierung der kultur-historischen Psychologie von Wygotski betrachten. "Daher könnten wir das grundsätzliche Ergebnis, zu dem uns die Geschichte der kulturellen Entwicklung des Kindes geführt hat, als die Soziogenese höherer Verhaltensweisen bezeichnen. Also, alle höhere psychische Funktionen sind interiorisierte Sozialbeziehungen und bilden die soziale Grundlage einer Persönlichkeit." (Wygotski, L.S./1960/, S.198)

Wygotskis Psychologie ist ihrem Wesen nach eine kulturhistorische Theorie, die aber die geschichtliche Entwicklung als ein im Grunde monotones Abwickeln auffasst, das den wichtigsten Erfindungen (Werkzeug, Schrift) entspricht. Andererseits verfolgt Elias eben den Prozess weiterer psychischer Differenzierungen -z.B.

Herausbildung von Selbstzwangapparatur, die die Triebregelung dem Zwang zur Langsicht, vermittelt durch Schamgefühle, Psychologisierung, Rationalisierung, aussetzt.

Sozialgenetisch und logisch betrachtet gehen alle diese Prozesse der Eröffnung des Wundtschen Instituts voraus. Sie machen es erst möglich, dieses historische Ereignis geistesgeschichtlich zu verstehen, d.h. wir können und dürfen das nicht als die Folge einer oder mehreren Ursachen erklären. Wir können nur Motive verstehen (gesellschaftliche Voraussetzungen, Interessen), in deren Zusammenhang auch ein wissenschaftliches Interesse an der Erforschung psychischer Strukturen entstand.

Diese Argumentationskette - die hier nur angedeutet werden konnte und die aufgrund neuerer Entwicklungen selbstverständlich auch fortgesetzt werden könnte - sollte als allgemeine

Begründung für die These von der konstitutiven neuzeitlichen Verankerung der Psychologie dienen. Trotz dieses wechselseitig konstitutiven Verhältnisses zwischen Moderne und Psychologie, ist diese Einsicht in das Wissensgebiet der Psychologie noch nicht eingegangen. Aber eben wegen der Erkenntniskompetenz - die auch eine Werthaltung der Moderne ist - sollte die Psychologie auch ihre Soziogenesetheoretisch aufarbeiten.

Psychologie -Postmoderne

Wenn man die bisherige Postmoderne-Debatte anschaut, dann fällt einem auf, dass unter den bekanntesten Gesprächspartnern -Literatur, Architektur, Malerei, Soziologie, Philosophie- Psychologie meistens nicht zu finden ist. Dies kann man wohl nur als ein Zeichen des fehlenden metatheoretischen Verhältnisses zwischen der Psychologie und der Postmoderne verstehen. Es scheint als ob sich die Psychologie der Postmoderne-Konjunktur entzieht. Aber dieses Entziehen soll überprüft werden im Hinblick auf latente psychologische Voraussetzungen, die sich Theorien über die Postmoderne angeeignet haben.

In der Postmoderne-Diskussion wird gegen den Universalismus, den Rationalismus, den Konsensanspruch, gegen alle nach Einheit strebenden Verfahren und Institutionen aufgerufen. Dagegen wird für die sich selbst legitimierende Singularität und die Pluralität von solchen Singularitäten, für den Dissens, für die Paralogie plädiert. "Indem das Interesse der postmodernen Wissenschaft den Unentscheidbaren, den Grenzen der Kontrollgenauigkeit, den Quanten, den Konflikten mit unvollständiger Information, den Frakta, den Katastrophen, den pragmatischen Paradoxien gilt, skizziert sie die Theorie ihrer eigenen Entwicklung als diskontinuierlich, katastrophisch, unberechtigt, paradox...Und legt als neues Legitimationsmodell (...) das der als Paralogie verstandenen Differenz nahe." (Lyotard, J.-F./1979/, S.172)

Man kann sich der Versuchung nicht entziehen, in dem postmodernen Begriffsgefüge - Singularität, Dissens, Paralogie, Patchwork der Minderheiten - das Gegenstück der Sprachphänomenologie des psychischen Leidens (sogenannte private Sprache) wiederzuerkennen. Es drängt sich einem auf, eine provokative Aussage zu

wagen: es scheint als ob die bisher zu therapeutisierenden Weltanschauungsmodelle in ihren sprachlichen Ausdrücken eine positive Umwertung erfahren hätten und als verallgemeinerte Figuren des postmodernen Wissens (und Lebens) befürwortet werden.

Liotard selbst leitet sein neues Legitimationsmodell aus der Phänomenologie der wissenschaftlichen Praxis ab. "Geht man von der Beschreibung der wissenschaftlichen Pragmatik aus, so muss der Akzent fortan auf den Dissens gelegt werden." (Liotard, J. - F. /1979/, S.190) Zu dieser selbstreflektierten wissenschaftspragmatischen Quelle von Lyotards Modell möchte ich die erwähnte "psychopragmatische" Quelle hinzufügen. Einen wichtigen Grund für diese Einbeziehung habe ich Lyotards Ansatz selbst entnommen: nämlich seiner Bevorzugung eines narrativen, Wissens gegenüber dem wissenschaftlichen. Da die Sprachpragmatik der alltäglichen Kommunikation, die auch viele Formen von misslungenen, gescheiterten Kommunikation umfasst, dem narrativen Wissen näher steht als die wissenschaftliche Pragmatik, empfiehlt sich eine Analyse hinsichtlich ihrer theoretischen Reichweite innerhalb des postmodernen Ansatzes.

Der phänomenale Isomorphismus zwischen der Begriffsapparatur der Postmoderne und der sprachlichen Artikulation des (abweichenden) Standpunktes psychischen Leidens ist sehr beachtenswert. Es entspricht zwar dem Anliegen der Postmoderne, eine umfangreiche Dezentrierung von allen bisherigen "Zentren" (von autonomen, einheitlichen Subjekt, von dominanten Argumentationsfiguren, von gesellschaftlichen Machtzentren etc.) zu unternehmen. Danach ergibt sich die Forderung nach der Rehabilitierung all jener ignorierten, verdrängten, nicht zugelassenen Inhalte, Verfahren, Träger.

Ihrem Wesen nach sind die Erfahrungen und Eigenschaften, die einer z.B. therapeutischen Korrektur bedürfen, eben solche, die von einer sei es von aussen auferlegten, sei es schon internalisierten Norm (Zentrum) abweichen, und die deshalb verdrängt werden müssen. Dadurch ist aber eine Dynamik des Leidens in Gang gesetzt, die mit dem Ausbruch des Verdrängten oder dem Zusammenbruch des Leidenden droht - es muss also Hilfe in der Therapie gesucht werden. Die therapeutische Sequenz beginnt mit einer Zulassung von stigmatisierten Erfahrungen. Aber selbst die weitestgehend am Patienten orientierte Therapie kann nicht umhin, danach zu streben, mindestens ein persönliches integratives Zentrum herzustellen. Die Therapie kann sich das Reich des Dissenses, der Paralogie nur als ein vorläufiges Interregnum leisten. Der Dissens muss durch Formen ersetzt werden, die für das Subjekt selbst verträglicher sind.

Im Hinblick auf diese Prozedur wird argumentiert (Antipsychiatrie, M. Foucault, z.B.), dass die Therapie sich auf diese Weise selbst den Machtinstrumenten anschliesst, d.h. zur Disziplinierung des Menschen als Voraussetzung einer Reproduktion der herrschenden Machtverhältnisse dient. Ich bestreite diese Einsicht nicht.

Mein Anliegen hier ist jedoch, auf ein grundlegendes Problem hinzuweisen, nämlich auf das der theoretischen und praktischen Legitimation von para-

psychopathologischen Modellen, die als universelle Modelle für die Konstituierung der Lebenswelt gelten sollten. Die Legitimierung kann nicht durch blosse Zulassung ersetzt werden. Mit einer blossen Zulassung des Verdrängten ist nur ein Schritt getan, der aber nicht hinreicht, um ein Gelingen der Kommunikation zu gewährleisten. Das Verdrängte muss in die innerpsychische wie auch interpsychische Kommunikation eingefügt werden. Jede Kommunikation aber setzt eine gemeinsame Sprache voraus, die ihrerseits der Teilnahme an einer gemeinsamen Lebenswelt entspringt. Erst der Zugang zu diesem Werkzeug und dessen Beherrschung eröffnet die Möglichkeit, der Singularität einen auch von anderen anerkannten Platz einzuräumen. Ohne diese Vermittlung kann keine Singularität als Singularität begriffen und verstanden werden. Also: Die Singularität in ihrem Recht, ihrer Legitimität, ihrer Entwicklungspotential ist nicht als etwas Selbstverständliches gegeben, wie es die Postmoderne unterstellt. Sie entsteht erst auf dem Wege einer konstituierenden Kommunikation, vor der die Postmoderne - voller Angst vor noch einer Wiederholung moderner Begriffsfiguren - zurückweicht.

Eine prinzipiell ähnliche Dynamik gilt auch für andere typische postmoderne Begriffsfiguren.

Die Postmoderne lehnt den Legitimationsanspruch ab, aber nach der Zurückweisung dieser typisch modernen Prozedur bleibt nur die Spontaneität synkretistisch nebeneinander wirkender "Spieler" (s. Lyotards Sprachspielmodell). Das Disintegrationsmodell enthält keine Vermittlungsinstanzen und Prozeduren, die ein anderes Spiel überhaupt erkennbar machen könnten. Wie könnten die Regeln, die für ein jeweiliges Spiel konstitutiv sind, übermittelt werden, d.h. wie kann ein Spiel überhaupt gespielt werden?

Entwicklungspsychologisch ist der Begriff des Anderen (sei es als ein Gegenstand, sei es als ein anderes Subjekt) weder eine natürliche Gegebenheit noch eine einfache soziale oder psychische Leistung. Die kognitive Herstellung der Perspektive des Anderen ist eine Leistung, die erst sehr spät, nachdem das Kind bereits der Sprache mächtig ist, erreicht wird. (s. Piaget, J.: *The Psychology of Intelligence*) Es kann keine Toleranz dem Anderen gegenüber geben, wenn es noch keinen Begriff des Anderen gibt. Um die Lebenswelt eines Anderen verstehen zu können, muss man sie zunächst kognitiv rekonstruieren können. Dafür sind aber hochentwickelte und organisierte Operationen notwendig, die keinesfalls zu den angeborenen Anlagen gehören. Wenn sie aber erst entwickelt werden müssen, dann stellen sich notwendigerweise die Fragen nach Übermittlung, Vermittlung, Norm und Verständigung. Und das ist gerade das von der Postmoderne zurückgewiesene Gefüge, für das kein entsprechender Ersatz erarbeitet wird.

Die Rekonstruktion einiger postmoderner Begriffsfiguren hat auf ihre psychologietheoretisch nicht begründbaren Voraussetzungen hingewiesen. Die Postmoderne selbst lehnt jeden Begründungsanspruch ab, wobei sie an die grossen philosophischen Begründungsprozeduren (transzendentaler Idealismus, Emanzipationsdialektik usw.) denkt. Ich meine aber, dass eine psychologische

Begründung unvermeidlich ist, wenn die Postmoderne das Terrain der Intertextualität verlässt und das Feld der Morrißschen Pragmatik betritt. Lyotards Gebrauch vom Sprachspielmodell verweist gerade auf die pragmatische Perspektive, die ihrerseits notwendigerweise mit den Prinzipien des menschlichen Denkens und Verhaltens zu tun hat.

Mein Anliegen bei der Betrachtung des Verhältnisses Psychologie -Postmoderne war keinesfalls das Verwerfen der Postmoderne auf Grund ihrer Übernahme von Logik und Figuren, die in Selbsterfahrung und therapeutischer Behandlung des Leidens einer "abweichenden", gestörten Lebensgeschichte zum Vorschein kommen. Die erstaunliche Verwandtschaft der beiden Begriffsmodelle war nur ein Anlass, die impliziten Voraussetzungen dieser Modelle zu veranschaulichen und in Hinsicht auf ihre sozialpragmatische Wirksamkeit kritisch zu beurteilen.

Für die sozialpragmatische Wirksamkeit muss man mindestens gegenseitige Verstehenspotenz der (Sprach) Spieler gewährleisten.

Rein kognitiv setzt jedes Verstehen (wie auch jedes Denken) eine Dezentrierungsfähigkeit des Denkens voraus. Dabei geht es nicht um die Dezentrierung von einem totalitären, universalisierten Subjekt, sondern um die Dezentrierung von der Singularität des Einzelnen selbst. Diese Dezentrierung kann aber nur auf Grund eines verallgemeinbaren kognitiven Spiels vollzogen werden. Erst ein solches allgemeines Spiel macht es möglich, auch die Singularität zu erkennen. Aber "noch immer gilt der moralische Universalismus als Feind des Individualismus, nicht als deren Ermöglichung. Noch immer gilt die Zuschreibung von identischen Bedeutungen als Verletzung metaphorischer Vieldeutigkeit, nicht als deren Bedingung. Noch immer gilt die Einheit der Vernunft als Repression, nicht als Quelle der Vielfalt ihrer Stimmen."(Habermas, J./1988/, S.180)

Gegen solche falsche Unterstellungen kann man auch psychologische Argumente anführen. Sowohl die ontogenetische Entwicklung als auch die Soziogenese psychischer Strukturen begründen die These, dass die Kommunikation zu unerlässlichen Voraussetzungen der psychischen Entwicklung gehört. Das Problem einer gescheiterten Entwicklung (in Form von psychopathologischem Leiden) lässt sich auf das Scheitern der Kommunikation zurückführen.

Insofern könnte man sagen, dass der Mangel an kommunikativen Instanzen zu verhängnisvollen Verkürzung der postmodernen Begriffsmodelle führt.

Das, was meiner Ansicht nach für den heutigen Stand des Verhältnisses zwischen Moderne und Postmoderne gilt -nämlich, dass die Postmoderne unhintergebar in der Moderne verankert ist könnte auch auf das Verhältnis der Psychologie zu diesen zwei Paradigmata übertragen werden. Postmoderne Psychologie steht auf den Schultern der Riesen - der modernen Psychologie, wobei sich keine von beiden dessen völlig bewusst ist.

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Gestalt psychology in Spain (1920-1989). A study through specialized journal publications

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ABSTRACT

The present study is a first approach to a work of a larger scope in progress attempting to establish and assess the influence of modern psychological schools in Spain. This paper deals with the impact of Gestalt Psychology, and involves the analysis of references to its main leaders (Wertheimer, Koffka, Köhler, Lewin) in 20 Spanish psychological journals.

Our results do not entitle us to establish the existence of a true Gestalt School in Spain. This, however, does not imply ignorance of Gestalt's most relevant contributions, which have left many traces in Spanish psychological literature.

The early development of psychology in Spain ought to be conceived as a result of importing ideas rather than the pursuit of original aims (Carpintero, 1981). This is the reason why the study of the reception of those psychological authors and movements largely responsible for the peculiar features of contemporary psychology is also a fruitful way of approaching the main lineaments of Spanish psychology. The present study is a first approach to work of a larger scope in progress attempting to establish and assess the influence of the main contemporary psychological schools on Spanish psychology.

This paper deals with the impact of Gestalt Psychology in Spain. It involves the analysis of references to its main leaders (Wertheimer, Koffka, Köhler, Lewin) in a number of Spanish psychological journals. Bibliographical references are an excellent indicator of the acknowledgement given to an author within a particular scientific community (Garfield, 1979; Carpintero & Peiró, 1983). In Spain, moreover, the development of psychological journals closely reflects that of psychology itself (Peiró & Carpintero, 1981; Tortosa, 1989). Thus, tracing the presence of Gestalt psychologists in them practically amounts to facing the whole history of Spanish psychology throughout the twentieth century.

Sources and method Twenty Spanish psychological journals constitute the source of our study (Table 1). In their selection several aspects have been taken into account.

First, only **scientific** journals with a certain degree of **continuity** were selected, even when these have in some cases undergone important transformations.

Secondly, attention was also paid to the journals' **temporal distribution**, covering from the 1920s -when the first Spanish journal partially devoted to psychology, *Archivos de Neurobiología*, was first published -up to the 1980s. Seven decades of Spanish psychology were thus reviewed, thereby obtaining a quite accurate picture of its activities.

In order to avoid an excessively local bias of the data, the journals' **geographic distribution** was also taken into account. Thus, although journals published in Madrid are most numerous (many of them, however, with a nation wide scope), journals published in Valencia, Barcelona, Murcia and Granada were also included.

Subject distribution was another aspect considered. It was our aim to offer a wide thematic spectrum from among those journals in which Gestalt presence was more likely. Our selection therefore includes journals of a general and experimental nature, as well as others on applied psychology, social psychology, developmental psychology and the history of psychology.

An **institutional distribution** was finally taken into account, too. Not only academically oriented journals (which however constitute the vast majority), but also professional journals, journals published by scientific societies, and even journals privately managed were therefore included.

Bibliographical references in the selected journals were analyzed according to the usual procedures followed in this kind of studies (Garfield, 1979; Carpintero & Peiro, 1981, 1983).

Citing journals

Our analysis of the 20 journals listed in Table 1 covers 70 years of Spanish psychological history. Along this period, Gestalt psychologists were cited 173 times, amounting to 2.54 references per year and 8.65 references per journal publication. However, they were not cited in all the years nor in all the journals considered, and this deserves additional comment.

In 6 out of the 20 journals no reference is made to Gestalt psychologists at all. This absence, however, does not seem to be directly related with these journals' main topics of interest, for they are cited in other journals with a similar focus.

So they are mentioned in just 14 of the journals analyzed. Therefore, excluding journals with no references to them, the average number of references per journal is now 12.35, and only in three of them is the number of references above average: in *Revista de Psicología General y Aplicada*, *Anuario de Psicología* -the two journals with longest standing within the history of Spanish psychology- and *Revista de*

Historia de la Psicología. The former was founded by José Germain in 1946, and covers practically on its own a whole period of Spanish psychology; namely, the period beginning in the postwar years and ending in the 1970s -when another period of great expansion and institutional and social consolidation begins. *Anuario de Psicología* was founded by Miguel Siguán in 1969 as organ of expression of the Department of Psychology of the Universidad de Barcelona, and it actually marks the beginning of this last period. The *Revista de Historia de la Psicología* is the first and only Spanish psychological journal entirely devoted to historical issues; it was founded by Helio Carpintero in 1980, and is published by the Department of General Psychology of the Universidad de Valencia.

However, it must not be overlooked that two of the journals registering a higher number of references are also those in which a greater number of years have been analyzed. Thus, when the average number of references per year in each of the journals was examined (Table 1), it was found that it is the *Revista de Historia de la Psicología* that is at its top, with an average of 2.6 references per annum. At some distance, it is followed by a group made up of 6 journals: *Boletín de Psicología* (1.5), *Revista de Psicología General y Aplicada* (1.36), *Estudios de Psicología* (1.2), *Anuario de Psicología* (1.05), *Revista de Psicología Social* and *Informes del Departamento de Psicología General* (1 each). A second group consists of: *Infancia y Aprendizaje* (.75), *Psicotecnia* (.71), *Papeles del Colegio* and *Psicología del Trabajo y de las Organizaciones* (.6 each), and *Cuadernos/Quaderns de Psicología* (.57). A last group, notably removed from the previous two, is composed by: *Análisis y Modificación de Conducta* (.33), and *Archivos de Neurobiología* (.24).

If we now group our journals thematically, we may get a preliminary idea of the areas in which Gestalt psychologists have exerted greater influence in Spanish psychology. References to Gestalt psychology have been most frequent in the one journal on the history of psychology, which shows the highest citation index (2.60). This is followed by social psychology journals (.92) -Lewin's powerful influence is strongly felt here (Lafuente & Ferrándiz, in press)-, journals on educational psychology (.66) and other applied fields (.60), and journals with a general orientation (.54). Journals addressed to professionals working in the field show the lowest citation index (.40) and come thus at the end of the list.

Volume and evolution of references

References to Gestalt psychologists, however, are not homogeneously distributed along the years studied. It is useful to divide this time span into three separate periods, following other studies on the development of Spanish psychology during this century (Peiró & Carpintero, 1981; Tortosa, 1989; Pérez, Tortosa & Carpintero, 1989). The first period extends to the mid-thirties, when the Spanish Civil War broke out and the slowly constructed scientific tradition in psychology was thereby abruptly disrupted. The second period began in the postwar period (from 1939 on), in an ideological climate clearly hostile to scientific psychology (Carpintero, 1984), and is characterized

by the constant effort to reestablish that broken tradition. A new expansion period begins in the seventies with the restoration of the democratic regime and the establishment of psychology as a specialty in university studies.

The degree of attention Gestalt psychologists receive in each of the above mentioned periods is very inconsistent: 4 references in the first, 31 in the second, and 138 in the third. In order to determine the historical variation in citation of Gestalt's works in Spanish psychology, however, it is not enough to consider the evolution of the overall number of references. It is also necessary to relate this to the number of journals scrutinized in each period (that is, one journal for the first period, two for the second, and eighteen for the third), as well as to the number of years they were published. A ratio of .06, .26 and .20 for each of the periods may be thus obtained. Therefore, the level of references to Gestalt psychologists are seen to experiment a dramatic increase from the first to the second period, while it decreases slightly but noticeably in the third.

Cited authors

Let us now turn to the differential volume and evolution of the references to the psychologists considered: Lewin is by far the most cited author (76 references), followed by Köhler (46), Koffka (29) and Wertheimer (22).

Once again, however, this data must be related with the number of years analyzed in the journals of each period. Lewin stands out as the author with a higher citation index (.72); Köhler and Koffka come next at some distance (.55 each); while Wertheimer is last with a score of .27.

To focus now on the differential evolution of their impact: Lewin is the one author whose citation index has been steadily increasing from one period to the next (.12, .20, .40). The rest of them may be seen to increase from the first to the second period, and to decrease again in the third. This decrease is slight in Köhler (.6, .27, .22), somewhat steeper in Wertheimer (0, .17, .10) and dramatic in Koffka (.6, .40, .09), who is by far the most visible author in the second period -his presence in current Spanish psychology seems to be much lower than that of Köhler's, in spite of having the same overall citation index. As for Wertheimer, he has kept the lowest index of all along the three periods.

To sum up: from 1920 to 1936 Gestalt psychology scarcely has any presence in Spain, even though its research seems to have been well known by some Spanish psychologists (Mira, 1924). From 1945 to 1968, largely due to his *Principles of Gestalt Psychology* (1935), Koffka's name stands out from the rest and becomes very visible in Spanish psychological literature. Moreover, some other outstanding works of the Gestalt School (Lewin's *A Dynamic Theory of Personality*, 1935; Wertheimer's *Productive Thinking*, 1945) start making their appearance in the Spanish psychological scene, thereby raising the overall level of its impact, which reaches its peak in this period. Finally, from 1969 to 1989, it is Lewin who takes off. His continuously raising

trajectory makes him a true "functional classic" in current Spanish psychology (Carpintero, 1981; Lafuente & Ferrándiz, in press).

Cited works

The 173 references found are distributed over 63 works by Koffka, Köhler, Lewin and Wertheimer; the citation average is therefore 2.75 references per work. More than half the works are Lewin's, also the most cited author. References to Lewin, on the other hand, are very scattered, Köhler being the author who concentrates a higher number of references per work.

Three groups of works may be distinguished as a function of the number of references obtained, each group including about one third of the total references. First, there is a small group made up by the six most cited works, accounting for 35.83% of references and, thus, appearing as the School's most significant works for Spanish psychology. A second group is made up of the fifteen works obtaining between 3 and 7 references, which account for 34% of references detected. Finally, a third group includes the remaining 52 works, that is, those receiving 1 or 2 references and accounting for the remaining 30.05%. The works most frequently cited, that is, those included in the first group mentioned, are listed in Table 2.

It is worth noting that all Gestalt psychologists considered are represented in the list. It may also be pointed out that, even though the list is headed by Koffka's and Lewin's books, Köhler is the only one to have three books in it, thereby becoming the most significant of them all from this point of view.

Citing authors

The references have been given by a pool of 127 authors, with an average reference of 1.36 per author. The group of twelve authors listed in Table 3 is responsible for more than one third of the references found (33.72%). They belong to many different academic areas (general psychology, educational psychology, clinical psychology, social psychology and the history of psychology) and are quite well-known in their respective fields. However, it is worth emphasizing that, consistently with the result found above, most references appear in historically oriented papers.

CONCLUSION

Our data does not entitle us to establish the existence of a true Gestalt School in Spain. The number of references to its main leaders is not too high, nor are the citing psychologists "gestaltists" in a strong sense of the term (the obvious exception being, of course, Mary Henle). However, this does not imply ignorance of Gestalt's most relevant contributions, which -as shown in our study- have left many traces in Spanish psychological literature.

It is in the area of the history of psychology where such traces become most evident. This suggests a consideration of Gestalt psychology as "something of the

past", that is, a School to be taken into account when preparing historically oriented revisions on certain matters, but with a no real presence in current research.

Obviously, this does not apply to Lewin, the most cited author of all, whose growing impact through the years allows him to be considered as a true "functional classic" in current Spanish psychology.

According to our analysis, it is from 1939 to 1968 when Gestalt influence becomes most apparent in this country. Koffka's name stands out in this period, his *Principles of Gestalt Psychology* also being the School's most cited work.

Wertheimer, on his part, has always been the least visible of the four authors. His *Productive Thinking*, however, ranges among the six most significant works by any of them.

As for Köhler, his works are seen to have a particular relevance in Spanish psychology. He succeeds in keeping an above average citation level along the second and third periods studied. He is also the one author with three books among the School's most cited works. Finally, he is the only one to have a paper published in a Spanish psychological journal while still alive (Köhler, 1959).

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Wertheimer, M. (1945). *Productive Thinking*. New York: Harper.

Table 1. Journals, Years, References

JOURNAL	YEARS	REFS. References/year	
<i>Archivos de Neurobiología</i>	1920-1936	4	0.24
<i>Psicotecnia</i>	1939-1945	5	0.71
<i>Rev. Psicol. Gral. y Aplicada</i>	1946-1989	60	1.36
<i>Anuario de Psicología</i>	1969-1989	22	1.05
<i>Análisis y Mod. de Conducta</i>	1975-1989	5	0.33
<i>Infancia y Aprendizaje</i>	1978-1989	9	0.75
<i>Inf. Dept. Psicol. General</i>	1978-1980	3	1.00
<i>Cuadernos/Quaderns de Psicol.</i>	1979-1985	4	0.57
<i>Estudios de Psicología</i>	1980-1989	12	1.20
<i>Rev. de Historia de la Psicol.</i>	1980-1989	26	2.60
<i>Papeles del Colegio</i>	1980-1989	6	0.60
<i>Psicológica</i>	1980-1989	0	0.00
<i>Informes de Psicología</i>	1982-1986	0	0.00
<i>Investigaciones psicológicas</i>	1982-1989	0	0.00
<i>Rev. Esp. Terapia del Comport.</i>	1982-1989	0	0.00
<i>Informació Psicológica</i>	1983-1989	0	0.00
<i>Anales de Psicología</i>	1984-1989	0	0.00
<i>Boletín de Psicología</i>	1984-1989	9	1, 50
<i>Rev. de Psicología Social</i>	1985-1989	5	1.00
<i>Psicol. Trabajo y de las Org.</i>	1985-1989	3	0.60
TOTAL		173	

Table 2. Most cited works

WORK CITED	AUTHOR	REFERENCES	%REFS.
<i>Principles of Gestalt Psychology</i>	KOFFKA	14	8.09
<i>A Dynamic Theory of Personality</i>	LEWIN	12	6.94
<i>Gestalt Psychology</i>	KÖHLER	10	5.78
<i>The Mentality of Apes</i>	KÖHLER	9	5.20
<i>Productive Thinking</i>	WERTHEIMER	9	5.20
<i>The Task of Gestalt Psychology</i>	KÖHLER	8	4.62

Table 3. Main citing authors

AUTHOR	REFERENCES	%
HENLE, M.	7	4.05
GERMAIN, J.	6	3.47
GARCIA VEGA, L.	5	2.89
GARRIDO, E.	5	2.89
GENOVAR, C.	5	2.89
SAMPASCUAL, G.	5	2.89
YELA, M.	5	2.89
PEIRO, J.M.	4.33	2.50
ARNAU, J.	4	2.31
BAMBAREN, C.	4	2.31
CASTILLA DEL PINO, C.	4	2.31
OVEJERO, A.	4	2.31
TOTAL	58.33	33.72

José Germain, Spanish neuropsychiatrist

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The formation of José Germain (1897-1986), "one of the initiators and firmest maintainers of Spanish scientific psychology" (Valenciano, 1981), arises from medicine, out of which his psychiatric vocation is outstanding, a fundamental issue to understand his thoughts and his conception of man. Our objective is defined in three aspects: determination of the scientific genealogy of José Germain, delimitation of his work in the field of neuropsychiatry, and a brief analysis of the context in which his work is achieved, an extensive and assorted work which taking form in a great quantity of publications which cover from neurology and psychopathology to experimental psychology (Misiak & Sexton, 1955).

Scientific Genealogy

From the end of the last century and until the beginning of this one, the lack of centers for clinical and experimental work, the lack of competent auxiliary personnel, the individualistic spirit and the resistance towards scientific labour on the part of many Doctors, complicated the problems of teaching and psychiatric assistance in Spain. The official education of Neurology and Psychiatry, was led by the teachers of Medical Pathology and Legal Medicine, not always qualified to promote the task of a complete Neuropsychiatric reorganization and to scientifically orient the student body. The attempts of free education in Hospitals and asylums, both public and private, even though they counted with the support of real specialists and masters, stumbled upon the serious obstacle of the non existing educational organization for research work (Germain, 1930).

This Psychiatry, with Mata, Peset y Vidal, Pi y Molist, Esquerdo, Vera, Valle, and Giné y Partagás, was inspired mostly by France, due to the fact that Pinel's influence and his schooling with Esquirol created the bases for a Natural-Scientific Psychology. Never the less, as of the decade of the twenties, there was a notable and predominant Germain influence. This influence achieved its first milestone with the adoption and development of Kraepelin's ideas, with the first prior German Psychiatrist,

Griesinger, not able to leave track of his works, due to the predominance, in those times, of the French School (his manual, which appeared in 1845, found no acceptance in Spain). After Kraepelin's works, followed the translations of the works of E. Bleuler, O. Bumke and K. Jaspers, "nevertheless, this flourishing period of clinical-psychiatric research work, mounted on a Kraepelinian base, would be severely interrupted by the civil war" (Dieckofer, 1984).

Spanish psychiatry received its first impulse from Forensic Medicine, whose most outstanding representatives were: Orfila, Mata and Esquerdo, who transmitted to their students, the task of developing a new discipline. Under the direction of Giné and Galcerán Granés in Barcelona, and of Simarro and Achúcarro in Madrid, the first national psychiatric schools of Spain were created, receiving also a marked influence from other specialists in other adjacent fields (Ramón y Cajal in the field of Neuroanatomy, and Marañón in Internal Medicine). The generation of Giné y Partagás and Esquerdo, has been considered as the "generation of the restoration", for being the first to initiate the Europeization of Spain, orienting firstly towards French and subsequently towards German psychiatry (Dieckofer, 1984).

Simarro played a key role in this process. A magnificent expert in the Neurophysiology of the time, he exerted a great influence on his disciples and those who prolonged his works in diverse fields: physiology of the nervous system, psychiatry, experimental psychology and pedagogy. Cajal was influenced by him, and as disciples of his we may also mention Achúcarro and Lafora, who lead to current Psychiatry and Neurology (Siguán, 1977). Simarro and his collaborators did not allow for the decadence of the modern clinical concept of Psychiatry, derived from the doctrine of the school of Kraepelin, on behalf of which, they favored and oriented personally and biographically, all scholars that consulted them (Germain, 1930).

Alongside them we may also point out to Sacristán, who introduced the theory of Kretschmer's constitution into the Spanish Psychology. And along with other contributions, Psychiatry had an ample base of German research "schools", accelerating thus in the decade of the thirties, the institutionalization of University Psychiatry. All this without forgetting the key role played by the investigations of Ramón y Cajal in the sphere of Histopathology, covering together with other direct disciples, an essential stage in the knowledge of the nervous system through Neurohistology (Gallego, 1983).

Between 1910 and 1920, a numerous group of disciples called on Cajal, and together they performed a key task in the subsequent institutionalization and development of the health sciences in Spain. Among these: Tello; Achúcarro, and with him Gayarre, Calandre, Sacristán, Fortún, and Del Río-Hortega; Lafora and Villaverde, disciples of Simarro; and finally, Lorente de No and Fernando de Castro. Later on other new disciples would come alongside Tello (Arteta, Puchol, Gorriz, Alonso) in the Laboratories of the Institute or of the Faculty; around Lafora (Prados Such, Gonzalo, López Aydillo, Llopis); and in the Laboratory of Del Río-Hortega (Gallego, Costero, Llombart). A flourishing school, which the civil war would put and end to (Gallego, 1983).

"Cajal and his disciples, especially Achúcarro, Tello and Del Río-Hortega, discovered histological facts of great importance which would set the guideline for new investigations. And Achúcarro and Gayarre, in the clinical sphere are the paladins of the era of Neuropsychiatric renovation, consolidating his collaborators and disciples... Around the same time, the works on clinical, therapeutic, anatomical and biological investigations, in the matter of nervous and mental sicknesses, were becoming quite frequent, and reflected the beneficial influence exerted upon Spain by the more progressive countries, which allowed for a change in orientation and technique in the daily clinical labour and in the speculative studies" (Germain, 1930).

If Achúcarro "was the first in Spain to accomplish the fertile conjunction of the man of science and the Clinic, the pathologist and the professional physician" as Marañón said, Lafora could be placed in early times at the vanguard of this movement, which unites science and clinic, histopathology and mental pathology (Carpintero, 1986).

Achúcarro and Lafora are the first Spaniards who, while being clinical psychiatrists, conducted investigations in neuropathology using techniques which the Cajal School and Achúcarro himself revealed as the most skilful. In this way, the positive Kraepelinian tradition; for each clinical psychiatric chart, a particular neurohistological alteration; would find in them a fertile culture, promoted by Alzheimer, Nissl and Spielmeyer, among others. The European status of Spanish psychiatry is initiated with Achúcarro and Lafora" (Castilla del Pino, 1972). Lafora, who had spent some time in Germany, followed the footsteps of his friend and colleague. Both first in Germany; in Munich, with Kraepelin and Alzheimer; and in Washington later. Starting off from neurologic histology he approached the problem of mental pathology, and didn't go into questioning the need, signalled by Kraepelin, of incorporating the services of Psychology, to fulfill the needs of Psychiatry (Carpintero, 1986).

Work in the field of Neuropsychiatry

José Germain finished his studies in medicine in the Medicine Faculty of the University of Madrid, obtaining his doctorate in 1923. Nevertheless, even before finishing his career, he set upon a Neuropsychiatric training (Valenciano, 1971). Upon the third year of his career, he frequents the laboratories of Calandre and Negrín, learning Histology, Anatomopathology and Physiology. In 1920 he succeeds Prados Such -who was pensioned off to London to work with Mott in clinical and experimental neurology- as assistant to Dr. Lafora, his indisputed master, in the Public Consulting at calle San Bartolomé (having been preceeded by Lafora, Achúcarro and Sacristán). "My trajectory as a Neuropsychiatrist, started alongside Dr. Lafora, who some years back had returned from the United States... He had predilection towards Neurology, and in his early years in Madrid, he was more inclined towards Neurology than towards Psychiatry" (Germain, 1983). From Lafora, he received a double tradition: that of Simarro -with his enthusiasm for positive science and with his ideological implications-, and from Cajal -with his will for rigorous investigation-

(Siguán, 1981). Lafora would introduce him to other masters, and to Cajal's Laboratory. And through his guidance, he would meet Ortega y Gasset.

In this Clinic, and around Lafora "the first school of Neuropsychiatry of Madrid would be founded, and surely the first in Spain, conformed by Prados, Villar, Escandón, Somoza and Germain himself" (Valenciano, 1971).

Upon finishing his studies, he goes to Geneva to work under Claparède, then to Berlin, where he befriended Köhler and studied in the Laboratory of Applied Psychology of Dr. H. Rupp (specialized in the problems of reaction time), prelude to his interest in Psychology. The following year, he went to Paris, to the Kecquer Hospital, where he studied Neurology with Sicard, Alajouanine and Guillian in the "La Salpêtrière", he also studied with Dumas and Janet (the latter advocated a psychological dimension in Psychiatry, and was the first to talk about the unconscious and the subconscious) and attended the Consulting Room of Dr. Toulouse in Saint Anne (Germain, 1980). From his work with Sicard, he brought back to Spain the techniques of Suboccipital punction and alcohol injection in the Gasser ganglion, in trigeminous neuralgia (Moya, 1986).

He returns to Madrid as one of the best trained Neuropsychiatrists of the time, and starts working, as a neurologist, a neurophysiologist and a psychiatrist, with Lafora and close to Cajal (Monasterio, 1987). Lafora offers him the post as house physician of his Neuropathic Sanatorium in Lower Carabanchel, where they actively collaborated with each other for a period of four years, (fundamental for his formation in clinical-psychiatry), being the first to apply in Spain, the malaria as a treatment for progressive general paralysis (a technique won the Nobel prize for Von Jauregg in 1927). From this joint venture, Germain published, among other works, some on Piretotherapy in Schizophrenia and in obsessive neurosis, and a paper to the Medical Quirurgical Academy about anancastic pathologies, in collaboration with Lafora (Germain, 1980). "The influence those four years had on me was tremendous: there is nothing like heading a service, day and night during four years, for acquiring the reins of the specialty" (Germain, 1983). His works and publications introduced him to the most prestigious psychiatrists in Madrid (Germain, 1980).

"To be able to correctly assess Germain's achievements, one has to remember Psychiatry as it was before the Fifties, when knowledge in Genetics was scarcely outlined, there were no treatments with Insuline-Cardiazol, no Neuroleptides and assistance in the Hospitals and Sanatoriums barely differentiated itself from the time prior to Pinel. Spanish scientific psychiatry was clearly influenced by Germany, where the Krapelinian Nosologie imperated" (Escardo, 1971).

Since his job at the Sanatorium allowed him some free time, he decided to accept the offer of taking over the Neurology Consultancy of the Institute for Reeducation of Labour Invalids, also located in Lower Carabanchel, close by to the Sanatorium, making both jobs compatible. The Institute was a charitable-educational entity with a medical character, devoted to the assistance and treatment of invalids, and that was, according to Germain (1980). "The trap that destiny set up for him", because within the Institute, there was a professional orientation service for the patients reeducated

there, and this way, without leaving his Consultancy in Neurology, which did not take up too much time, or his job at the Sanatorium, which left him with ample free time, he began collaborating actively with the personnel of this Service: Mercedes Rodrigo, psychologist; Antonio Melián, MD; and José Mallart, psychotechnician.

In these two centers, the Sanatorium and the Institute, Germain carried out the works he published between 1926 and 1929, with a clear neuropsychiatric orientation. Even though Spanish Psychiatry was influenced by German Psychiatry, Germain's background was much wider and richer, influenced as he was by Anglosaxon and French Psychiatry (Escardo, 1971), an aspect that may be clearly appreciated through the bibliography used in his publications.

In Europe, the Psychogenetic investigations contributed to the understanding of Psychosis, but offered no treatment for this kind of mental illness. Nevertheless by empirical means, a series of somatic therapies that acquired a great degree of success with psychosis cases, was discovered. "The first efficient somatic therapy was discovered in the bounds of the first psychosis recognized as having an organic nature: the progressive general paralysis, the so called paretic therapy" (Ackemecht, 1968). Back to Hippocrates, there may be found in medical literature references to the improvement produced by feverish outbursts, specially Malaria, over some mental illnesses. In 1917, Von Jauregg applied Malaria to general progressive paralysis and obtained a resounding success. The Malariotherapy has remained a milestone in the history of Medicine. It is also worth mentioning Jacog Klaesi as one of the pioneers of modern organic treatment of functional psychosis, he introduced prolonged sleep therapy in 1922. A few years later the so called shock treatment therapies appeared, and also in this aspect, since ancient times, it had been established that strong psychic or physical commotions could provoke the improvement or curation of mental illnesses, reason for the application of artificially provoked shock. The Insulinical shock was the first modern method of shock. During the decade of the Twenties, Manfred Sakel applied the coma and the Insulinic convulsion, in a systematic manner on psychotics with great success. In 1933, L. Von Meduna developed another convulsive or shock method, with the administration, endovenously, of Cardiazol. After 1938 this method was substituted almost totally by the electroshock (Ackemecht, 1968). All of these techniques were known by Germain and applied throughout his published works.

With Lafora's help, in the Neuropathic Sanatorium and in the Policlinic, his first three publications emerged. As house physician of the Sanatorium, he published a piece of work on "The Nervous System in Psychiatry", in the journal *Archivos de Neurobiología*. With this paper, he pretends to arouse interest among the professionals towards this particular theme, very much abandoned and relevant not only from the anatomical-physiological point of view but also from the clinical one (the syndromes that produce the alteration of this system are usually complex and widely extended, treated as if they were second grade pathologies, being their symptoms mixed among other better defined, nervous, digestive and endocrine symptoms, which absorb the

interest of the Clinical Chart) (Germain, 1926). His work is centered upon the clinical exploration of the vegetative system, using the classical tests of the epoch (Atropine, Pilocarpine and Adrenaline) in 27 schizophrenic subjects. The obtained results are exposed, as Germain states himself, "in a rather fast and superficial manner", probably because from his point of view, "we should not be content with merely presenting the facts, it is necessary to group them, seeking analogies and affinities that might very well be the future nosological links, employed to clear up the ethiology of many mental illnesses... and in this manner their therapeutics will acquire real importance, abandoning the Symptomatic treatment, replacing it by its causal treatment" (Germain, 1926).

In the neurologic policlinic of Dr. Lafora, and alongside Manuel Villar, they publish, in 1926, another paper related to the vegetative system: "Remission of a case of Schizophrenia after a Fixation Abscess". Fixation Abscesses can be provoked artificially by injecting intramuscularly, an irritating product that will increase the organism's defensive power. It is the application of a leucogenic method (increasing the amount of Leucocytes in the bloodstream) to a precocious dementia, because what the authors denote to justify their work "there is no precise and specific treatment for precocious dementia, which is why as many attempts made to determine this are justified, as long as they don't endanger the patient" (Villar & Germain, 1926). The Fixation Abscess was chosen among all the Leucogenic methods, for two reasons: a) because the leucocytosis produced is very intense, which produces a stimulation of the sympathetic nervous system which is very much weakened in this illness, contributing to the re-establishment of the endocrine-vegetative equilibrium; and b) due to the importance that the emotional factor concedes in the remission of mental illnesses (beneficious role of the painful shock produced by the abscess, as it modifies the nervous vegetative system). The authors denote the excellent results obtained in a case of Schizophrenia, Catatonic form, by two fixation abscesses, which cut, completely, the evolution of the process, restoring the patient back to normality.

Developed also in Lafora's Sanatorium was the work published in 1927 in the journal *Archivos de Medicina, Cirugía y Especialidades* entitled "The Malatiotherapy in precocious dementia", in which Germain figures as house physician of the Sanatorium and Consultant Physician of the Institute for the Reeducation of the Handicapped. In this case, a Piretogenic method is used. Germain states that the Malaric therapy had been practically devoted to the treatment of general paralysis, while very few cases of treatment of precocious dementia were accomplished using this technique, this is what they try to accomplish, inoculating, venously, malaric blood, in five cases of schizophrenia, in its different manifestations (catatonic, hebefrenic,...). Of all these only one resulted positive, the rest modifying only some secondary symptoms. Germain denotes that the success of this treatment is achieved by interrupting the state of delirium or of acute agitation, in cases of acute mental confusion.

Germain recognizes the importance of both the Fixation Abscess and the Malaric Treatment methods. The former, because of the pain it produces, fixes the attention of

the patient, detouring him from his obsessive and raving ideas. The latter, upon repeating the strong physical commotion that precedes the malaric fever, also determines a fixation of the patient with physical symptoms, interrupting the course of his delirious thoughts. In conclusion, it all centers upon trying to interrupt the patient's fixation upon the mental, and coax him to attend to the physical (Germain, 1927).

The remaining publications carried out by Germain while performing as neurological consultant of the Institute for Labour related Invalids, impose a change of direction of Germain's original objectives, even though they are studies related to neurology. The objective of these is to orient the Medical- specialist when it comes to establishing the diagnostic of the injured during labour related accidents, and fixing the type of incapacity. This stage that Germain goes through, denotes the transition of his clear Neuropsychiatric interests towards psychological ones, which as we said before, emerge because of his relation to the Professional Orientation Service of the Institute, as he himself puts it (1983), "and there is where, we could say, from the neurology where I was, to the psychiatry that followed, to a more psychiatric psychology to, practically, psychology itself, my objectives changed and redefined themselves".

"On Cerebral Commotion" is a paper published by the Memoirs of the Institute in 1929. It is a study on cerebral traumatism, seen from a clinical and medico-legal point of view. From the clinical point of view, the syndrome that produces this type of lesions is characterized by migraine, nausea, loss of memory, decrease of attention, and slowness in the idea association process. Mentally, it is characterized by an acute confusion, that may become chronic and lead to mental breakdown if there is a prior toxic background, and neurologically; by modifications in the Cefaloraquideal liquids tension, vago-sympathetic unbalance and vestibular reactions. From the legal point of view, cerebral traumatism create a serious problem for the Medic-evaluator, because "the evaluation of an incapacity requires not only a precision diagnostic problematic, because not always the commotional symptomatology is related to the cause, but also requires a delicate and difficult psychological investigation (Germain, 1929).

His other publications constitute different chapters in Antonio Oller's book, *Medical Practice in Work related Accidents*, published in 1929, and features "Distant consequences of the Medular traumatism", "Traumatic Injuries of the Periferic Nerves" and "Simiation in general from the Neurological point of view".

In the first one, Germain achieves a general extensive study on Medular traumatism, before going into the long-term consequences of the same. In this last aspect, he denotes that there are cases in which the latency period between the traumatism and the presentation of the clinical syndrome can last months and sometimes years, with the complexity that arises at the moment of making a diagnosis and fixing the incapacity. In "Traumatological Injuries of the Periferical Nerves", he emphasizes that "in no other chapter in Pathology as in this one, exists such a fine and detailed knowledge of Anatomy and Physiology, to be able to establish a precise

diagnosis" (Germain, 1929). In his publications, he briefly describes the historical study of the traumatic injuries of these nerves, denoting the ethiology and pathogeny of these injuries, the manner in which the exploration and diagnosis should be accomplished, describing, finally, the symptomatology of the injuries of particular nerves (radial, cubital, median, plexo brachial, ciatic). Finally, in his last publication, which refers to simulation, which is co-authored by Antonio Oller, they emphasize that these type of studies did not start until the establishment of the mandatory military service in Spain, and the promulgation of laws for work-related accidents. Likewise indicating that the simulation of work-related accidents is of such complexity that its proper study requires the competence of diagnosis elements that diverse professionals own (Surgeons, Internists, Neurologists, Oculists, etc.). They also study the personal elements that determine the simulation, supposing that the external motivation is no other than to collect a compensation. They also analyze not only the ethiology and frequency of the simulation, but also its clinical classification, and its general and surgical diagnosis, ending with a series of prophylactic measures which they outline as the most important, raising the Cultural and moral level of the worker to teach him to handle positive ethical values.

CONTEXT

Finally, as a mandatory reference to provide the setting for José Germain's figure as a Neuropsychiatrist, we must make a brief description of a series of events and happenings taking place between 1924 and the Spanish civil war, closely related to both Germain and the Psychiatry of the time. We wish to enhance in first place, the creation, in 1920, of the journal *Archivos de Neurobiología, Psicología, Fisiología, Histología, Neurología y Psiquiatría*, of which by 1929, Germain would become chief editor, coinciding with Cajal's presence as President of Honor of the journal's EditorialComitee. As Martínez Pardo stated in 1978, "the Neuropsychiatrists were lacking a specialized journal in which to publish their works. Until then all works were published in journals of diverse nature such as: *Revista Clínica de Madrid*, *El Boletín de la Sociedad de Biología*, *Los Progresos de la Clínica*", *La Medicina Ibera*, *Plus Ultra*, *El Siglo Médico*, and the medical page of the newspaper *El Sol*, directed by Lafora. In 1929, the journal starts publishing a series of Monographs: *The Supplements of the Archives of Neurobiology*, "in order to alleviate the existing lack of Spanish books and translations of a scientific interest about the principal topics of the journal" (Martínez Pardo, 1978). The journal *Archivos* was founded by a Professor of Metaphysics (Ortega y Gasset) and two Psychiatrists (Lafora and Sacristán), and was specialized in psychological, psychiatric, and biological subjects.

Summoned by Mira and Rodríguez Arias in December of 1924, a meeting of Neuropsychiatrists takes place in Barcelona. Such meeting, which counted with Kraepelin's presence, approved the birth of the Spanish Association of Neuropsychiatrists, destined to study and oversee the progress of the field. Among the tasks to be accomplished by this assembly was that of accepting Sacristán's proposition

referring to officializing, Kraepelin's 1920 classification of mental illnesses, and the founding an official Psychiatric League. The project of the creation of a National League of Mental Hygiene was entrusted to Doctors Saforcada, Mira and Rodríguez Arias. The provisional Directive Council was constituted by Saforcada, Sacristán, Busquet, Rodríguez Arias, Sanchís Banús, Gimeno Riera, López Albo, Prados Such, and Escalas Real (Germain, 1930). In 1926, with the celebration of the First Annual Meeting of this Association in Barcelona, the By-laws were approved for the Spanish League for Mental Hygiene as a filial Organization, of which Germain would be secretary during many years. Even though its fundamental nuclei was medical, the association admitted non medical members, while the League, as other foreign leagues, was integrated by Specialists, Practical Medics, Hygienists, Psychologists, Pedagogues, Jurists, Military, Industrials, Sociologists, etc. with the objective of accomplishing an adequate popular advertising aimed towards the public understanding of the problematic that the Mental Hygiene involves.

In the second annual meeting (Madrid, 1927), the basic sections of the League were initiated (advertising and public education, medical assistance and social protection of psychopaths, professional orientation, pedagogy, criminality prevention, etc.). Also the first Directive Council is elected, integrated by: Cajal, Saforcada, Lafora, Juarros, Rubiano, Torres López, Vallejo Nájera, Sacristán, Sanchís Banús, Mesonero Romanos, Gimeno Riera, Rodríguez Arias, López Albo, Delgado Roig, and Prados Such. Some local committees were also formed (Germain, 1930). In 1930, the First International Congress of Mental Hygiene, took place in Washington, to which Sacristán, Germain and Rodríguez Arias assisted. The first two as Official Delegates of the Spanish Government, and the third as representative of Barcelona's Municipal Council, invited by the American Committee.

In 1931, Germain is appointed as Medical Chief of Psychiatry and Mental Health of the General Direction of Health, and soon after, in 1932, he would become Psychiatry and Psychology teacher in the National Health School; in those times, a professorship only existed in the Faculty of Psychiatry of the University of Madrid, but not in Barcelona (this last one, was in charge of Emilio Mira). Germain would keep both jobs until 1936. "I dedicated the mornings to these jobs and the afternoons to the Institute. It was then when I stopped practicing Medicine as a psychiatrist in order to be able to devote more time to the Institute; this was against the advice of Lafora and others of my peers" (Germain, 1980).

In 1931, the Superior Psychiatric Council is born, conformed by Lafora as president, Germain as secretary, Fernández Sáens as Vicepresident and Mira, Prados Such and Sacristán as Vocals. The Council accomplished many tasks during its short existence (1931-1936). For the first time in Spain, in 1931, norms for "Regulations for a National Psychiatric Statistics" are established and centralized in the Mental Hygiene Section of the National General Direction of Health, presided by Germain; is created the First Mental Hygiene Dispensary; in 1932, the first Patronage of Social Psychiatric Assistance is created -of which Lafora was also president); a qualification

and planning of the subordinate personnel of the public and private psychiatric establishments, is achieved; the Diploma of Psychiatric medical orderly is created; several inspections of the public and private establishments are carried out; numerous improvements of the assistance of psychotic ill are implemented, etc.

Likewise, between 1931-1935, the first "Weeks of Mental Hygiene" are developed, consisting of a series of acts in various environments and locations, conducted by psychiatrists, jurists, pedagogues and other professionals, seeking the objective of forging a mentality about psychiatric problems (Valenciano, 1977). "These were weeks in which we would go to the main towns in each province... and to the very cities. We carried out campaigns on explanation and prevention: stressing that mentally ill was not a crazy person, he was a human being, who can be treated as such, and could be managed as any other patient in a Hospital, in a preventive Center. We would talk about that day's Hospital..." (Germain, 1983). Germain, in his triple condition as secretary of the Hygiene League, Medical Chief of the Mental Hygiene and Psychiatry Section of the General Direction of Health and Secretary of the General Council, played a key role in the organization and development of the above mentioned "weeks", which took place annually, with conferences, publications and radio emissions on subjects relative to Mental Hygiene.

"The Neuropsychiatrists Association and the Mental Hygiene League changed the atmosphere around Psychiatry. In other words, Psychiatry was beginning to become known as a science. Because before... there were some, at the turn of the century, mostly Catalonians, who got interested... but this growing interest in Psychiatry, which starts with the first psychiatrists of the century -Fernández Sanz, Juarros, Esquerdo and Simarro-, at the level of personal influences, never became something social, of social change, until the appearance of the Mental Hygiene weeks, the publications of the Mental Hygiene League, the minutes of the Congresses of the Neuropsychiatrists Association... We all conducted Psychiatry through the most modern path" (Germain, 1983).

Once the Civil War breaks out in Spain, Germain leaves Spain, spending a season in the Psychiatric Sanatorium of Repond, in Switzerland, where he participated in the first Insulinical treatments for schizophrenia (treatments started by Sakel in Vienna). Later on, Germain moved to Lovaine with Michotte; passing his last two years in Paris with his old masters (Dumas, Janet, Toulouse and Alajouanine), attending to the Psychoanalytic Institute, where apart from the courses, he started a psychoanalytic analysis with Dr. Odier. Upon his return to Spain, "I found no panorama. Till 1952 I couldn't return to Psychotecnica, and towards Health I have never returned!. The psychiatric panorama was impoverished" (Germain, 1983).

For Marañón (Germain, 1965), Lafora, along with Simarro, Achúcarro, Sacristán and others, formed part of the so called "Postwar Generation" which broke in Spain, the old traditions and molds. It was the generation that opened up to foreign Clinics (French and German) and was used to scientific journals that managed to penetrate every now and then. It was a generation that created "schools", without any official support, with the simple attractions that its qualities could offer. That's why the

students that took part, shared their formation with the authentic masters of the Faculty, absent from the academic world. We think that in spite of their youth, to this generation we can incorporate the names of Emilio Mira and José Germain, two of our Psychologists of a major international projection.

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Rafael Lorente de Nó: A Spanish contribution to the neurophysiological explanation of complex psychological processes

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ABSTRACT

The aim of this paper is to analyze the scientific significance of Rafael Lorente de Nó's work, as well as his influence on the development of modern neurophysiological theory. This is carried out in three main steps. In the first place, an outline of Lorente de Nó's overall neurophysiological contribution is presented. Secondly, his influence on D.O. Hebb's neuropsychological views is assessed. Finally, his general impact on Social Sciences is established. The recent death of Lorente de Nó (1902-1990), an eminent member of the Spanish Neurohistological School, seems to us a timely occasion to raise some important aspects of his psychological significance.

The neurophysiological contribution of Lorente de Nó

During the first third of the 20th century, an important Neurohistological School headed by Santiago Ramón y Cajal (1852-1934) and composed of his collaborators and disciples was consolidated in Spain. Among its members, the following stand out: Pedro Ramón y Cajal (1854-1950), Domingo Sánchez Sánchez (1860-1947), Jorge F. Tello Muñoz (1880-1958), Nicolás Achúcarro Lund (1880-1918), Pío del Río-Hortega (1882-1945), Fernando de Castro Rodríguez (1896-1967), and Rafael Lorente de Nó (1902-1990). Most of these authors worked within the limits of anatomic research. However, Fernando de Castro and Rafael Lorente de Nó -the last two direct disciples of Cajal- were responsible for the emergence of a physiological approach within the School.

The main Cajalian assumptions (localizationism, neural communication, developmental plasticity) were transferred by Lorente de Nó from neurohistology to neurophysiology, thereby achieving a new model for the functional organization of the

neocortex, which was to place him among the forefathers of contemporary neurophysiology (Castro, 1981; López Piñero, 1985).

Lorente de Nó's research evolved from the cytoarchitectonics of the cerebral cortex and the confirmation of the neuron theory to the electrophysiology of nervous conduction. His findings on the acoustic area and vestibular system were described in several classical studies which are still influential in contemporary neuroscience.

Standing out among his contributions is his proposal of the concept of "elemental unit". This concept is based on the Cajalian notion of functional systems or "isodynamic groups of neurons" in the visual cortex which are specifically activated by elementary sensory impressions, and it is meant to designate a vertical cylinder or column of cortical tissue whose central axis would be formed by a specific afferent fibre containing all possible classes of cells capable of operating the process of nervous transmission from the afferent fibre to the afferent axon.

One of Lorente de Nó's first original findings was his general anatomophysiological description of the acoustic system. Primary acoustic nuclei form a minute system which has its own "brain" -the dorsal nucleus; the function of these nuclei is to select the transmission of impulses resulting from the activity of long and short chains of superimposed neurons. Vestibular nuclei and reticular substance form together a huge system of chains of intercrossed neurons. Superimposed on this system there are parallel chains containing cerebellous neurons.

However, Lorente de Nó's greatest contribution is specifically associated to his ideas on the nature of synaptic transmission in the central nervous system, including a suggestive outline of the evolution of neural action, in which the neocortex is formed by a large network of "closed or reverberatory paths" (Figure 1). This neurophysiological model was to have a great impact on the future development of neuropsychology and neurocybernetics by virtue of two fundamental principles: 1) the intimate relationship between the reverberatory action of synapses and their structural changes; and 2) the perfect correlation between behavior and neural functioning. From this point of view, learning would simply be the process of bringing up to date a complex evolutionary process characterized by its neural plasticity.

Finally, let us point out that Lorente de Nó's contributions to present day knowledge of psychological processes and behavior (perceptual processes, learning, memory, motivational processes,...) are extremely wide. In this paper, we will restrict ourselves to the analysis of a particular dimension of his impact on contemporary science: his influence on psychophysiology through the work of Donald O. Hebb (1904-1985), the Canadian psychologist, one of the most eminent 20th century theorists in the field of behavior and brain functioning.

Lorente de No's influence on the neuropsychological conception of D.O. Hebb

The endowment of complex human processes -cognitive, motivational and emotional processes- with a strong neuropsychological foundation is probably one of

Hebb's most significant contributions, and it is this achievement that links him with Lorente de Nó.

In his book *The Organization of Behavior. A Neuropsychological Theory* (1949), Hebb aimed at a general theory of behavior in order to throw a bridge between Neurophysiology and Psychology. According to him, understanding behavior is a matter of understanding the overall action of the nervous system, and viceversa (Hebb, 1949). The very title of this first theoretical systemization -his most important work, according to most specialists- shows his interest in highlighting the organization and structuring of behavioral processes. By so doing, he strengthened a new orientation in neuropsychological theory with two main characteristic features: centrality and structurality -two features which go well beyond the narrow limits of the behavioristic frame (Carpintero, 1989).

In brief, Hebb's 1949 theory stated that any frequently repeated specific stimulation leads to the development of a "cell-assembly", a diffuse structure including cells from the cortex and the diencephalon (and maybe also from the brain's basal ganglions) capable of acting as a closed system for a short time. This cell structure produces a facilitation of other analogous systems and, in general, a specific motor facilitation. A series of such events constitutes a "phase sequence": the process of thinking. Any cell-assembly may be activated by a previous assembly and/or a sensory event. Central facilitation of one of these activities on the next is the prototype of "attention". According to Hebb (1949), the answer to the question of the directionality of thought, which became a must since Humphrey's 1940 in-depth review of it, lies in this central facilitation and in its various relationships with sensory processes.

There certainly is a striking parallelism between the above mentioned original statement by Lorente de Nó's and Donald O. Hebb's own elaboration. The similarities between both views may be readily seen by comparing the diagram of a closed path in the human cortex according to Lorente de Nó (Figure 1) and the diagram used by Hebb to outline his notion of a "cell-assembly" (Figure 2).

Hebb believes that this type of cortical organization is essential to adult human behavior in a state of wakefulness. He also thinks there is an "inner" alternative organization in infancy and in sleep, consisting in a hypersynchrony in the activation of cortical cells. In addition to these two forms of cortical organization there may also be a disorganization, for such cell-assemblies depend solely on a very delicate temporal regulation which may be affected both by metabolic changes and sensory events not matching preexisting central processes. When such a disarray is just transitory, we are facing an emotional disorder. When it is chronic, however, we are facing a neurosis or a psychosis. According to Hebb, this theory is a form of connectionism of the "switch" or telephone exchange variety, even though it does not postulate direct connections between afferent and efferent paths. Indeed, this is not a psychology of the S-R type, if by R we mean a **muscular** response. On the contrary, connections help to establish central autonomous activities which will be the basis of future learning.

As a paradigmatic example of Hebb's theory, let us consider his view of motivational processes, a conception which evidences Hebb's outstanding contribution to the emergence of incentive based approaches in motivational psychology as well as the current vigor of his theory (Mayor *et al.*, 1987).

The basic hypothetical variable -the phase sequence- is of a physiological nature, a fact revealing the important role played by biology in Hebb's work. On the other hand, expectations are also emphasized as a characteristic feature of motivation relevant phase sequences, implying that a large quantity of learning is involved in motivation processes (Hebb, 1949). Motivated behaviors -that is to say, as Hebb interprets them, adaptive attack, escape behavior and so on- are only determined by emotion after a learning process has generated an "organized phase sequence". Even in the case of a biological motive such as hunger, the motivational variable is of a fundamentally acquired nature and is related to previous experience.

Hebb's theory, therefore, really goes beyond the merely homeostatic motivational hypothesis of the need reduction model. This point was later developed by his disciples at McGill University, but it may already be seen in the above mentioned work by Hebb. In his analysis of hunger motivation, for example, food deprivation is of course involved, but it is not the only, nor even the main motivational factor at stake. Stomach contractions and level of sugar in blood are indeed associated to hunger, but in no simple way. Initially, lack of food only determines a disorganized, fortuitous behavior, which is not in itself motivated. Only after a learning process has taken place, an organized phase sequence emerges, that is, a motive as Hebb understands it. Likewise, in sexual motivation, an organized phase sequence of an acquired nature appears, the presence of certain hormones in the blood stream being only an added condition.

For the purposes of the present study, it must be emphasized that this theoretical and research line opened by his 1949 book would be hardly conceivable without Lorente de N6's findings and their significance for Hebb's neurophysiological theory. Indeed, with a generosity that gives him credit, the importance of Lorente de N6's contributions for his own work is acknowledged by Hebb himself: both his 1949 book and his *Essay on Mind* (1980) -his last, where his retrieval of the concept of mind may be said to culminate-, as well as his well known *A Textbook of Psychology*, are all indebted to Lorente de N6's ideas. In this latter work, Lorente de N6 is mentioned in many occasions, and is described as "the distinguished neuroanatomist and physiologist to whom we owe most of our knowledge of these matters" (referring to "cell-assemblies") (Hebb, 1958, p. 69).

In this and other works, Hebb even goes as far as to attribute to Lorente de N6's physiological demonstration of closed circuits -which had been anatomically described by Ramón y Cajal- the overcoming of the **impasse** in which the theoretical development of the physiological basis of complex behaviors found itself in the late 1930s (Hebb, 1958, p. 227; 1980a, p. 292; 1980b, pp. 9, 34, 84-85). In his autobiography, moreover, Hebb tells that, in raising the crucial issue of describing a concept in terms of neural mechanisms, his thoughts "stalled, partly because, like everyone else, I was still

thinking of the brain as a through-transmission device and partly because of difficulty in reconciling the facts of learning (which must be localized in specific synapses) and the facts of perception (which, it seemed, is not localized). I had given up thinking about the problem for two years or so, when Hilgard and Marquis (1940) drew my attention to Rafael Lorente de Nó's work and led me to write *The Organization of Behavior* (Hebb, 1949), which contained a theory quite different from any of my earlier ideas" (Hebb, 1980a, p. 292).

Lorente de Nó's influence on social sciences

The remarkable influence exerted on the scientific community by this eminent Spanish researcher may be objectively assessed by considering the number of references he receives in social sciences journals between 1964 and 1988. The following bibliographic repertoires have been taken into account as data sources: *Science Citation Index (S.C.I.)* and *Social Sciences Citation Index (S.S.C.I.)*. The high number of references in the *S.C.I.* places Lorente de Nó nearly at the same level of impact as D. O. Hebb himself, and well above many other authors -some of them as relevant as A. R. Luria. Lorente de Nó gets many more references in the *S.C.I.* than in the *S.S.C.I.*, a fact that may be accounted for the also greater number of medical journals -Lorente de Nó's professional field- of the former repertoire.

However, it should be taken into account that Lorente de Nó's greatest influence took place in the 30s, when his most important works were published. These works are also those receiving the largest number of references within the period considered (1964-1988), a period corresponding to Lorente de Nó's last years, when he was an old and sick man and his productivity declined. In the 60s, his work was somewhat neglected, although it attracted new attention in the following years. The highest quotas are found in 1977 and 1978, also the years in which both repertoires take into account a greater number of journals.

The attention paid to Lorente de Nó's work by the scientific community is particularly focused on a small number of writings, namely the following: "Studies on structure of cerebral cortex: continuation of study of ammonic system" (1934), "Vestibulo-ocular reflex arc" (1933), "Anatomy of eighth nerve: General plan of structure of primary cochlear nuclei" (1933), "Physiology of the nervous system" (1949), "The studies on structure of cerebral cortex" (1933), "Analysis of activity of chains of internuncial neurons" (1938), and "Action potential of motoneurons of hypoglossus nucleus" (1947).

The first three studies are very significant of Lorente de Nó's impact on the international scene, particularly in the context of the social sciences. They amount to 39% of the total references received by this author in the period between 1964 and 1988. "Studies on structure of cerebral cortex: Continuation of study of ammonic system" (1934), currently acknowledged as a classic, accumulates nearly 25% of total references. The second most cited work, "Vestibulo-ocular reflex arc" (1933), constitutes a true transition from histology to physiology. In the third place, "Anatomy

of eighth nerve: General plan of structure of primary cochlear nuclei" (1933) is also acknowledged today as a classic.

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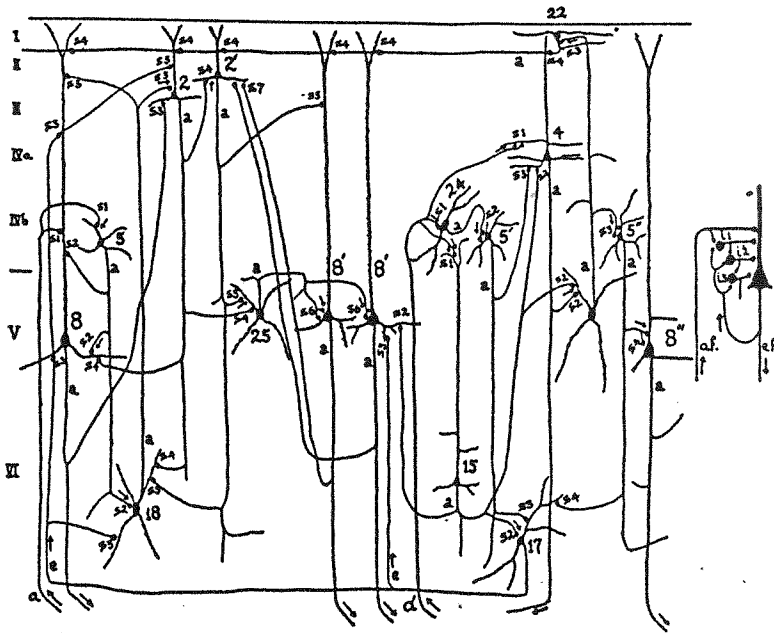


Figure 1. Diagram of closed path in the human cortex (Lorente de N6, in J. F. Fulton, *Physiology of the Nervous System*, Oxford University Press).

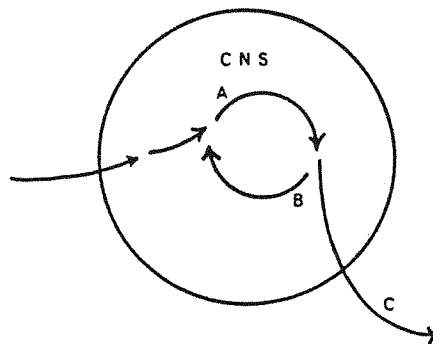


Figure 2. Diagrammatic representation of re-entrant, closed or reverberatory pathway, when incoming excitation excites A, A excites B which again excites A, and so on. The continuing excitation may then be transmitted to motor organs via C (Hebb, 1958).

Abalance on behaviourism after Burrhus Frederic Skinner's death (1904-1990)

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ABSTRACT

Analysis of Skinner's theoretical contributions as well as the goals he intended to reach through his experimental designs.

Nevertheless, the development process of behaviourism itself would meet unforeseen difficulties which would influence both internal criticism (regarding the fulfillment or not of the rules enunciated), and external criticism (in the sense of having excessively counted on the rules propounded and their sufficiency to explain human behaviour).

More specifically, the late articles published by Skinner in American Psychologist are analyzed, where he squarely attacks the cognitive paradigm and other psychological currents.

We conclude by a global assessment of Skinner's works and behaviourism in general. Assessment which is positive for their methodological contributions in the History of Psychology, but much more questionable from a global viewpoint.

INTRODUCTION

Weighing up his own intellectual life, Skinner himself has recognised that his fundamental objective has been that of explaining human behaviour in terms of physiological responses to the environment and to propound the scientific study of these responses in connection with that environment. At the same time, the most devotedly cited authors, particularly in his beginnings, were I. Pavlov and his works on conditioned reflexes, B. Russell's articles on behaviourism, and Watson's ideas on the origin of the paradigm -its founder-.

Always loyal to his motto of being novel and controversial, Skinner devoted one of his late works to the ethical problems of science (**Beyond Freedom and Dignity**, 1971), where he argues that those large concepts (Freedom, Dignity) have been selfdestroyed by both: recent developments in behavioural technology, and advances in the physical and biological sciences. He doesn't mince his words when he calls these eternal problems "nonsense fallacies".

Much in the same way against the emerging cognitive paradigm, would go his late works "Whatever Happened to Psychology as the Science of Behaviour?" (*American Psychologist*, August, 1987), "The Origins of Cognitive Thought" (*American Psychologist*, January, 1989), and "Can Psychology be a Science of Mind?" (*American Psychologist*, November, 1990). It is in these articles where we notice his virulent attacks, even trying to ridicule the vocabulary used, on the cognitive paradigm. (1)

From what has been said above, we consider it not unfitting to set forth a balance on behaviourism's future after the death of its most brilliant defender, B.F. Skinner, whose task has certainly been that of champion of the behaviouristic paradigm for the last 50 years.

Quite a different story is if the paradigm has been able to sustain the assumptions it was based on, and which were listed as irreplaceable.

RISE OF BEHAVIORISM AND INTENDED OBJECTIVES

Palermo (1971) has stated that behaviourism was a Kuhnian revolution against structuralism. Contrariwise Joncich (1968) says that the arrival of behaviourism was, like adolescence, perfectly foreseeable by 1913.

If we take the first view, the "behaviourist revolution" was enormously helped by the success of the studies by Thorndike (1898-1911) and Pavlov (1900) on conditioning. With this revolution a new period started: Psychology would stop being a science of the mind or conscience, to become a science of behaviour. Watson's behaviourism (1913) would repudiate consciousness and similar concepts such as, mental states, imagination, thought, etc. and even methodological concepts such as "cause", "effect", for their lack of objectivity. On the other hand, by pulling out the object of study of Psychology from within the individual and by placing it on the same level as other natural sciences, the methodological approach to Psychology would not need be different from that of the Natural Sciences. This starts the period of operationalism and positivism in psychological experimentation, explicitly leaving out any introspective trace. Watson's article (1913) entitled "Psychology as the Behaviourist views it" clearly defends this position and it is certainly taken as the declaration of principles of the behaviourist school.

After the firm declaration of principles of the young specialist on animal psychology, Watson's ambitious methodological program contrasts strongly with the relative poverty of its results. The whole human behaviour (excepting some simple reflexes) rests on the learning of conditioned reflexes which are considered as elementary units of behaviour. Between 1916 and 1928, the number of works with a behaviourist orientation in "The Journal of Experimental Psychology" rose steadily. Watson's program began to gain adepts influenced perhaps by the expansion that logical positivism of the Vienna Circle (considered as the most valid model in scientific knowledge and theories' conception) was acquiring, becoming the predominant movement in the U.S.A. between 1920 and 1930.

DIFFICULTIES MET DURING ITS DEVELOPMENT

1) *Behaviourism-biologicism-cognitivism framework problems*

Through J.B. Watson (1878-1958) and the rise of "behaviourism" begins a process of change within Psychology which would have strong influences on its later development and shape. Despite the undeniable contributions by psychological schools with different orientations (Gestalt and the Wurzburg school, for example), until very recently almost unanimously Psychology seems to accept (at least implicitly) the general postulates of the so called "behaviourist" or "neobehaviourist" school. The habitual fact was to find most publications in terms of S-R. The main influence of this movement was doubtless in relation to its own conception of the object and method in Psychology. In what refers to the object, behaviourism's influence was evident from the fact that, more or less unanimously, it had been accepted that the object of study of Psychology was precisely the "behaviour" of the organism, disregarding the different connotations assigned to this term.

Due to behaviourist's emphasis on "external behaviour", the processes controlling the appearance and modification of specific behaviours, i.e. learning, became a central focus of attention. The contributions of Psychology in this field are well known (in Basic as well as in Applied Psychology) and form one of the most solid pillars of present Psychology. It was the logical reaction against the mentalism they intended to fight. In the classical behaviourist manual on "General Psychology" learning got about 50% of the text.

In Spain, professor Yela (1980) has analysed with great clarity the evolution of behaviourism from its beginnings until the present, giving five stages (2):

1. Phase of birth and diffusion of behaviourism: Watson.
2. Phase of great theories and systematic behaviorism: Hull, Tolman, Guthrie and Skinner, who share a common methodological objective, but differ on the method's interpretation (inductive-deductive), the nature of learning (connection S-R, S-S associations, expectation) and its underlying acquisition mechanism (contiguity, reinforcement, confirmation of expectation etc.).
3. The crisis phase (between 1950 and 1960) represented firstly by an internal criticism, in the sense of not adequately accomplishing the objective rules it aimed at as a basis (Estes et al. 1954), and secondly, by an external criticism of those rules, in the sense of having excessively believed in them and their sufficiency (Koch 1959, Hebb, 1960).
4. The decline phase of behaviourism: from "systematic behaviourism" to the "Psychology of behaviour". The behaviourist interpretation is rejected, but behaviour is kept as the aim of Psychology. This trend may be clearly seen in the interpretation of learning and conditioning (Estes, 1972; Wagner, 1969).
5. The fall phase of behaviourism, when most psychologists consider behaviour not as the only objective of psychological research, but as one of the routes, though fundamental for most, to verify psychological hypotheses.

However, some authors have gone even further. We find authors as Mackenzie (1977) for example, who have tried to show not only that behaviourism had failed, but that it had no alternative but failure as a result of its internal contradictions.

On the other hand, the failure of neobehaviouristic theories coincided with the crisis of logical positivism as the only model of the conception of theories and scientific knowledge. As a result, at the end of the 1950s, behaviouristic psychological theorisation began to be abandoned, even though it was maintained as the main objective of Psychology. It is the time when the various areas of specialisation within Psychology began to appear, though somewhat on the fringe of the new lines of the Philosophy of Science that were springing up.

Just as a sample, in Psychophysics, authors from skinnerian behaviourism such as Savage (1966) and Zurriff (1972), who tried to criticise classical psychophysics for its foundation on private events, were strongly rebutted by Stevens (1966) and Gerscheider (1976), even though Stevens shared with behaviourism its operational interest. Psychophysics continued its own tradition linked to the study of psychological scales, as the works by Thurstone and Stevens on the new psychophysical methods prove.

What has been said referring to sensation, can also be said about other fundamental areas of the behaviourist paradigm, such as the fields of motivation and animal learning. Behaviourism certainly dominated until very recently, although its opposition by ethologists such as Lorentz, Tinbergen and Hinde, had already begun in the 1950s.

Likewise, with regard to human learning and particularly verbal learning, until the appearance of cognitive psychology the predominant expositions came from functionalism (not behaviourism) through the works of McGeoch (1932), Melton (1940), Underwood (1957) and Postman (1961) on forgetfulness and interference theories, while the behaviourist attempts of the school of Hull's and Spence's did not have any lasting effects.

With regard to other areas of influence and application such as Social Psychology and Evolutive Psychology, the behaviouristic point of view was never really predominant. The enormous influence of Piaget's work on Developmental Psychology must be remembered, and that of Festinger, Schachter and Kelley on Social Psychology, both formulated and prevailing during the years of the official reign of behaviourism.

Seen from within, the theoretical crisis of behaviourism (obvious in almost all areas of specialization) led to the open rejection (by some behaviourist psychologists) of all types of theorisation which considered that the progress of psychology was due more to the accumulation of facts than to the verification of "hypotheses".

However, for other psychologists the new situation eased the expansion of behaviourism's conceptual framework towards new models, as a result of the research they were carrying out.

As McKenzie (1977) and Leahey (1985) (3) have pointed out, the rejection of all theorization (represented in Psychology by the Skinnerian (behaviourist) movement, still strong after the 1960s), had to face the criticism of the new epistemological conception of the Philosophy of Science, which appeared as criticism of logical positivism (Suppe, 1977, 1979; Pinillos 1980; Asquith and Kyburg 1979).

Authors such as Toulmin, Kuhn, Hanson and Feyerabend had emphasized "the context of discovery", that is, that even within the process which governs all science (even the sciences which reject the theory), this process is carried out from within a conceptual framework which determines, to a great extent, what problems deserve to be studied, and what kind of solutions are acceptable.

The works on learning written during the 1960s by behaviourist psychologists, added proof to the fact that learning laws brought to light with rats and doves, could not be extended not only to humans, but to other animals of the same species.

During these years, and while Skinner was developing his dove-guided missile, the young Breland couple, who had become professional trainers of animals, collaborated with him. In 1961, this couple made public their difficulties in an article entitled "The Misbehaviour of Organisms" (playing with Skinner's own title *The Behaviour of Organisms*). In their article, the Brelands insisted that the hidden premises of the animal's organism should be examined, that animals are not a virtual "tabula rasa" to receive all types of learning, and that not all responses are conditionable to the same degree. The collaborators sincerity was breaking the master's "dogmas".

In the same line, John Garcia, a follower of Krechevsky (himself a follower of Tolman), proved that rats, avoiding conditioning's classical laws, "knew" almost "instinctively" that their vomiting, in spite of the temporal lapsus, had been brought about by a pleasant liquid, which led them to "avoid" it (1966) (4). The classical principles of behaviourism, as it happened with the Brelands, were crumbling in the hands of "the biological" (Leahey, 1985).

On the other hand, behavioural biologists tended to consider morphology and behaviourism as adaptations evolved and programmed by genetic mechanism. Fixed action patterns and the specific behaviour of species give relevant examples of this type of analysis. The controversy behaviourism-biologism has moved within these two extreme lines of explanation of behaviour.

Nowadays, a certain eclecticism has gained ground; extreme positions have become more flexible, and there is a mutual acknowledgement of the importance of biological and environmental factors of behaviour (Kimble, 1981).

Among the studies which have greatly contributed to the acknowledgement of the importance of biological factors, particularly of the genetic endowment of the species, we should mention the research (carried out at the University of Granada) on the biological predisposition for the acquisition of certain responses (5).

The imprinting phenomenon (Horn, 1979); gustative learning or "Garcia's effects" (Garcia et al. 1966; Puerto y Molina, 1980; Gallo, 1981; Arnedo, Molina y Puerto, 1982); the autoshaping phenomenon (Brown and Jenkins 1968; William and William, 1969; Honing and Staddon, 1977; Locurto, Terrace and Gibbon, 1980; Tudela, 1981); as well as the study of the rhythmic features of behaviour are a result of a series of internal biological oscillations which impose serious conditionings to external behaviours (Carthy, 1974; Puerto, 1982; Vila and Fernandez, 1982).

All of these led to a progressive reentry of the biological framework, which had been practically excommunicated from psychology for several decades.

Authors such as Seligman and Hager (1972), Hinde (1973), Bolles (1970, 1975), Garcia (1966, 1981), Dickinson (1981) and Hebb himself (1960, 1980) who as followers of K.S. Lashley had always maintained an open behaviouristic attitude, decisively contributed in recent years to the return of the biological framework into psychology (Schmitt and Worden, 1974; Gazzaniga and Blakemore, 1975; Uttal, 1978; Cotman and McGauch, 1980; Puerto, 1981); Lamarck and Darwin, so crucial to the emergence of psychology, had finally made a come-back.

2) *The crisis of behaviourism around 1950*

Around the 1950s, however, a period of option and change for behaviourism began (Leahey, 1982). The theoretical explanation of reinforcement advanced by Hull (1943) in terms of reinforcement of associations (S-R) by reduction of "drive", came into a crisis in the 1950s by Hull's own hand. The "drive" within Hull's system was conceptualized as an "intermediate variable of motivational character" exclusively related to the energizing aspects of behaviour; the "drive" did not exert any type of directive control on behaviour; its function was to promote behaviour allowing for the performance of the dominant response within the hierarchy of associations S-R, and this was a direct attack from within, on the Watsonian principles. Likewise, the results of research on "latent learning" (Blogett, 1929; Tolman, 1932; Muenzinger and Conrad, 1954) and "change of incentives" (Crespi, 1942; Zeaman, 1949), where the presence of cognitive and motivational factors, not reducible to the concepts of "drive" and habit reinforcement", was demonstrated.

All these forced Hull to modify his point of view, and to introduce a new concept in his system: "motivation by incentive". The concept of "motivation by incentive" was a recognition of Tolman's (1932) previous viewpoint about the existence of "goal expectancies", "cognitive maps", etc, which direct and regulate behaviour.

The reelaboration of the concept of "incentive" (Leahey, 1982), instead of placing the emphasis on the internal conditions of the organism (biological needs), placed it on the external aspects (features of the reinforcer), stressing the pull exerted upon the organism by specific external stimuli against the push aspect characteristic of drive. Therefore, it was necessary to postulate a mechanism different from impulse, and capable of explaining the anticipatory effect of reinforcement and the apparent

purposiveness of behavior, as Tolman had anticipated. Although such explanation does not seem easy within a behaviouristic framework only endowed by associative and energizing factors, Hull's school, mainly Spence (1956) and Logan (1960), believed that they had found the answer in the so-called "fractional goal anticipatory responses", which were supposed to be classically conditioned. Within this perspective, the concept of incentive contained three assumptions (Hilgard and Bower, 1976):

1. The incentive's effects are motivational-energizing. Its performance is similar to that of drive.
2. The incentive is based on the appearance of fractional goal anticipatory responses, or on any reaction which takes place in the black box. These responses give rise to interoceptive stimulation, thus forming the unit Rg-Sg.
3. Fractional responses are classically conditioned by situational stimuli.

All these assumptions are far removed from the initial classical postulates of the behaviourist paradigm. A stronger paradigmatic revolution would take place in behaviourism in the following decade, after the "informal behaviourism or neohullian psychology" in the words of Leahey (1982). Starting in the 1960s, experimental research on learning began to seriously question the basic suppositions of behaviourist formulations of the principles and laws of learning. In particular, experimentation on learning, by itself, would remove:

1. The assumption of association by temporal contiguity between stimuli and responses, or between responses and reinforcers as the fundamental variable in associative learning.
2. The assumption of reinforcement as strengthener of S-R connection, or the probability of occurrence of responses.
3. The assumption of the universality and generality of the learning principles studied in a limited number of animal species.
4. The assumption of the total passiveness of the organism during the learning process, considered as a mere receptor of the S-R connections.

As we shall see below, criticism of such assumptions came from the study of a series of learning phenomena in the fields of classical and instrumental conditioning, which were not suitable for the traditional behaviouristic explanations.

Regarding the first assumption (very shaky, following the paradigmatic crisis around 1960), and within the context of classical conditioning, the crisis of the traditional point of view was brought about by a series of findings related to the role of contingency, between CS and IS, against that of "contiguity" (Rescorla, 1968; Mackintosh, 1974; Dickinson and Mackintosh, 1978; Rescorla and Holland, 1982), the study of "compound stimuli conditioning" particularly the blocking phenomenon (Kamin 1969; Rescorla and Wagner, 1972; Mackintosh, 1978; Rescorla, 1980), the study of "latent inhibition phenomena" and "sensory preconditioning" (Wagner, 1978; Webb, 1981), and the study of "aversive gustative conditioning" (Garcia et al. 1966, 1977; Seligman and Hager 1972; and applications of it, in Spain: Puerto and Molina, 1980; Arnedo et al. 1982).

As for the second assumption, reinforcement as a strengthener of S-R connections within the context of instrumental or operant conditioning, the new theoretical orientations appeared around the study the phenomenon of "autoshaping" (Brown and Jenkins, 1968; Williams and Williams, 1969; Boakes, 1977; Jenkins, 1977; Herrnstein, 1977; Schwartz and Gamzu, 1977; Terrance and Gibbson, 1980), the "Premack principle" (Locurto Premack, 1959, 1971; Timberlake and Allison, 1974; Timberlake, 1981), the "superstitious behaviours" (Skinner, 1948; Staddon and Simmelhag, 1971), "avoidance learning" (Bolles, 1972, 1975; Heline, 1977; Seligman and Johnston, 1977; Mineka, 1979), and "learned helplessness" (Overmaier and Seligman, 1975; Maier and Seligman, 1976; Maier and Jackson, 1979; Seligman and Weiss, 1980). (5)

Similarly, in the area of motivation the "cognitive theories on emotion and incentive", somehow derived from Tolman's initial propositions, acquired special relevance (Rescorla and Solomon, 1967; Solomon and Corbit, 1974; Solomon, 1980; Bindra, 1969, 1974, 1976; Mandler, 1975; Dickinson and Boates, 1979; Plutchnik, 1980).

The assumption of the universality and generality of the learning principles would meet new expositions following the changes operated on the conceptualization of learning and motivation, referring to the acceptance of the importance of biological and cognitive factors in the explanation of behaviour; factors which had been ignored or minimized in orthodox behaviourist and neobehaviourist models. In the case of classical conditioning, for example, the new cognitive interpretations reject automatic association by temporal contiguity of stimuli as the learning mechanism. Instead, such a mechanism is accepted as a perceptive process of predictive relations among stimuli.

What classical conditioning would do, would be to provide information on the probability of occurrence of a biologically important fact (the IS), starting from certain situational traces (the CS). The new cognitive interpretations of classical conditioning would approximate this type of learning to the cognitive models of information processing and the theories of knowledge representation (Wagner, 1976, 1978; Dickinson, 1980; Rescorla, 1980; Aparicio, 1982).

The fourth assumption, the passiveness of the organism in the learning process, would similarly crumble down in what refers to biological factors, as research would prove the existence of behavioral variables with many shapes, which seriously limit the basic behaviourist assumption of universality and generality of the learning rules, and which force us to take into consideration the importance of the species' genetic endowment as well as the intraindividual biological changes of cyclical type (biological rhythms) that organisms display (Seligman and Hager 1972; Hinde and Stevenson-Hinde 1973; Carthy, 1974; Uttal, 1978; Horn, 1979; Staddon; Kimble, 1981).

As a definition of Psychology as **the science of activity** would entail, what is gradually being stressed from the biological and cognitive perspective, is the active role of the subject (the organism) as mediator between the stimuli and the response (Pinillos, 1980; Yela, 1982; Mayor, 1982, 1985; Fernández Trespalacios, 1986). (6)

The analyzed evolution, in contributions as well as in its difficulties, has been carried out "from within". We haven't searched for the critics of the paradigm, but for the internal difficulties that behaviourist research itself has found, colliding with its own theoretical assumptions. Most of the authors read in preparing these lines are but the "crème" of the 25 Group of the American Psychological Association ("Experimental Analysis of Behaviour"). As Marx pointed out referring to future expectations of capitalism, capitalism would fall from "its own internal contradictions". The problem is that this has happened precisely to so-called scientific communism and not to capitalism.

Something similar has happened to the behaviourist claim of wanting to clarify all the wonderful complexity of living beings through the external analysis of behaviour; an enormous amount of behaviouristic aspects have been revealed which are inexplicable by behaviour itself.

SKINNER'S ATTACK ON THE COGNITIVE PARADIGM AND OTHER PSYCHOLOGICAL TRENDS

In August 1987, Skinner would take the psychological community by surprise with the furious article "Whatever Happened to Psychology as the Science of Behaviour" (7), where he does not mind describing the other paradigms which interpret and practice psychology in contexts not shared by behaviourism, as "obstacles". He names and criticizes Humanistic Psychology, Psychoterapy and Cognitive Psychology.

In the article, Skinner distinguishes between "early" and "radical" behaviourism. He acknowledges the debt of the former period to Darwin, Lloyd, Morgan and Watson, as the links of a gradual concern for the behavioural theme perhaps, as a reaction against "the heavily mentalistic psychology of the time". He places and organizes the future evolution of the current around Watson, of course, who tried to replace "instincts" with "habits", and around the works of Lashley on the nervous system. Without calling them dissidents, he dispatches Tolman with the laconic phrase "Tolman restored purpose to the organism and still later installed cognitive maps and Hypotheses". Similarly he refers to C. Hull as the builder of an elaborated system of internal processes, such as the "afferent neural interaction", which would reach a powerful physiological content. In short, Skinner charges these authors with having left psychology in the mentalistic and behavioural debates of Homer's time, some 3.000 years ago. (8)

With regard to "radical" behaviourism, Skinner acknowledges H.S. Jennings (1906) and J. Loeb (1910) as the continuators of this current, particularly for the formulation of tropisms to explain responses as a whole issued by the organism, summing up what E. Mach, P.W. Bridgman and B. Russell, had been requesting within the framework of Philosophy of Science. These currents would converge in his PhD thesis *The Concept of the Reflex in the Description of Behaviour* (1931). It was nothing internal to the organism but the rules of its responses to stimuli what gave the right explanation to what the organism did. These were the third variables, those called by Tolman "intervening variables". (9)

The path set upon by this “radical” behaviourism is seen by Skinner as an escape from “verbal contamination”, a departure from self-observation, a progressive analysis to “ignore consciousness”, explaining everything as a gradual study of the history of reinforcements and the inherent consequences of them in every living being. This radical behaviourism would find the keystone to change psychology into an authentic science, being also responsible of its great development for decades.

Nowadays, however, three obstacles against the science of Psychology stand out in Skinner’s opinion. Let us examine each in more detail.

The first is “Humanistic Psychology” (10). Just trying to establish the concept of “processes” instead of “contingencies of reinforcement” entails a radical difference. Likewise with his interpretation of life, already far from Darwinian evolution principles which have inspired Psychology from its outset, “It also seems to threaten ethical, religious, and governmental systems that hold people responsible for their conduct. Who or what is responsible if unethical, immoral, or illegal behaviour is due to heredity or personal history? Humanistic psychology has attacked behavioral science along these lines. Like creationists in their attack on secular humanists (with humanists on the other side), they often challenge the content or selection of textbooks, the appointment of teachers and administrators, the design of curricula, and the allocation of funds”. We think those lines speak by themselves, indicating how Skinner lived in his last years the battle we would call “paradigmatic”, between behaviourist and humanistic psychology.

The second obstacle for the science of behaviour is, according to Skinner, psychotherapy, in the sense that some demands of professional helpers are becoming hindrances for the scientific analysis of behaviour. For Skinner the dialogs of the psychotherapeutic relation turn out to be ridiculous, as well as the privacy of relations that may be established within their context: “Psychotherapists must ask people what has happened and how they feel because the confidential relationship of therapist and client prevents direct inquiry” (11). But he keeps his stronger fire for the psychoanalytic current, when he states that “It is not suprising that they should then construct theories in terms of memories, feelings, and states of mind or that they should say that analysis of behaviour in terms of environmental events lacks “depth”. (11)

But undoubtedly, and taking the above references to humanistic psychology and psychotherapy as skirmishes, Skinner aims his heaviest fire against “Cognitive Psychology”. The gradual appearance of works in this line, the subsequent exponential growth of publications, particularly after 1950, and the widening areas of study which seem to adhere to this current made him ask: “Is there any field of psychology today in which something does not seem to be gained by adding that charming adjective to the occasional noun?”. (12)

The return of the subject, the return of the mind, are but his sweetest expressions for this paradigm. Information processing theory seems to present the problems as though they “vanish like magic”. Mentalistic introspection desperately turns “to brain

science, asking it to tell them what perceptions, feelings, ideas, and intentions really are!" (10).

The new discipline, called "Cognitive Science", receives the hardest blows; logicians, mathematicians and computer scientists connected with it, are treated like the psychologists who follow this line of research.

But the damage is not yet irreparable, because, "By their very nature, the antiscience stance of humanistic psychology, the practical exigencies of the helping professions, and the cognitive restoration of the royal House of Mind have worked against the definition of psychology as the science of behaviour". (12)

A second paper of B.F. Skinner, "The Origin of Cognitive Thought" (1989) (13), was dedicated to revising the emerging cognitive paradigm. With no other help than a simple English Dictionary, he analyses the usual vocabulary of cognitive psychologists describing the states where emotions, feelings and states of mind occur, to show that, in fact, we are facing behavioural descriptions. When they bodily appear and become information capable of being transmitted by the subject, we use the same names we could use to describe them.

Cognitive Psychology for Skinner is just a return to the mentalistic vocabulary of philosophy, which experimental analysis had taken pains to bury. A simple etymological analysis would sustain his propositions. The verbal contingencies of reinforcement would explain this field better than introspective observation. Body conditions are not the cause of behaviour but collateral effects of the causes. According to Skinner, people spontaneously pose the questions in the "external" field. Science requires a language for those external realities and behaviourist analysis is the only road to achieve it.

The last paper bequeathed by B.F. Skinner "Can Psychology Be a Science of Mind?" (1990) (14), has followed the same line. Introspection was never a satisfactory path, Psychology sprang as a science to overcome the philosophical discourse, the human mind (if it is "what the brain does") belongs entirely to the terrain of physiology as far as the brain is concerned, and to psychology as far as behaviour is concerned.

As it appears nowadays, the behaviour of organisms is the final product of three types of variation and selection: first, natural selection in a Darwinian sense; second, the variation and selection carried out by operant conditioning, the role of reinforcement in an individual as well as in a social sense. (Perhaps these respectful lines by Skinner on the process of "imitation" come a bit late. Bandura and Walters would have liked to have found them a decade ago). The third, variation and selection "fault", would be determined by the contingencies of the accidental selection. The intertwining of the species in a vital as well as cultural sense, turns out into a box full of behavioural surprises. (15)

To establish a science centered on the study of this "body-cum-brain", as Skinner likes to repeat, we have but the following possibilities: Physiology as the study of

organs, cells, tissues, and the chemical and electric changes in them, or the sciences pertaining to the variation and selection which take place in those organisms (Ethology, Behaviour Analysis, and partly, Anthropology). There is no possible place for that "science of mind". "Cognitive Psychologists try to restore the status quo. Behaviourism, they declared, was dead". (16)

It is obvious to conclude that Skinner lives the clash cognitivism-behaviourism as radical for the history of Psychology.

NOTES

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- (5) Mora, J.A. (1986) *Psicología Básica*. Madrid: Narcea. Specially pages 99-100, as well as 167-199.
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- (8) Ibidem 783 to the end.
- (9) Jennings, H.S. (1906) *The Behavior of the Lower Organisms*. New York: Columbia University Press; Loeb, J. (1916) *The Organism as a Whole from a Physicochemical View Point*. New York: Putnam.
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- (11) Ibidem note 7, 784.
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- (15) Ibidem, 1207.
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On the crossroad to Europe -Psychology of the lost identity of Bulgarians

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"Something elusive of definition- the body of the East but without its spirit...a traitorous deserter from itself".

That's is how we were seen us in the early Twenties by the Grand Rabbi of Bulgaria Marcus Ehrenpreis. In a few words he concentrated the main characteristics of our people- always running "from" or opposing something which finally appears to be its essence, unpredictable and vague even for its own longings. Trying to proceed forward by stepping backward. Bulgarian identity lost in contradictions. The problem is even greater now when, for half a century, the Bulgarians were forced to bring to life a socialist utopia and to conform traditional moral values to the imposed Communist morals. Out of this has come a total loss of the sense of reality and the ability to judge, a split mentality and a feeling of hopelessness.

In this paper I aim at uncovering some of the reasons for that psychological crash. They may be generally summed up in two main groups -internal and external, inborn and imposed ones. The first are to be referred as "the Bulgarian model", the second as "the kingdom of words".

The bulgarian model

This is a state of mind. A special way of thinking. A type of mentality. To put it simply -an opposite to everything (even our gesture for "yes" is in a way what for other people means "no"). In the Bulgarian model soldiers have never lost a battle and generals have never won a war. Throughout the world the Great is one step distant from the Ridiculous; in the Bulgarian model there is no difference. It is worse than the Bermuda triangle. In the Bermuda triangle things just disappear, and are no more to be seen. In the Bulgarian model things are obvious and here, but they are not what they seem to be. And no one knows what they really are. A model of paradoxes. Historically affirmed psychology of uniting contradictory values and entities- childish naiveté and paternal naughtiness, inferiority complex and sense of uniqueness, uncertainty and obstinacy. People who try to find their way to Europe via Bosphorus or the USA. People, who cannot find where they really are -the place they belong to. Spiritual emigrants at home and homesick patriots abroad.

"The type - writes M.Ehrenpreis, - is psychologically and socially truly 'a wavering form', a composite of Easterner and Westerner...In a spiritual sense these creatures are homeless; they are no longer Orientals nor yet Europeans. They have not freed themselves from the vices of the East nor acquired any of the virtues of the West...Happiness seems absent from these regions and all things appear equally neglected and impoverished".

Because of that mentality everything in the Bulgarian history seems unnatural and paradoxal. Gradualness, this important characteristic for every normal development, is almost entirely missing in it. So that it appears that the only persistent feature is the absence of any persistency and continuity. Everything is just quick leaps and violent uplifts followed by much deeper falls and periods of absolute immobility and feebleness. One example: in the Middle Ages there existed two Bulgarian kingdoms - both failed; in both cases lasting slaveries followed. Yet, the most amazing thing is that, half a century ago, the same thing happened to the Third Bulgarian kingdom!. Now we are again at the beginning. As though there had been no life, no history!. We begin "anew". And again on the well-known model. Unfortunately for the essentially controversial Bulgarian mind, this time the splitting caused by the dogmas of socialist ideology was added.

Having in mind that consciousness is a linguistic continuum, I will try to explain it through a kind of language analysis. Most recent research in linguistics has introduced the term "homo Balcanicus". It was adopted by psychology in order to describe the binary thinking characteristic of the inhabitants of the Peninsula. That same thinking which in antiquity gave birth to mythology and which, for Aristotle, was supposed to be "a paradigm of some common law", appears as a basis for ideological manipulation. To put it in Lévi-Strauss' words, "Nothing is so similar to mythological thinking as political ideology -ideology has just replaced mythology".

Socialist Ideology

The leading principle in revealing its essence can be defined as "socialism as logocracy" or "the system as a kingdom of words". Through the help of it, it is made obvious that the "new world" is not a new social order, but a kind of fictitious

world opposed to reality: created in man's mind by the means of words as signs and allusions. Going out of this world results in a psychic crash, because everything in the real world is vice versa to the model shaped in the mind.

The "word-strategy" defines objects by signs signifying the opposite to their essence. For them are created special dichotomic structures in which they are bound to antonyms aimed to strengthen the effect. The artificial sign-system is -in fact- meant only to hide the real essence of things and to misinform. As it comes out, this part of the binary opposition -the antinomy which is put to criticism and rejection, equals the real meaning of the word. This is the language of the illegal structure (the protostructure). It exists, but it must never be understood; once it has been understood, the system is over.

To illustrate this structure, let me give one well-known example from the history of culture:

The name Lucifer as a word means "the one who brings light". Yet it stands for evil as a synonym of dark. In this way the word itself is opposed to its meaning:

Lucifer= dark <-----> light
(light)

This paradoxal usage of a word to signify something contrary to its etymological meaning forms the basis of ideological speech. The way it is produced can be easily decode, if we apply the method of associative chain.

ANGEL ---> Lucifer ---> Light ---> Sun ---> Fire ---> Hell ---> Dark ---> Evil ---> Devil

As we know, the construction of such a chain showing all the gradual steps between the two ends, reveals the process of creating metaphors. But it presupposes thinking and creativity; yet, thinking and creativity do not exist in ideology where allusion reigns. With allusions things are simple: black or white, evil or good, Devil or Angel. Devil as opposed to Angel. Evil as opposed to good. Black as opposed to white. No shades. No steps in between showing the gradual transformation from one to another -consequently, their associative relationship, not opposition! And like that they are imprinted in the mind with the help of devices which just *show*, but do not *prove*.

Characterized by the obscurity of taboo and the simplicity of allusion, the protostructure reaches people's mind through mechanical devices of ritual-conjuring perloquitivity (repetitions, epithets, images and easily decoded in genetic aspect stereotypes) which lead to psychic break. In this way the new myth is created -one morbid psychic mixture of the most archaic subconscious and some modern ideas about a future society of fraternity and freedom. "Words invalidate reality" - the absurd logic of the famous maxim of F. Bacon now turns to reflex which destroys the inborn ones and makes our vital reality fall ill. The phrase is "black magic", its creators are nothing but "quacks" and the process of inducing such diseases in society is therefore to be defined as "political shamanism". The result is: splitting of the "patient's" mind. The imaginative world of ideology produces a schizophrenic world which brings up a schizophrenic society and its "artistic" contribution to civilization: schizophrenic man -homo socialisticus. It sounds like a diagnosis. And it really is.

According to the common norms of a rational mind, such a person is impossible. Impossible namely as a person, as Man -human, social, conscious. This is a creature alienated from the surroundings, closed into himself and his imaginary world. And all this because of the fact that his essence is alienated from the artificially created official world.

The ancient dichotomic model which tried to explain human essence with the help of inherited binary thought, has nothing to do with the binary oppositions of the mentioned type. Oppositions which devastate human mind and through which nature

itself is destroyed, plunged into unreal and artificial antinomies. The most important of them are to be found in the structure mother-father or queen-king, the psychoanalytical explanation of which is well known. They are witnessed in the epithets used for the Party (with capital P!) and its Leader. The images of Party-mother and Leader-father equal the incestuous symbols of son-tyrant and mother-country and form some special kind of cult alphabet. The letters of this alphabet are commonly used *not* for description of the existing reality, but for creation of an irreal one as its substitution.

Due to its hypostases, "real" socialism is not to be separated from the symbols of family. Party, state, society, family -this is the symbolic suggestion of its ideology. The socialist state is nothing but one big and happy family! The national "family", of course, is part of the "international family of socialist states" (as the well-known expression of the propaganda ran); strengthening the impression of kinship and family intimacy of life in the socialist camp are the allusions from the "family circle" like "brotherhood", "brotherly people", etc.

But what happens then to the family which we used to define as "the smallest cell of society"? This family is transformed into its primal form stressing the natural - instead of social- character. Equating sexual experience to a primitive physiological act, the Party tried to achieve its main goal -procreation of children, "sons", as a human mass. For stimulation of the reproductive function the most active part of the population was gathered in camps, summer schools, etc., being in this way separated from the family with its moral restrictions (alluded to as "hypocritical bourgeois morality"). Labour and sex were so fused together, that we can speak of the "economics of sexuality" (W. Reich).

For conceiving the "new man" -a child and a perfect example of the uniform society- the ideology undertook to build up a new "sexual conspiracy" both by the dogmatic concepts of everyday life and by the stereotypes imposed through education and art.

The Father. Produced by a consciously manipulated collective unconscious, the image of the Father (personified in the communist Leader) does not represent any individual figure, but centers the "sado-masochistic" experience of the masses. Once it has been abolished, the whole society proves to be inhabited by unrealized Oedipuses attempting to accomplish the role of micro-tyrants. On the other hand, the role of the Leader as Pater Familiae hints at the suggestion that socialist state is one big "family" with "Daddy" as its magic center; a fact that proves (after we decode the symbolic meaning of all the elements) that this kind of state is nothing but a form of tyranny. A dictatorship which in the person of the Father embodies the power of the almighty Party-mother.

The Mother. The image of Party-mother is to be found at the top of the value-system. It executes the power in an absolutely male form (tyranny), which can also explain why in some of the countries overthrowing dictatorship, at the head of the state a female leader might be promoted - one unconscious mechanism at work for soothing the painful experience of tyranny. The exaggerated role of the party as mother is to

be supported by the following example: recent literary research aimed at revealing talent as a function of family background, noticed that in the articles of the Bulgarian Encyclopaedia there was no data about the parents of prominent 20th century Bulgarian poets and writers. Instead of that, everywhere is noted their membership in the communist party. The suggestion of this manipulation is obvious - all gifted people in the country are children of one Mother - the Party!

As socialist ideology has monopolized all values, socialization of people can only be achieved through its mediation. Even husband and wife are first of all "comrades". Consequently, the procreation of family is equated to contribution to the class struggle. Losing in this way more and more their ability to judge and distinguish soundly, a lot of people give up the idea of procreation and receive erotic satisfaction in political life or at the work place. Yet, the crazy idea of absolute control over intimate human life generates dual morality. Finding themselves involved in such a schizophrenic situation, people naturally try to oppose its antinatural essence and close into their own selves. In some morbid state of mind they imagine that the private can be kept apart from the social with its powerful ideological magic. But no! Everything and everywhere is shaped after the imposed Party model. As a result, instead of birth triumph planned to secure abundant generation as "mass", the Bulgarians witness a record decrease in population. The death rate is higher than the birth rate. People reject reproduction. Is this the end of the Bulgarian model? Certainly not. This is just a reaction of the immune system of the nation, which -like any living organism- tries to cast away disease. Health will come gradually with time.

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Une expérience de psychologie en 1922: l'ectoplasme de la Sorbonne

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ABSTRACT

During the spring of 1922, some experiments were progressing under the auspices of a journalist by French psychologists: H. Piéron, G. Dumas, L. Lapique and H. Laugier. They tried to observe, at the Sorbonne Laboratory of Physiology, the materializations supposed to be produced by a famous medium: Eva C. These "experiments of control", as Piéron said, failed. In this paper, we study the scientific and philosophical context of this episode and the newspapers' reactions.

Au printemps de l'année 1922, quatre savants de grand renom se livrent, dans l'amphithéâtre de la faculté des sciences de Paris, à de bien curieuses manipulations. A tour de rôle, chacun d'eux, le jeudi, déshabille une jeune femme et lui fait revêtir un costume de bain, la protège d'une lumière excessive en l'asseyant derrière un rideau; puis, tous ensemble et dans la bonne humeur semble-t-il, tout en menant une discussion à bâtons rompus à laquelle prend part la "protectrice" de la jeune femme, attendent que celle-ci veuille bien produire un ectoplasme.

En vain; mais ces messieurs dont le sérieux ni la patience ne sont pas (et ne seront pas) mis en doute attendent durant 15 semaines, prenant peut-être un certain goût à l'exercice.

Ces expériences, relatées dans la grande presse sous l'expression "Les expériences de la Sorbonne", sont réalisées à la demande pressante, insistante, d'un journal à grand tirage, *L'Opinion* (hebdomadaire qui paraît depuis 1907), à l'initiative plus précisément de l'un de ses journalistes, Paul Heuzé qui, depuis quelques années, entrepris une enquête sur l'état des sciences physiques et par la suite sur les phénomènes paranormaux, la métapsychique. Il a sans peine convaincu Henri Piéron (professeur de psychologie physiologique) d'organiser des "expériences de contrôle" qui permettent de répondre "définitivement" la question alors très à la mode: les matérialisations d'ectoplasme sont-elles ou non une supercherie? Piéron s'est adjoint deux collègues: G. Dumas (professeur de psychologie expérimentale et pathologique à la Sorbonne) et L. Lapique (professeur de physiologie générale); H. Laugier, alors chef de travaux dans

le laboratoire de physiologie générale de Lapicque, est convier à les rejoindre, puisque les charges de Lapicque ne lui permettent pas d'assister toutes les séances. Les résultats de ces expériences seront publiés dans les colonnes du journal sous la forme d'un rapport officiel et garantis la notoriété de ces savants.

Leur travail consistera à vérifier, dans les conditions d'observation les plus "scientifiques" possibles, si oui ou non, un célèbre médium, Eva C. (qui se faisait précédemment appeler Marthe Béraud, et s'était fait connaître par les matérialisations plus que douteuses qu'elle avait produites devant Ch. Richet en 1905 à Alger) fait apparaître comme on le prétend un ectoplasme; celui-ci serait constitué par une sorte de fumée qui prendrait la forme d'individus vivants ou morts, connus ou non, et sortirait de certaines parties variables de son corps selon les dires de Madame Bisson, la grande prêtresse de la non moins célèbre Eglise Ectoplasmique.

Les expériences commencent le 20 mars 1922. Elles comportent un rituel strict, respectant les règles exigeantes de l'observation scientifique mais celles aussi, non moins exigeantes, de la production d'ectoplasmes; Mme Bisson, l'entraneuse du médium, énonce ces dernières exigences: le médium, que Mme Bisson met "en état second", vraisemblablement hypnotique, a besoin de se trouver dans l'obscurité, entouré de rideaux formant un cabinet clos que le médium appelle sa "maison" (censé permettre une "concentration de forces"); les observateurs ne doivent pas trop éclairer le médium (plus il y a de lumière, affirme Mme Bisson, et plus les phénomènes de production sont réduits) ni en aucun cas saisir la substance que produit le médium qui est dans ce cas d'une sensibilité accrue et souffre de tout contact; un tel contact pourrait même entraîner la mort.

Les expérimentalistes accèdent à toutes ces demandes et leur rapport fournit les mesures détaillées et très précises de l'éclairement de la salle, de la longueur du rideau, de la taille du cabinet, etc. Ils ajoutent l'obligation de porter un maillot de bain "une pièce", de se soumettre à une fouille systématique, de se laisser tenir les mains par un ou deux "contrôleurs" pendant les séances et d'accepter même, au bout de 6 semaines, la présence d'H. Laugier sur une chaise dans le cabinet noir; cette présence commencera par importuner Eva, qui s'exclame "mais qu'est-ce qu'il fait là, ce petit?", (elle appelle indistinctement tous ces professeurs "mon petit" et les tutoie) et comme celui-ci la serre d'un peu près elle proteste "qu'il est embêtant!"

Les expériences commencent donc le 20 mars, et se dérouleront entre 16h.30 et 19 heures à différentes heures de la semaine; seul H. Piéron assistera aux 15 séances alors que Lapicque ne sera là que pour la première.

Au total, même si ce n'est pas sans une pointe de déception, tout le monde doit se rendre l'évidence...il ne se passe pas grand'chose: tout au plus peut-on noter que, lors de la 3ème séance, sur l'affirmation par Mme Bisson que "le phénomène est là", Dumas constate, en passant la tête dans le cabinet, que le médium fait sortir de sa bouche une substance grisâtre qui malheureusement s'évanouit lorsque le professeur Piéron allume sa lampe de poche! De même, la 7ème séance, les diverses palpations du cou du médium par Dumas et Laugier ne leur permettent que d'apercevoir une salive

d'écume entre les lèvres du médium. Aux 9ème et 10ème séances, le médium, qui est semble-t-il irrité, ne vient pas. A la 12ème séance enfin, le 20 mai, on croit le miracle imminent: Eva déclare que "ça vient", que "c'est là, sur son épaule gauche"; alertés, les professeurs dégraphent le maillot, découvrent la poitrine... rien de spécial. Au moment où Piéron emmène le médium se rhabiller... elle déclare que "ça revient", sa respiration se fait haletante, ses râles et ses cris (comparés par Mme Bisson à ceux d'une femme en couches) tiennent tous les présents en haleine...une substance plate et souple s'échappe quelques instants de ses lèvres, ne dépassant que de quelques millimètres... pour s'évanouir aussitôt.

Interrogée par les savants sur ce qu'elle appelle "être prise" par le phénomène, Eva déclare "c'est comme pour avoir un enfant, c'est souvent quand on en veut qu'on n'en peut avoir, et inversement"; dans cet état, le médium fait selon les contrôleurs des efforts pour vomir. Sur la nature exacte de la substance régurgitée par deux fois, les savants contraints à un éclaircissement insuffisant, déclarent ne pas pouvoir se prononcer: elle semble inerte, d'apparence résistante, et selon Dumas qui la toucha lors de la 3ème séance, visqueuse et tiède, ressemblant une feuille de caoutchouc. D'ectoplasme, en fait, il ne peut donc être question.

Le rapport des 4 professeurs, publiés en 1922 dans divers journaux comme l'*Opinion*, bien sûr, ou le *Psychic Magazine*, est donc négatif. Paul Heuzé reste cependant en contact avec le milieu des métapsychistes et se laisse convaincre de susciter de nouvelles expériences avec un médium polonais, Guzik, aux talents réputés et qui serait peut-être moins perturbé par les conditions de contrôle. Cette fois, c'est à un éminent physicien du Collège de France qu'il s'adresse: P. Langevin, professeur de physique générale et expérimentale. Celui-ci accepte de "contrôler" Guzik, en compagnie de quelques amis de H. Piéron: E. Rabaud, professeur de biologie expérimentale à la Sorbonne et membre du conseil de gestion de l'Institut de psychologie, A. Marcelin, assistant de chimie physique à la Sorbonne et I. Meyerson, directeur adjoint du laboratoire de psychologie physiologique (que dirige H. Piéron) et qui a le grand avantage de parler polonais. Ces expériences de l'automne 1923 permettent de démasquer Guzik et de ridiculiser les experts des l'Institut métapsychique qui avaient vanté ses grands pouvoirs.

Devant ces échecs répétés, la presse française s'en donne bien sûr à coeur joie, la presse de droite en particulier. Léon Daudet député de Paris, par exemple, en juillet 22 signe un article intitulé "Alas! Alas! poor Richet" dans lequel il couvre d'insultes expérimentalistes et tenants de la métapsychique et recommande à Ch. Richet la lecture du rapport sur "notre vieil ectoplasme de familles, amusement des médiums et tranquillité de leurs parents". Ou cet article non signé, intitulé "La petite femme de la Sorbonne". Dans l'Oeuvre aussi, un article signé P.S.: "Les gaietés de l'ectoplasme".

Les plaisanteries ne sont pas les seules réactions: dans la *Revue de Métapsychique* de Juillet-Août 1922 (n° 4), dont le Comité comprend entre autres Ch. Richet, C. Flammarion et G. Gelley, on avance prudemment que les 4 signataires du rapport de 1922 n'étaient peut-être pas qualifiés: "on ne s'improvise pas ectoplasmiste". Au

même moment, dans le *Psychic Magazine* de juillet 1922 (n° 158), on décrit la préparation du 3ème Congrès International de Psychologie Expérimentale, dont le président d'honneur sera Ch. Richet et le programme consacré à la métapsychique. La figure centrale de ce mouvement, cible préférée des journalistes, est incontestablement Ch. Richet; il a été le professeur de physiologie très respecté et admiré de nombre des savants qui procèdent aux contrôles. C'est d'ailleurs peut-être une des raisons pour lesquelles ils consentent à s'y livrer. Comme le révèlent les enquêtes de P. Heuzé cependant, ce grand physiologiste est d'une insondable crédulité; il est le jouet avoué de quelques supercheries savoureuses et lorsqu'on le lui avoue pour finir, il n'en veut rien croire. Il croit en fait profondément aux revenants, en un temps où ces croyances font à nouveau recette: la Grande Guerre a fait beaucoup de morts en Europe et on constate qu'un peu partout on tente des expériences pour entrer à nouveau en contact avec les disparus.

L'échec de ces expériences de contrôle ne fait que redoubler l'impression de fraude et de mensonge qui entoure le mouvement psychiste: impression encore accrue par la parution, dans un journal américain lui aussi à grand tirage, le *New York Herald* du Dimanche 4 juin, d'un article "The marvel that amazed the Sorbonne scientists", non signé. Ce long article (2 pages entières) ne contient pas une ligne de vérité. Il est publié avant que le rapport des quatre professeurs ne soit connu et suite, est-il annoncé, à une information émanant du bureau du *New-York Herald* à Paris. Il publie un "faux" rapport des savants de la Sorbonne, établissant bien entendu qu'un ectoplasme a été produit, que tous l'on touché, qu'il représentait une jeune et jolie jeune femme, bien en chair (on nous dit que la moisissure, dont elle laisse la trace en disparaissant révèle, au microscope, la trace de cellules de l'épithélium), respirant et bougeant, répondant aux diverses demandes de ces messieurs; plus, cette matérialisation a été réalisée dans une grande salle très ensoleillée de la Sorbonne devant tout un aréopage de savants attentifs. Selon ce journal, le procès-verbal affirme que cette petite femme, bien formée et à la chevelure opulente, aux yeux bleus et aux lèvres bien rouges, d'environ 8 pouces de haut, est demeurée 10 secondes dans la main d'un des savants observateurs, ce qui lui permet de confirmer non seulement la perfection de son corps mais que ce corps est pesant, sec et doux. Bref, l'ectoplasme est bien vivant. Toujours selon le *New-York Herald*, Mme Bisson, sculpteur de grande notoriété et aimée des intellectuels parisiens, a convaincu sans difficulté le "grand astronome" Camille Flammarion (connu pour ses "convictions" métapsychistes).

Une déception seulement: l'ectoplasme ainsi matérialisé n'a pas été reconnu par Mme Bisson; alors que les récents ectoplasmes de Conan Doyle et quelques autres représentaient toujours des proches disparus ou des célébrités, celui-ci est anonyme. Le journaliste s'interroge donc sur cette ectoplasmique petite femme, caressante et familière, mais inconnue.

Plus intrigant en fait, le nom des 4 savants signataires du rapport officiel est celui d'inconnus à la Sorbonne: le professeur Jeanson, "éminent inventeur", qui aurait signé le rapport avec les professeurs Jean Le Febvre, Jean de la Beauville, René Duval et Anna Barbin et Mme Bisson (seul nom exact).

La presse française, *L'Opinion*, en particulier, fait état de cet article renversant (Paul Heuzé, le 24 juin 1922), et disserte longuement sur les mensonges dans lesquels la dite presse entraîne la populace.

En fait, le texte de ce faux rapport reprend à peu de choses près celui d'un texte signé Maurice Jeanson (présenté comme un industriel assistant Mme Bisson dans ses recherches), daté du 25 mai 1921 et que Mme Bisson publiera par la suite (1923).

Ceci constitue sans aucun doute un épisode de l'histoire de la presse; mais aussi un épisode de l'histoire de la psychologie française.

On peut se demander en effet ce qui a pu entraîner ces 4 positivistes convaincus dans une aventure qui tourne vite au vaudeville et augmente les ventes des journaux à scandale. En fait, ces savants sont là pour écarter un danger qui, de leur point de vue, menace l'existence de la psychologie comme science: l'hypothèse de la production de matérialisations par la seule action de l'esprit, incompatible avec les convictions positivistes des psychologues français.

L'attitude de Piéron et celle de Laugier sont à cet égard les plus révélatrices. Conformément aux enseignements de Ribot, conformément en fait à l'aspiration à l'objectivité qui anime nombre de travaux au début du siècle, Henri Piéron s'est fait un héros de la lutte contre tous les charlatanismes qui, telles les sirènes, attirent depuis toujours la psychologie vers l'écueil de la métaphysique, son ennemie déclarée.

Depuis les premières années du siècle, Piéron a entrepris ce qu'il appelle des "expériences de contrôle" destinées à démontrer que tous les phénomènes de parapsychologie (de métapsychique), qui constituent alors l'objet de ce qui s'appelle "psychologie expérimentale", révèlent de manoeuvres frauduleuses; par exemple: il réalise en 1902 une expérience négative avec Vaschide (émigré Roumain qui travaillait chez E. Toulouse, s'intéressait entre autres aux rêves et avait selon Piéron dans son Autobiographie un "penchant pour la métapsychique de Richet") pour mettre en évidence les phénomènes de télépathie; ou l'expérience qu'il réalise en 1905 pour démasquer un jeune homme qui prétend n'avoir jamais étudié la musique et composer sous l'inspiration de musiciens disparus qui "habiteraient" son esprit à son insu; ainsi aussi en 1923 peut-on lire dans les Mémoires de la Société de Biologie le compte-rendu d'expériences menées en 1920 par Piéron avec, déjà, Dumas, Lapique, Meyerson et Rabaud (et Delacroix) montrant que les prétendus phénomènes de vision extra-rétinienne provenaient en fait d'un bandeau oculaire mal placé.

En réalité, tout en se déclarant chaque fois "ouvert à tous les faits inexplicables et désireux d'être convaincu de leur réalité", Piéron veut faire de la psychologie une forteresse imprenable, colmate toutes les brèches par lesquelles ce que sa psychologie "scientifique" prétend évacuer peut réinvestir la place: le "psychisme". Ce combat interminable va être mené en compagnie d'Henri Laugier: d'abord élève de Richet à la faculté de médecine, puis de Lapique et de Dastre (un des biologistes néo-lamarckiens protecteurs de Piéron, ami de son père) à la Sorbonne, Laugier devient assistant de Lapique jusqu'en 1923 date à laquelle il est nommé Directeur du nouveau

laboratoire de physiologie appliquée à la prophylaxie et l'hygiène mentale créée par Toulouse à Ste Anne. Dès lors, l'influence de Toulouse se fait sentir: Laugier adhèrera au programme (très vivace dans la France d'après-guerre) selon lequel la seule réponse possible aux problèmes de l'humanité est un meilleur usage de la connaissance scientifique. Toulouse et plus tard Piéron, comme Charles Richet d'ailleurs, vont soutenir un courant convaincu de la nécessité de mieux gérer le potentiel génétique humain, auquel Toulouse donnera le nom de biocratie. Et, au moment où Laugier participe aux expériences de la Sorbonne, il milite, conformément à ces convictions scientifiques et en compagnie de Ferdinand Buisson et de Léon Blum, au comité directeur de l'organisation "Les compagnons de l'Université nouvelle", née dans les tranchées en 1917. Laugier deviendra chef de cabinet du ministère de l'éducation en 1925; puis avec Albert Bayet, lui aussi membre de l'organisation, il fonde "L'Union rationaliste", dont le but exprimé est de défendre et répandre dans le grand public l'esprit et les méthodes de la science.

On comprend donc que, pour ces deux hommes, fervents positivistes, toute irruption de l'irrationnel constitue une menace et en 1922, dans l'amphithéâtre de la Sorbonne, ils ne sont pas en train de se prêter à quelque plaisanterie distrayante et fortement goûtée par le grand public. Ils sont en quelque sorte sur le front, sous la bannière de la psychologie scientifique pour démontrer son irrésistible pouvoir et la débarrasser des fantasmagories qui viennent troubler sa pureté et détournent la "populace" (le mot est de Piéron) de la vraie science et des progrès par la Raison. La suite des avatars de leur combat est une autre histoire.

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The relation between psychology and other sciences: the case of the psychology of social conflicts from 1970 to 1985

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ABSTRACT

The content of this paper is a study of the presence of Psychology in the field of research about peace and conflict, through two specialized journals, between 1970 and 1985. The most noticeable result is a progressive decline of the psychological contribution. This decline does not seem to be due so much to a loss of interest by part of psychologists about these topics, but to the difficulty of connecting a too "psychological" Social Psychology with the predominantly structuralist views recently developed by conflict and peace research.

INTRODUCTION

The permanent human interest in the study of social conflicts and the questions related to war and peace reached a scientific status and a certain degree of institutionalization during the present century, and has even led to the appearance of a new discipline: Peace Research (PR). During this process the role of Psychology, together with other sciences such as Sociology, Political Science, Anthropology, Economy, etc., has been an important one. In this paper we would like to present some data and some reflections about the contribution of Psychology to this field in the past few years.

In order to place our work within a historical context we would like to mention briefly some of the landmarks in this field¹. It could be said that up to the 20th Century reflection on conflict was in the hands of sociologists, moral philosophers and political thinkers. It is only in our time, and especially due to the disturbances caused by the IWW that other social scientists began to show interest in the analysis of war and its prevention. There was an early contribution of Psychology, with psychologists such

as L.F. Richardson, who began the statistical study of war with his *Mathematical Psychology of War* in 1919, and Theodore Lentz, who created several centres for peace research in the fifties. Another great breakthrough was the creation of the Society for the Psychological Study of Social Issues (SPSSI) in 1936, which promoted studies about peace, among other issues.

However, it was the context of "cold war" and East-West tension, with the threat of world destruction that it implied, that gave rise to the study of PR as a discipline, when many researchers realized that the world situation was entering a "cul de sac". At the same time, social sciences and particularly the study of international relations underwent a "behaviorist"² revolution that, questioning the traditional methods used by Political Sciences (supported almost exclusively in history), proposed taking the social entities (groups, governments, political elites...) as "actors" whose behaviour was to be studied, and promoted a methodological revolution based upon data collection and their quantitative analysis. In the field of PR this "behaviourism" translated into a strong interest for what Psychology would be able to contribute; for example, perception and communication processes, the escalation of distrust and suspicion that could worsen conflicts, the leaders' personality, negotiation models... Osgood's model (1962) for the reduction of reciprocal tension was probably the most important landmark.

Later on, with the lessening of tension between the superpowers and, consequently, a smaller possibility of a devastating armed conflict, other problems became more evident; especially oppression in the 3rd World countries, victims of colonialism and economic dependency. This led many peace researchers to focus their interest on exploitation and underdevelopment. Johan Galtung coined his concept of "structural violence" in the late 60's (Galtung, 1969); this concept includes any situation that undermines the human possibilities of development, even though it does not imply open conflicts: starvation, illiteracy, etc. What mattered in structural violence was the objective situation, beyond the behaviours, perceptions or motivations of the actors involved.

This displaced the stress from Psychology to other sciences such as Sociology or especially Economics; on the other hand, this change of viewpoint gave way to strong debates within the discipline.

Nowadays, the situation within PR is more eclectic, partly due to the efforts made by researchers in order to overcome confrontations; partly because in the early 80's the nuclear threat increased again, becoming a priority. In this context, it would be interesting to know what role Psychology has played in the last few years.

During the present century it has contributed a lot from different points of view: the theory of innate or acquired aggression, the application in the international sphere of the knowledge about individual processes (like the ones mentioned above), studies about people's opinions and attitudes in relation to these issues, experimental study of conflict and bargaining (mainly through experimental "games"), analysis of

intergroup relations in situations of cooperation or conflict, etc³. But together with all these, there are affirmations in the sense that either Psychology has hardly contributed significantly to the field of peace (Lagerspetz, 1987; Ingleby, 1987), or that the Social Psychology of conflict is still to a great extent something to be developed (Tajfel, 1983).

OBJECTIVE AND METHODOLOGY

It is within the context of the dilemma we were posing at the end of the introduction where we place our work, which tries to contribute some data for the consideration of this problem.

The method we chose to approach this issue was to carry out a bibliometric analysis of the articles on Psychology appeared in a sample of the publications in this field. For this purpose we selected two journals, those considered to be the most important and most representative by researchers in this field (Chatfield, 1979; Mack, 1985), and representing at the same time two different views of this topic: the *Journal of Conflict Resolution* and the *Journal of Peace Research*.

The *Journal of Conflict Resolution* (*A Quarterly for Research Related to War and Peace*) was published first at the University of Michigan in 1957 by a group of researchers interested in issues such as war prevention, disarmament and the study of conflict in general. It appeared in the context of "cold war" and took part in the "behaviourist" orientation of PR at that moment. From 1959 it was linked with the Center for Research on Conflict Resolution of the same University; when the Center disappeared, Sage Publications took over the journal in 1972 with a new team of editors from the University of Yale, who were more engaged in Political Science. In this new period we can see the evolution towards a "harder" and more quantitative line. Up to 1974, the journal kept a section especially dedicated to the studies that used game theory ("Gaming").

The *Journal of Peace Research* is considered to be the most important European journal in this field. It appeared in 1964 and its existence is linked to the International Peace Research Institute in Oslo (PRIO), created by Johan Galtung. It should not be surprising, therefore, that the journal combines the interest in open conflict and war with the "structural violence" stated by this author: underdevelopment, dependence between countries, etc.; as their editorial policy, they prefer those articles containing proposals with political and practical content. Although its contributors are to a great extent from Scandinavian countries, at least in its first years, the journal has served as a bridge between Europe and the USA.

Since these two journals began to be published in different years, the period being studied was set between 1970 and 1985, when both publications, as well as the field of research on peace and conflict, in general, could be considered as being consolidated, after the debates that were raised in the late 60's between the two tendencies mentioned in our introduction.

The starting hypothesis was that the presence of Psychology in both journals would have different treatments, due to the fact that their general approach to peace and conflict was different, as well as the geographical and cultural context in which they had appeared.

For this study we selected and analysed all the articles related to Psychology that were published by these journals over those sixteen years. Due to the deliberately interdisciplinary character, not only of both journals, but of the research as well, it was not always possible to draw a clear distinction. This is why we decided to create two categories, one of purely psychological articles, and another one in which there was a great influence of Psychology, although the papers included cannot be said to strictly belong to this discipline.

RESULTS

1. Global comparison of the presence of Psychology in both journals.

In tables 1 and 2 we can see the presence of Psychology in both journals by dividing the whole period into four-year periods and distinguishing the purely psychological articles from those merely related to Psychology, and from the addition of both. The percentages show the quantity that those articles represent over the total number of articles in each period. As we can see, there are two obvious results: on the one hand, the difference between the journals in relation to the presence of Psychology; on the other hand, the decline of this presence over the years. This latter question is for us the most interesting and to it we shall devote the next section of this work; in this section we shall go into deeper details about the differences between both journals.

According to the proposed hypothesis, the contribution of Psychology is clearly different in both publications. In the JCR, the psychological contribution ranged from 20.88% (in psychological articles) to 28.39% (if we include articles related to Psychology), whereas in the JPR this contribution was limited to either 4.90% or 9.26% respectively. In other words, if we limit ourselves to purely psychological articles, the American journal has published (considering its size) four times as many as the Norwegian one.

This is not the only difference between them. We can briefly point out other aspects⁴:

The total number of authors who published articles related to Psychology in JCR was 213, while it was 34 in JPR. We should not be surprised, therefore, that when considering the productivity of each author we also find out that it is higher in the American journal (there are three authors with five articles each, and eight with three articles) than in the Norwegian one (the greatest producer has three articles)⁵. Moreover, if we consider the general productivity in both journals, in JCR psychologists occupy more outstanding places in the classification. Another distinctive feature is the high degree of collaboration among psychologists who published in the JCR, in terms of the multiple authorship of an article, which reveals the existence of well established

research teams; on the contrary, the most prolific authors in the JPR sign all their articles alone.

In relation to the content of the articles, some differences also appear. In JCR the experimentation through laboratory games in the cooperation and competition processes, of power, of coalitions, etc., and research on conflict and its ways of resolution are the priority issues. In JPR the issues are directed towards the criticism of existing theories (for example, the use of aggressiveness to explain social conflict) and the proposal of new developments (studies centred more on socialization and conformity), the analysis of ample social phenomena and the study of perceptions about peace and war.

The difference in the quantitative presence of Psychology as well as in the issues studied can be understood, in our opinion, if we take into account the context in which the journals appeared and their theoretical orientation in the field of PR.

JCR was born at a time and in a country (USA, late 60's) where Social Psychology was highly developed. Even its foundation was marked by the presence of psychologists. On the other hand, the "behaviourist" context in which it appeared also explains a greater emphasis in the use of Psychology from its beginning. Finally, although JCR has not been insensitive to the interest aroused in the late 60's by "structural violence", its predominant line has been the study of open conflicts and their resolution.

However, in JPR there is undoubtedly an influence from the incipient state of European Social Psychology, after its dismantling provoked by nazism and IIWW⁶. Besides, its line has been much more marked by Galtung's proposals, devoting much effort to the structures of violence already present in society. Psychology, consequently, must care more for the way in which society shapes individuals than for merely individual or interindividual processes. Even then the type of analysis required by the concept of "structural violence" has more tendency towards using sociological, economical or political categories rather than psychological ones.

2. Diacronic evolution of the presence of Psychology

Now we enter in what we consider the core of our results. As shown in Tables 1 and 2, Psychology has suffered a noticeable decline in the second half of the whole period. After a culminating point in the years 1974-77, its contribution falls to a third or a quarter, depending on the journal. The most striking point of these data is their parallelism in both publications, however different, as we noted. Given this coincidence, it seems clear to us that we must think of broader causes than editorial policy in each concrete journal. These causes might be found, either inside psychology itself, or in the field of peace and conflict research, or in a conjunction of factors emanating from both spheres.

Among the psychology-related causes, the first to be proposed would be a decaying interest in these topics, i.e. that the authors who made contributions to JCR and JPR have ceased to deal with them (in the JCR, for example, the greater part of

the most prolific authors do not publish after 1978). Notwithstanding, a brief inspection of other journals show us that it is not so⁷. Psychologists, and especially most of the relevant authors in the two publications under study go on dealing with aggression, violence, international relations, conflict or bargaining; but they just publish it mostly in psychological journals now.

Simultaneously, we have found that references to JCR are sometimes frequent in the *Annual Review of Psychology* -e.g., McGrath and Krawith (1982) cite it in ten occasions-, but turn out to be very infrequent in later years (see Levine & Moreland, 1990; Messick & Mackie, 1989; Sears, 1987; there are, however, two mentions of JCR in the paper by Tesser & Shaffer (1990) on attitudes). As for JPR, there is not any mention of it in the same reviews.

It would be of interest to test the hypothesis that some recent research fields, like Political Psychology, may have absorbed part of the interest in these topics (Sears, 1987). In any case, it seems that the decline of Psychology in the JCR and the JPR has not so much to do with a lack of research as with a loss of contact with these journals.

Likewise, the cause can hardly be attributed to a lack of concern with Psychology on the part of PR. Everts' survey (1972) on peace research centres showed that psychologists' presence in them had diminished over the period 1966-1971, but at the same time there was a widespread interest in an increased participation of psychologists. What it is more, some of the research priorities of the International Peace Research Association are topics very related to behavioural sciences (e.g., Peace Education).

It seems, then, that the problem should be placed neither in one of those fields nor in the other in an isolated way, but rather in the conjunction of some factors emanating from both fields, which have interfered with its linking; we think concretely that it is due as much to the "crisis" inside Social Psychology as to the change of perspective experimented by the PR itself.

In relation to the internal situation of Social Psychology, the later years of the period studied by us coincide with the so-called "crisis" of Social Psychology, during which critical reflections proliferated. Their most notorious aspect was the "psychologist" bias of Social Psychology, i.e., the proneness to an individualistic reductionism that looked for the causes of social behaviour in the intrapsychical processess, among other characteristics: an overestimation of laboratory research, naturalistic, non-historical and non-cultural biases, etc. -see, for example, Cartwright (1979) or Pepitone (1981)-. Inside the area of our interest, Tajfel (1982, 1983) considers that most of the theories in this field are generalizations starting from individual -or interpersonal at best- processes, and that the properly psychosociological level has been not yet reached. More radically, authors like Plon (1974) or Ingleby (1987) have criticised that these biases can play an ideological role in the maintenance of *status quo*. Even in the more recent reviews from *Annual Review of Psychology* cited above there are recurring complaints about the excessive artificiality and lack of social contextualization in many studies on intergroup relations.

As can easily be seen, with that perspective, Social Psychology makes its relation with other social sciences more difficult, when dealing with complex phenomena.

While the behaviouristic approach dominated PR, this did not imply any problems, given that this approach placed psychological processes in a prominent place; but as soon as the more structuralistic views in PR became dominant, Psychology was progressively more disconnected. In other words: the change in perspective inside PR was not accompanied by a similar transformation -necessary, in any case, and not only for this purpose- of Social Psychology, in the direction of a more truly social explanation of individuals' and groups' relational behaviour; and this, to our view, accounts for the progressive isolation of the discipline with regard to conflict and peace research, as can be concluded from our results⁸.

FINAL COMMENTS

What has been described until now seems to present a rather negative perspective of the possible contribution of Psychology to the study of conflict and PR, but in these reflections we would like to note some more hopeful possibilities. Firstly, it must be considered that psychological research in this field has not vanished, although has lost its link with the field of PR. Besides, there is a certain revival of the interest for these topics, as the celebration in Helsinki, in 1984, of a Congress of European Psychologists for Peace evidences.

However, the problem of the relationship between Social Psychology and other social sciences remains unsolved. As long as Social Psychology keeps the same approaches as the problems dealt with, the relationship will remain difficult. But there are psychologists who maintain the possibility and necessity of this linkage, if it is established on other bases. As Tajfel (1983) puts it, inside complex social realities, there are several levels of analysis and explanation. We will have to assume the fact that, in many phenomena, psychological factors are not the most relevant or basic in this "explicative hierarchy", but that does not mean that they have no relevance or cannot produce effects on other reality levels. In any case, the psychological contribution, likewise that from any other discipline, must be understood inside the frame of a global account of social reality, able to deal with their different levels, the weight of each, and the way in which they interact. This would make possible not only a contribution by Psychology to the other sciences; it would improve our theories, besides, providing them with a broader, historical and social, context, which they lack today.

For this to be possible, two main elements are needed, in our opinion. First, that Psychology (in particular Social Psychology) makes an effort to overcome the theoretical and methodological reductions and biases previously noted (surely emanating from its almost obsessive concern with being "independent" and "scientific").

Secondly, a reorientation in the problems that Psychology has dealt with inside this realm would be necessary. If, in the account of conflicts and war, emphasis is moved from the actors' behaviour to the role of social, political and economical structures as the main causes of confrontations, the psychologists' interest must be not

so much centered upon behaviours, perceptions, attitudes, motivations, stereotypes,... shown when conflict is open -they would be more a consequence than a cause of the situation, although they can play a role, too- but in the ways in which socialization makes individuals enter and remain in these conflict-producer structures; or also, in the circumstances that made a certain change in these structures possible for individuals. The implication of people in war, against their own profit and conservation, has surely not so much to do with an innate or learned proneness to aggression; the role of conformity, obedience, depersonalization, etc. can be of greater importance.

These suggestions are not new in this field -see, for example, Eckhardt (1974), but till now their impact does not seem to be significant. Another controversial question is if Psychology can really contribute to the explanation of these phenomena, let alone the avoidance of war. Of course, we do not try to make predictions about this; our aim is to go on with our interest in this subject, observing its later development, with the hope that cooperation between Psychology and the rest of social sciences and disciplines will improve in the future.

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NOTAS

- ¹ For a more detailed historical analysis, see Fisas (1987) and Mack (1985)
- ² This "behaviourism" does not coincide with so called "behaviourism" as a psychological school
- ³ Given the impossibility of a more detailed presentation here, we submit the reader to the numerous works on this topic; one of the most recent and richest in perspectives being that by Falk and Kim (1980)
- ⁴ A detailed analysis of the data hereby summarized can be found in Pedraja (1989)
- ⁵ The most productive authors in JCR are John Fox, Melvin J. Guyer and Sverre Lindskold. Other prolific, and at the same time very important authors in Social Psychology in general (Miranda et al., 1984) are T.V. Bonoma, B. Schelenker and J.R. Tedeschi, just to name the most relevant ones. As for the most prolific contributors in JPR, they are William Eckardt, David, L. Cole, Samuel S. Kim and Robert E. Klitgaard.
- ⁶ This fact is even more noticeable when we consider that the authors with greater psychological contribution in the JPR are in three cases from the USA and in one case from Canada (Eckhardt).
- ⁷ Since our research is not so far finished, we do not offer concrete data here; however, through the *Social Sciences Citation Index* and the *Psychological Abstracts* we have seen that these authors keep publishing on these topics. A quick checkup of the later volumes of the *Annual Review of Psychology* has provided us with some elements on the matter, too.
- ⁸ What is more, in sociological literature about conflict there are frequent utilizations of the adjective "psychological", meaning a subjective, irrational conflict, which lacks real entity out of the subject's perception.

Table 1: *Presence of psychological and psychology-related articles in JCR, in the whole and in each four-year period.*

<i>Period</i>	<i>A: Psychol.</i>		<i>B: Related</i>		<i>A + B</i>	
	<i>n</i>	<i>%</i>	<i>n</i>	<i>%</i>	<i>n</i>	<i>%</i>
1970-1973	33	23.74	13	9.35	46	33.09
1974-1977	36	30.77	11	9.40	47	40.17
1978-1981	21	18.10	3	2.59	24	20.69
1982-1985	10	9.35	9	8.41	19	17.76
<i>Total</i>	100	20.88	36	7.52	136	29.94

Table 2: *Presence of psychological and psychology-related articles in JPR, in the whole and in each four-year period.*

<i>Period</i>	<i>A: Psychol.</i>		<i>B: Related</i>		<i>A + B</i>	
	<i>n</i>	<i>%</i>	<i>n</i>	<i>%</i>	<i>n</i>	<i>%</i>
1970-1973	6	6.74	6	6.74	12	13.48
1974-1977	7	7.87	6	6.74	13	14.61
1978-1981	3	3.49	2	2.33	5	5.81
1982-1985	2	1.94	2	1.94	4	3.88
<i>Total</i>	18	4.90	16	4.36	34	9.26

Behaviourism and moral responsibility: From Watson to Skinner

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Behaviourism tried to explain all organism behaviours with the hypothesis based on the fact that behaviours, no matter their kind, do not depend on internal variables which are inaccessible to the observer, but on external variables, thus possible to control. This hypothesis obviously included moral behaviours which were considered, at least by traditional Philosophy, as behaviours controlled, "par excellence", from inside the individual by virtue of what was usually called "moral sense".

From a basic unconcern about the behavioural field of the ethics at first and later with the development of social behaviourism, the upholders of this theory are trying to explain what is ethical starting from the behavioural hypothesis. Privately, behaviourism never needed to refer to moral aspects but things changed when it tried to deal with social problems. It was then when it had to face the subject of responsibility and moral behaviour, as understood by society, and competing with the psychoanalytical explanation.

Determinism and personal responsibility

Watsonian behaviorism interpreted all behavior, including that called **voluntary** and involving choices, in physical terms. All acts are physically determined in advance. But Watson's own interest was less in the philosophical problem of determinism *per se* than in the consequent or corollary question of personal responsibility. He argued against the assumption that individuals are personally responsible for their actions in the sense that they possess free behavior, free will. The implications of this belief are especially important in relation to criminal behavior. The behaviorist would accept punishment of criminals as a part of a system of social control, but not on the basis of a theory of retribution. Instead of making individuals pay for violations, Watson would attempt to reeducate them. He conceded that if satisfactory reconditioning could not be achieved, the person might have to be kept under restraint or be destroyed (Marx y Cronan-Hillix, 1987, 167).

Watson himself developed a visionary programme for social improvement, a so-called experimental ethic to be based on behaviorism. At the end of his book **Behaviorism**, he states: "I think behaviorism does lay a foundation for saner living. It

ought to be a science that prepares men and women for understanding the first principles of their own behavior. It ought to make men and women eager to rearrange their own lives, and especially eager to prepare themselves to bring up their children in a healthy way. I wish I had time more fully to describe this, to picture to you the kind of rich and wonderful individual we should make of every healthy child; if only we could let it shape itself properly and then provide for it a universe in which it could exercise that organization - a universe unshackled by legendary folk lore of happenings thousands of years ago; unhampered by disgraceful political history; free of foolish customs and conventions which have significance in themselves, yet which them the individual in like taut steel bands" (Watson, 1925, 248).

Albert P. Weiss' Programme of social control and its discussion by psychologists at the end of 1920's

Under the expression "social control" are to be included those form of stimulation consisting of the traditional, conventional, and legal forms of behavior which act as stimuli for others to do or not to do certain acts. Social control may be the result of teaching, both formal and informal, the result of a mandate by a recognized authority. When new forms of social control are first established as rules or laws, says Weiss, new biosocial stimuli (such as verbal arguments) act in releasing those responses of assent or dissent in those individuals who, directly or indirectly, enforce the rules. For Weiss, "ethics in this sense studies the methods through which a programme of social control is established in a social organization" (Weiss, 1928, 389).

Ethics is to reduce to observed fact that human activity is classified into "good" and "bad" and that social organizations have developed forms of stimulation directed toward increasing the duration and frequency of the good acts and decreasing the duration and frequency of bad acts. Such types of stimuli may be named "social control stimuli".

The implication of this statement is that behavior which contributes most to maintain this normal rate will be classified as good, and that which interferes will be classified as bad.

According to Weiss, conceptions such as "free will" and "personal responsibility" will vanish with the introduction of scientific methods, "although they may prove useful as pedagogical devices for those not scientifically trained. Behaviorism in ethics, says Weiss, implies that only the best scientific methods should be used in formulating this programme of social control" (Weiss, 1928, 397).

Weiss' research immediately set off a strong response from other psychologists who stressed their disagreement with Professor Weiss in the *Journal of Abnormal and Social Psychology*. W.H. Roberts' article, "Behaviourism, Ethics and Professor Weiss", may be cited: "Behaviourism and Ethics just will not mix. Professor Weiss has attempted the impossible and the only result is that he exhibits the poverty of behavioural categories, and the impossibility of founding any ethics at all on such a metaphysics" (Roberts, 1928, 393).

A third author comes into controversy; although he disagrees with Weiss' point view, he believes that it is necessary and interesting for psychology to study the moral aspects of behaviourism.

"There are two chief theoretical reasons", writes Fiscus Liber, "for the psychologist's concern with ethics: 1) the recent advances in social and genetic psychology, which have furnished us with hypotheses and information sufficient to begin the study of ethics from a psychological point of view, 2) the present state of instability and change of the current morality" (Fiscus Liber, 1929, 3). He summarises his approach in several points: 1) To admit their postulate-like qualities, 2) To submit them to criticism, eliminating all those which can be derived from more general proposals and retaining only those whose derivatives are best suited to the conditions in which a person or his/her group is living, to make them more adaptive.

Skinner's point of view about moral responsibility

The relevance of Skinner's views about moral learning may be gleaned especially from the book called *Science and Human Behaviour* (1953), but possible social implications are shown in his novel *Walden Two* (1948) and his book *Beyond Freedom and Dignity* (1971) (Graham, 1972, 91).

There are two important assumptions of Skinner's approach. 1) All human activity, including thinking being regarded as "behaviour", is "determined" by variables which are potentially discoverable. 2) The most profitable approach to the study of behaviour is in terms of relationships between events in the environment and behavioural responses of the organism to such environmental events or stimuli. The most important factors influencing behaviour, therefore, are the nature of the immediate environment and the history of past environmental events and the organism's response to them.

In this line Skinner's view of moral responsibility is essentially, at least for psychology, misguided. In *Science and Human Behaviour* (1953) he writes: "An analysis which appeals to external variables makes the assumption of an inner originating and determining agent unnecessary" (Graham, 1972, 97). The scientific advantages of such an analysis are many. The traditional conception of what is happening when an individual controls himself has never been successful as an educational device. It is of little help telling a man to use his "will-power" or his "self-control", he writes. An alternative analysis of the behaviour of control should make it possible to teach relevant techniques as easily as any other technical repertoire. In the *Beyond Freedom and Dignity* (1971) Skinner says: "We can follow the path taken by physics and biology by turning directly to the relation between behavior and the environment and neglecting...states of mind, feeling... intentions" (Skinner, 1971, 15).

Skinner is quite explicit in his definition of responsibility in terms of association of rewards and punishments with different forms of behaviour. In this way, Skinner solves the problem, which has been a major one in law, of determining whether a person is or is not "responsible" by virtually denying the usefulness of such a concept of responsibility at all.

When we regard a person as “insane” what we mean is that we cannot control his actions in the most usual ways, and “diminished responsibility”, which is not insanity, simply presents a lesser degree of uncontrollability. The conclusion is clear. People, though “reasonable” are manipulable organisms rather than “responsible” beings. For Skinner it is necessary to endow the individual with a knowledge of consequences or some sort of “expectation” to bridge the gap between past and future. But we are always dealing with a prior history of reinforcement and punishment... The reasons for an action are simply some of the variables of which behaviour is a function (Skinner, 1965, 342-343).

Man is conceived, therefore, not as a being with the power of “realizing” purposes or “endeavouring” to achieve ends or “trying to live up to ideals” or practising the principles of justice or equity, but as a responding organism controlled by his past history and the nature of the situation in which he finds himself. To be moral is to conform to certain kinds of expectations of reinforcement which may very well be symbolically mediated by the individual himself, but which have their ultimate origins in reinforcements of the others.

Such an interpretation of the behavior of the cultural designer brings us to an issue of classical proportions. Eventually, a science of human behaviour may be able to tell the designer what kind of culture must be set up in order to produce a given result, but could it ever tell him what kind of result he **should** produce?

Skinner writes: The word “should” brings us into the familiar realm of the value judgment. It is commonly argued that there are two kinds of knowledge, one of fact and other of value, and that science is necessarily confined to the first. But it is not true that statements containing “should” or “ought” have no place, says Skinner, in scientific discourse. There is at least one use for which an acceptable translation can be made. A sentence beginning “You ought” is often a prediction of reinforcement consequences. For Skinner, the sentence, for instance, “You ought to love your neighbour”, may be converted into the two following statements: first, “the approval of your fellow men is positively reinforcing you”; and, second, “loving your fellow men is approved by the group of which you are a member” (Skinner, 1965, 429); and both proposals may be demonstrated scientifically.

CONCLUSION

From our point of view Skinner’s position has remained unaltered for years without having made the least allowance to Cognitivism (Cognition Theory) not even within the limits of Moral Psychology. As it is known, this has not happened either among other representatives of the theories of learning such as Aronfreed, Bandura, Hoffman, Berkowitz, etc. In relation to this, we may consider Skinner as the most modern representative of the first radical behaviourism in this aspiration to give a strictly scientific explanation of moral behaviour.

Maybe this is why the defender and protagonist of the psychology of moral development in recent years, Lawrence Kohlberg, refers mainly to Skinner in order to

bring face to face and compare the cognitive-developmental model and the behaviourism of moral learning. And this does not happen because Skinner has studied the subject specially but because, due to his radicalism, he blocked from the very beginning all possible attempts to underpin and shape the zone of the psychology of moral development. The contrast between the Skinnerian approach and the cognitive-developmental approach is pinpointed by Kohlberg as follows: facing a moral behaviour lacking attitudes, feelings and values judgments, the cognitive-developmental view attempts to integrate both behavior and internal states in a functional epistemology of mind. It takes inner experience seriously by attempting to observe thought rather than language behavior and by observing valuing processes rather reinforced behavior" (Kohlberg, 1981). The cognitive-developmental approach derives from a pragmatic methodology that attempts to integrate the dichotomies from the inner versus the outer, the immediate versus the remote in time. The cognitive-developmental approach focuses on an empirical search for continuities between inner states and outer behavior and the immediate reaction and the remote outcome.

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Une revue "fin de siècle": La Revue de Psychologie Clinique et Thérapeutique. 1887-1901

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ABSTRACT

There is a general agreement to consider that the beginning of clinical psychology in France traces back to 1949.

However, no one mentions that, between 1897 and 1901, the Revue de Psychologie Clinique et Thérapeutique was published under the direction of two physicians, Hartenberg and Valentin.

In order to understand the reasons why this journal was published for such a short period, I shall try to place it within the medical, psychological and philosophical context of the time, through an analysis of its aims and contents.

Les quelques auteurs français qui évoquent l'histoire de la psychologie clinique, après avoir rendu un rapide hommage au précurseur que Pierre Janet représente à leurs yeux, indiquent généralement que ce secteur de la psychologie ne se constitue, en France, qu'en 1949, date d'une conférence donnée par Daniel Lagache devant le Groupe de l'Evolution psychiatrique (1).

S'agit-il là d'une erreur ou d'une "occultation inquiétante", comme le suggère Prévost, qui, à ma connaissance, est le premier à signaler avec insistance que, du mois de décembre 1887 au mois de décembre 1901, une Revue de Psychologie Clinique et Thérapeutique, dirigée par Hartenberg et Valentin, puis, durant les derniers mois de son existence, par Hartenberg et Aimé, parut mensuellement(2)? Nous verrons par la suite que Paul Hartenberg fut, en son temps, loin d'être un inconnu, dans le milieu restreint des aliénistes à tout le moins, et que la Revue... fit appel à maints contributeurs connus, sinon célèbres.

Ceci amène quelques questions: pourquoi cette revue fut-elle créée, pourquoi son existence fut-elle si brève, et, enfin, à quoi est dû l'oubli à peu près total qui la frappa? Répondre à ces questions implique que l'on s'efforce de la situer, tant par ses objectifs et son public que par son contenu, dans le contexte médical, psychologique et philosophique du moment, ou, en d'autres termes, dans la configuration des personnes et des forces en présence.

Dans sa première livraison, en décembre 1897, le Bulletin, signé des deux directeurs, est intitulé "Notre Programme". Il mérite qu'on s'y attarde quelque peu: d'emblée, la "Revue..." s'adresse "au public médical" et ses auteurs désirent "contribuer (...) à légitimer et à étendre les récentes conquêtes de la Psychologie dans le domaine de la Clinique et de la Thérapeutique". Il y est dit ensuite que, grâce aux progrès de l'histologie et de la physiologie, ainsi qu'aux travaux des aliénistes sur les psychonévroses, "la psychologie est devenue une des branches les plus parfaites de la biologie." Suit un hommage appuyé aux travaux des précurseurs et des représentants les plus connus de l'Ecole de Nancy, ce qui n'a rien de surprenant lorsque l'on sait que Paul Hartenberg fut élève de Bernheim et que sa thèse de médecine, soutenue à Nancy en 1895, s'intitule *L'élément psychique dans les maladies*. D'ailleurs, dans le programme, quelques lignes plus bas, les auteurs insistent sur la nécessité d'étudier le rôle de "l'élément psychique" dans la genèse des troubles, puis proposent une définition de la psychologie clinique (opposée à la psychologie expérimentale, de laboratoire) qui ne devrait pas être reniée par les spécialistes contemporains: "La psychologie clinique, tout en puisant dans les recherches de laboratoire de précieux renseignements, observe la vie psychologique elle-même, considérée comme un tout concret et réel. (...) Sous les influences combinées de l'hérédité et du milieu, elle poursuit le développement, normal ou pathologique, de la personnalité. Sa tâche n'est pas de schématiser, mais d'individualiser".(3) L'approche clinique a pour but de pratiquer avec discernement la psychothérapie, c'est-à-dire, ici, le traitement par suggestion, puisqu'à l'époque les termes étaient absolument synonymes. La psychothérapie, enfin, a pour ambition d'être utile non seulement à l'individu, mais encore à la collectivité, en permettant la prophylaxie des névroses, grâce à "la pédagogie suggestive et l'orthopédie mentale". Ce sont là des expressions d'usage courant à l'époque, même si elles surprennent le lecteur de 1991. On les rencontre dans de nombreux travaux utilisant la suggestion, et elles traduisent clairement la prétention des médecins à réformer et rééduquer tant l'individu que la société, laquelle doit leur reconnaître le pouvoir que leur confère l'accession de la psychologie, conçue comme une branche de la médecine, au stade scientifique. Enfin le programme s'achève sur un appel à la coopération internationale, dont la "Revue..." se veut un instrument.

Cette présentation un peu longue devrait nous permettre de mieux comprendre l'analyse, qui va suivre, du contenu de la "Revue...". Des extraits de ce programme furent, jusqu'en 1900, reproduits sur la dernière page de chaque numéro.

Chaque numéro comporte quatre rubriques: Tout d'abord, un Bulletin, signé par l'un des directeurs, à l'exception de trois d'entre eux en 1897-1898, qui sont dûs à des auteurs étrangers et présentent respectivement la psychothérapie aux Etats-Unis d'Amérique, en Russie et en Espagne et d'un autre en 1901, signé par E. Régis, et traitant du parricide. Le Bulletin est suivi de Travaux originaux et Faits cliniques, puis d'une Revue critique d'articles, d'ouvrages ou de revues récents, et, enfin, de Variétés et nouvelles, comme on en trouve à l'époque dans nombre de revues, en particulier médicales.

Le Bulletin correspond à ce qu'en termes contemporains on appelle un Editorial. Le ton est souvent moralisateur ou polémique, les thèmes abordés, bien que variés, présentent certaines constantes: il s'agit parfois de plaidoyers pro domo, présentant les méthodes thérapeutiques des auteurs (cf., par exemple, le bulletin de février 1901, intitulé *La psychothérapie nouvelle*, c'est-à-dire "l'entraînement suggestif"), ou encore leurs travaux de recherche (cf. en février 1899, un Bulletin sur la timidité, signé Hartenberg, par ailleurs auteur d'un ouvrage intitulé *Les timides et la timidité*).

Quelques Bulletins présentent le point de vue des auteurs sur les congrès récemment tenus. L'exemple le plus intéressant date d'Aout 1900: Hartenberg y déplore que, lors du XIII^e Congrès International de Médecine, la psychologie clinique, c'est-à-dire "l'étude des troubles psychiques des névroses", et la psychologie thérapeutique aient été presque totalement négligées. Il trouve également des plus fâcheuses la division de la Neurologie et de la Psychiatrie en deux branches distinctes et prédit que l'étude des névroses, "dédaignées des neurologistes et sacrifiées par les psychiatres", en pâtira. Et, conclut-il, "comme notre psychologie étudie surtout les formes psychiques des névroses et s'efforce de les guérir ainsi s'explique son peu de succès et sa position incommode entre deux branches qui s'écartent de plus en plus". Ces regrets devront être pris en compte quand il s'agira de comprendre les raisons qui ont amené les auteurs à mettre fin à leur revue.

Toutefois, dans la plupart des Bulletins, les auteurs utilisent des faits d'actualité (spectacles, ouvrages récemment parus, évènements politiques ou judiciaires, etc.), pour présenter leur opinion sur des sujets qui leur sont chers, en particulier ceux de la dégénérescence, de la politique pénale ou de l'éducation: Valentin, en 1898 et 1899, consacre trois bulletins au "tueur de bergers" c'est-à-dire Vacher, "le plus grand criminel de tous les temps", exécuté en 1898, malgré sa folie manifeste et les protestations de nombreux aliénistes, en particulier celles d'Edouard Toulouse. A travers ces bulletins (parmi d'autres), il apparaît clairement que les auteurs sont des partisans convaincus de l'école pénale positive italienne et en particulier de Lombroso. Il sont, corrélativement, convaincus de l'urgence de la lutte contre la dégénérescence, thème qui revient avec une insistance quasiment obsédante dans leurs écrits. Leurs positions sont fermement eugénistes, et ils affichent, pour étayer leurs thèses, un pessimisme très "fin de siècle": la dégénérescence vaincra, et "les gens simplement normaux sont appelés à disparaître" (février 1900). Il déplorent également "le nervosisme contemporain" et pensent qu'"il faut relever l'énergie nerveuse des populations urbaines" (mai 1898). Dans le domaine de l'éducation, ils réclament une "pédagogie scientifique" et une "psychologie infantile" afin d'éliminer les cancrenards qui encombre les classes. Signalons enfin le bulletin de Janvier 1900, intitulé *Corrélations psycho-sexuelles*, dans lequel Hartenberg se défend contre le reproche que, semble-t-il, on lui avait adressé, d'accorder trop de place, dans la Revue..., aux sexuelles, citant de nombreux auteurs dont Freud, pour affirmer l'importance de la sexualité dans la "médecine nerveuse".

Les Travaux originaux et Faits Cliniques, sont en général de courts articles, si bien qu'une contribution un peu longue est scindée en plusieurs parties. On constate une différence entre les signataires des articles de l'année 1897-1898 et ceux des années suivantes: on trouve, la première année, une majorité d'auteurs français (ou francophones) connus pour leurs travaux sur l'hypnotisme, qu'ils appartiennent ou non à l'Ecole de Nancy: Liébeault et Bernheim, mais aussi Voisin, Dumontpallier, Crocq, et Durand (de Gros). Par la suite, on voit le nombre des contributeurs francophones (et spécialistes de la suggestion ou de l'hypnotisme) diminuer notablement en 1899 pour disparaître complètement en 1900 et 1901 (si l'on excepte, évidemment, les articles des directeurs de la Revue...). Le contenu des articles change également: alors qu'en 1897-1898, un tiers environ des articles traitait de la suggestion et de l'hypnotisme, ils disparaissent à peu près totalement par la suite au profit, pour l'essentiel, de Travaux Originaux en psychophysiologie ou en psychiatrie médico-légale. On y trouve des noms célèbres, en particulier ceux de Lombroso et surtout de Wernicke, dont la publication des *Principes de psychophysiologie* s'étend sur 6 numéros en 1899 et 1900, ainsi qu'une série d'articles du portugais Bombarda sur la doctrine de Fleschig. Notons au passage que Wernicke et Fleschig sont deux représentants éminents de la psychiatrie organiciste allemande. Il ne m'est pas possible de dire si cette politique de publication vient d'un manque de contributeurs francophones disposés à collaborer à la revue, ou encore susceptibles d'exprimer des points de vue compatibles avec ceux de ses directeurs, ou si elle est délibérée, au nom de la coopération internationale annoncée dans leur programme.

En ce qui concerne les Faits Cliniques, ils sont essentiellement dûs aux deux directeurs de la Revue..., et présentent des traitements et guérisons obtenus grâce à la psychothérapie. A l'examen, on s'aperçoit que leurs méthodes sont très semblables à celles qui, beaucoup plus tard, seront utilisées dans les thérapies comportementales. Ainsi, pour traiter une phobie, Hartenberg utilise une technique analogue à celle qu'on appelle, de nos jours, thérapie par immersion. On constate également que la notion même de psychothérapie évolue explicitement durant la courte vie de la "Revue...": Alors qu'en 1897, devant la Société d'hypnologie, Valentin indique qu'elle consiste "à actionner la suggestibilité" des malades par différentes méthodes, on voit peu à peu, dès 1898, s'élaborer une séparation entre des procédés psychothérapeutiques employant la "suggestion verbale pure", que d'ailleurs Hartenberg propose d'appeler "logothérapie", précisément pour la distinguer d'autres procédés, supposés, comme le dit Valentin lors du IV^e Congrès International de Psychologie, en 1900, après s'être dégagés "de tout compromis avec le vieil hypnotisme empirique et traditionnel", utiliser "les plus récentes découvertes de la psychologie positive, science naturelle des fonctions du cerveau": la parole perd progressivement de son importance au profit de méthodes "rééducatives" motrices, fondées sur une conception que nous pourrions qualifier de réflexologique de la thérapie. L'objectif, certes, reste le même, il s'agit toujours d'obtenir la "guérison" en supprimant le symptôme, soit par la parole, soit par la rééducation, mais on voit bien que le choix de la méthode implique une conception différente des rapports du psychique au somatique et que nos auteurs évoluent vers une

approche périphérique du traitement "psychique", position, finalement, plus compatible avec leur organicisme fondamental que celle qu'ils affichent dans leur "Programme": comme le dit Hartenberg en septembre 1900, la psychologie "sera donc avant tout une Psychologie anatomique" (p.259).

L'examen du choix des ouvrages présentés et souvent abondamment commentés dans la Revue Critique et celui du contenu de la rubrique Variétés et nouvelles n'apporte guère de nouveauté par rapport à l'analyse qui précède: cosmopolitisme, organicisme, intérêt pour la criminologie, la psychiatrie médico-légale et les problèmes éducatifs, lutte contre la dégénérescence. La place accordée, dans les Variétés..., aux études de psychiatrie ou de psychologie appliquées à la création artistique, littéraire, ou, plus généralement, au génie, va tout à fait dans ce sens: on y trouve par exemple des articles sur la neurasthénie d'Ovide, la maladie de Blaise Pascal, la psychologie de Schopenhauer, la folie et le génie de Christophe Colomb (sous la plume de Lombroso), etc...

En résumé, nos auteurs semblent avoir bien assimilé le modèle médical français de la fin du siècle, tel que l'ont décrit, en particulier, Nye (1982) ou Harris (1989)(4): poureux, l'orthopédie et l'hygiène mentales ainsi que la lutte contre la dégénérescence, pratiquées par les médecins, sont les piliers de la réforme sociale et de la "régénération" nationale.

En décembre 1901, le Bulletin, anonyme pour la première fois, annonce, en substance, que la Revue..., ayant atteint son but (?), cesse de paraître, malgré son succès...et que ses directeurs vont "s'employer à des travaux spéciaux plus homogènes". Il semble bien qu'il s'agisse là d'un discours officiel, et que les raisons précises qui motivent cette décision restent inavouées. Toutefois on peut faire quelques hypothèses.

Avant de poursuivre, il me faut dire quelques mots de Paul Hartenberg (1871-1949), qui semble bien avoir été le pivot de cette entreprise. Il publia tout au long de sa carrière, un grand nombre d'articles et d'ouvrages consacrés à des questions de psychiatrie, mais également, plusieurs romans, sous son nom, sans prendre de pseudonyme, contrairement à l'usage du temps. En outre, il reproduit, dans ses ouvrages médicaux, au titre de descriptions cliniques, de larges extraits de ses romans (5). Ce qui doit, en particulier, retenir notre attention, est qu'il est probablement le premier introducteur en France des travaux de Freud. En effet, il publia en 1901, dans la Revue de Médecine, une série d'articles sur La névrose d'angoisse, repris en 1902 sous forme d'un opuscule de 83 pages (6), dans lequel il expose de façon très détaillée la conception freudienne, et la reprend à son compte, tout en émettant, naturellement, quelques réserves sur la généralité de l'étiologie sexuelle. Un certain nombre d'indices montre qu'il suivait de près, à l'époque, les travaux de Freud: on trouve, par exemple, en septembre 1898, une longue revue critique de La sexualité dans l'étiologie des névroses, par Sigismond (sic) Freud. Peu de temps après, semble-t-il, il devint un anti-freudien notoire, au point que dans son ouvrage Psychologie des Neurasthéniques, en 1908, il ne cite même pas Freud lorsqu'il parle de la névrose d'angoisse.

Comment peut-on expliquer l'échec, puis l'oubli, de cette revue et de la tentative, qui était sienne, de fonder une psychologie clinique?

Il me semble que, tout au long de leur entreprise, Hartenberg et Valentin sont, comme ils le disent eux-même d'ailleurs, "entre deux branches".

Ainsi que nous l'avons vu ci-dessus, le traitement "psychique" qu'ils prétendent pratiquer est sous-tendu par une conception radicalement organiciste de la psychologie, qui apparaît aussi bien dans leurs écrits que dans leur politique éditoriale. En conséquence, leurs méthodes thérapeutiques évoluent, pour l'essentiel, d'une pratique de la suggestion telle qu'elle se présente à la fin du XIXe siècle, et dont la fécondité vient, d'ailleurs à son insu, de l'accent qui est mis sur le rôle de la parole, vers des techniques de rééducation motrice. En d'autres termes, ils ne peuvent ou ne veulent pas dégager leur "psychologie clinique" du modèle médical de leur temps, comme le font, au même moment, Janet, qui d'ailleurs, n'est jamais cité dans la Revue..., et surtout Freud, qui dès 1896 publie un article en français dans lequel il met en question l'étiologie héréditaire de l'hystérie.

Du point de vue scientifique et institutionnel d'autre part: ils se présentent, même si c'est à tort, comme les défenseurs d'une approche psychologique, fondée sur leur expérience de la "médecine de ville" alors que la médecine française, après la mort de Charcot en 1893, retourne, avec Babinski en particulier, à une conception neurologique des "vrais" troubles mentaux. Ils sont partisans de Lombroso dont l'école française de criminologie, Tarde en tête, conteste les thèses depuis 1885.

Enfin et surtout, ils défendent leur psychologie clinique contre la psychologie "expérimentale", alors qu'au même moment cette dernière occupait le terrain institutionnel: Ribot enseignait la psychologie expérimentale et comparée au Collège de France (Janet lui succéda en 1902) et dirigeait la Revue Philosophique, cependant que Binet était directeur du laboratoire de psychologie physiologique de l'Ecole des Hautes Etudes et de la revue L'Année Psychologique. Enfin, chez les médecins, le célèbre psychiatre Edouard Toulouse, Médecin en Chef de l'Asile de Villejuif, avait obtenu en 1900 le rattachement à l'Ecole des Hautes Etudes du laboratoire de psychologie expérimentale qu'il avait créé dans son service et dans lequel il employait des psychologues, en particulier Piéron. Il publie en 1900, dans la Revue de Psychiatrie, un Bulletin intitulé L'évolution de la psychologie (6) qui ne laisse aucun doute sur son expérimentalisme en la matière.

A tout ceci, il faut ajouter que la psychologie française était essentiellement représentée par des philosophes ou des philosophes-médecins, ce que n'étaient pas Hartenberg et Valentin.

En conclusion, il me semble que ces derniers appartiennent vraiment au siècle finissant, alors que l'institutionnalisation de la psychologie française est faite par des expérimentalistes. Et que leur "psychologie clinique" n'a que peu de points communs avec celle qui, beaucoup plus tard, verra le jour, grâce à la psychanalyse, dont, d'ailleurs, Hartenberg ne voulut rien savoir.

NOTES

- (1) Reuchlin M. (1957) - *Histoire de la psychologie*. Paris. Presses Universitaires de France ("Que sais-je?"), 1980. 76-81. Favez-Boutonnier J. (1959) - *La psychologie clinique. Objet-Méthode-Problèmes*. Paris. Centre de Documentation Universitaire. 1959. 4-5. Plaza M. (1989) - *La psychologie clinique. Les enjeux d'une discipline*, in Revault d'Allonnes et al. - *La démarche clinique en sciences humaines*. Paris. Dunod. 3-4.
- (2) Prévost C.M. (1969) - A propos des origines de la psychologie clinique. *Bulletin de psychologie*. 1969-1970. XXIII. 119-124. (1973) - *Janet, Freud et la psychologie clinique*. Paris. Payot. (1988) - *La psychologie clinique*. Paris. Presses Universitaires de France ("Que sais-je?"). J'ai toutefois relevé un certain nombre d'inexactitudes dans ces travaux. Selon leur auteur, la collection des numéros de la "Revue..." qui se trouve à la Bibliothèque Nationale est incomplète. En revanche, celle qui se trouve à la Bibliothèque de l'Ecole de Médecine est presque complète, à l'exception du numéro d'Avril 1901. Par ailleurs, je suis, dans l'ensemble, en désaccord avec les interprétations de l'auteur.
- (3) "La psychologie clinique a pour objet l'étude de la conduite humaine et de ses conditions (hérédité, maturation, condition physiologiques et pathologiques, histoire de la vie), en un mot l'étude de la personne totale "en situation" (D.Lagache).
- (4) Nye R. (1982) - Degeneration and the Medical Model of Cultural Crisis in the French Belle Epoque, in Drescher S., Sabeau D. and Sharlin A. (Eds). *Political Symbolism in Modern Europe*. New Brunswick, NJ. 19-41. Harris R. (1989) - *Murder and Madness. Medicine, Law and Society in the "fin de siècle"*. Oxford, Clarendon Press.
- (5) Sur ce point, voir Carroy J. (1990) - Dédoubléments. L'énigmatique récit d'un docteur inconnu. *Nouvelle Revue de Psychanalyse*. XLII, Automne 1990, 151-171. Et, du même auteur, dans ce numéro, "Le théâtre de la peur d'Alfred Binet".
- (6) Hartenberg P. (1902) - *La névrose d'angoisse*. Paris, Alcan.
- (7) Toulouse Ed. (1900) - L'Evolution de la psychologie. *Revue de Psychiatrie*. 1900, III. 257-261.

Eugenics as Social Intervention to Prevent Mental Disorders: The Psychiatrist and Psychologist Wilhelm Weygandt (1870-1939)

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ABSTRACT

Wilhelm Weygandt, student of Wundt's and Kraepelin's, was involved in developing a "Clinical Psychology" that was methodologically oriented towards the principles of empirical and experimental psychology. In this respect he can be regarded as a forerunner of current clinical psychology. In this paper the importance Weygandt attached to eugenic programmes for preventing mental diseases is examined. After explaining the concept of 'eugenics' in its historical and social context, a short summary of Weygandt's biography is presented. His scientific publications referring to eugenics and "racial hygiene" are analyzed with regard to evaluation as well as justification of eugenic programmes. Particularly, differences in content and rhetoric under different socio-cultural and political conditions are described. Finally the results are related to the current debate on eugenic programmes for mentally disabled persons.

INTRODUCTION

Wilhelm Weygandt, a student of Wilhelm Wundt, was involved in developing a "Psychiatric Psychology", that was orientated towards the experimental as well as the descriptive-ethnopsychological methods of his teacher in Leipzig. In this respect, Weygandt can in a way be seen as a forerunner of clinical psychology and therefore he is also a subject for the History of Psychology.

In this paper I am concentrating on examining the importance Weygandt attached to eugenic programmes in order to prevent mental diseases. At first I would like to discuss the concept of "eugenics"; then a short summary of Weygandt's biography is presented (cf. Nebel-donat, 1990). His attitudes towards eugenics and "racial hygiene" are described. Particularly, differences in content and rhetoric reflecting the respective sociocultural-political conditions are analyzed. For Weygandt lived through three quite different historical periods: the Second German Empire, the Weimar Period and the Nazi-dictatorship.

THE ROLE OF EUGENIC INTERVENTIONS PREVENTING MENTAL DISEASES

1. On the concept of Eugenics

The concept of "eugenics" was introduced by the English anthropologist Francis Galton (1822-1911), a cousin of Charles Darwin, in 1883.

Galton's ideas are reflected by the following quotation:

"What an extraordinary effect might be produced on our race", if it were common practice to "unite in marriage those who possess the finest and most suitable natures, mentally, morally and physically", and on the other hand, "to anticipate the slow and stubborn processes of natural selection, by endeavouring to breed out feeble constitutions and petty and ignoble instincts..." (Galton, 1865, 1873; quoted in Freeman, 1983, p. 10).

Accordingly we distinguish between *positive* and *negative* eugenics; the former supports the tradition of desirable traits, the latter suppresses the tradition of undesirable qualities (cfl. Weingart et al, 1988, p. 16). In this I would like to point out that we can basically distinguish between scientific, religious-ideological and political aspects of eugenics (cf. Buss, 1975). Eugenics became an international movement. There are still traditional strands influencing the modern sciences of human genetics and differential psychology (cf. Probst, 1990, Weingart, 1988).

2. Biography of Wilhelm Weygandt

Wilhelm Weygandt, born in Wiesbaden in 1870, first studied philosophy. The title of his dissertation, tutored by Wundt, was "Origin of Dreams" (1893). Then he studied medicine, putting the main emphasis on psychiatry. He studied also with Emil Kraepelin, another student of Wundt, in Heidelberg. From 1908 until 1934 Weygandt was director of the Hamburg Psychiatric Clinic (since 1919 University Professor, cf. Meggendorfer, 1940). From 1919-1928 Weygandt was a member of the liberally-minded "German Democratic Party" - like his colleague William Stern at the Psychological Institute - and also of the free masons lodge "Schlaraffia" until 1933. For this reason the Nazis disqualified him as an unreliable person and so he had to give up his job a year later. He was "...a child of his liberal period, who was burdened severely by his former life" (Meggendorfer in his obituary, 1940). Weygandt defended himself against this judgement and struggled for being recognized by the new political class. He stressed, for example, that only very few members of the university had concentrated as intensively as he had for as many years, on subjects "in accordance with national-socialistic ideas, particularly the doctrines of racial hygiene" (letter of April 8, 1934, to the president of Hamburg University, - State Archives of Hamburg):

3. An analysis of Weygandt's scientific publications with regard to the role of eugenics and "racial hygiene"

In Germany "eugenics" was frequently replaced by the concept of "racial hygiene". Its Germanic-racial connotation was slowly growing, particularly in the

thirties. Looking at Weygandt's publications written under different cultural and political conditions we can find continuity as well as discontinuity in his attitudes towards eugenic and racial hygiene programmes.

3.1. CONTINUITY

Weygandt put down his basic ideas on prevention of mental diseases already in 1902 and they continually turned up in the following years: Hereditary transmission is the most important reason for psychoses and "therefore the most radical measure would be to prevent the reproduction of the mentally abnormal as well as the congenitally afflicted" (1902a). In his opinion the state should see to "preventing mental diseases" by legal interventions like prohibition of marriage, sterilization, castration. Furthermore psychosocial and educational measures are mentioned, as for example fighting addiction, hygiene of pregnancy and birth, education (prevention of overburdening children in schools), balancing social contrasts, or raising common prosperity. Weygandt, consequently, did recognize the psychosocial causes of mental diseases.

Also in his early publications (1904), Weygandt presents detailed proposals on prohibiting marriage for "congenitally afflicted persons". In the following years he was to take up this topic again and again: Weygandt divided the population into three classes: A ("*mentally healthy*"); B ("*mentally at moderate risk*": neuroses, migraine, alcoholics, who are not mentally ill, etc.); C ("*mentally ill and high risk persons*: psychoses, mental deficiency, sexual perversions, mentally ill alcoholics, mentally disturbed epileptics etc.). This tripartition resulted in the following marriage rules, in short according to Weygandt: A + A ("is allowed"), A + B ("is allowed with reservation"), all the other combinations (A + C; B + B; B + C; C + C) are forbidden.

3.2. CHANGE AND DISCONTINUITY

a) Changing his liberal-humanitarian attitudes

In 1900 we can find a couple of comments characterized by a liberal-humane spirit. He spoke of the mentally ill as the "unhappiest among our fellow creatures. We have to care for them in a social and human way". Education should play a basic role in this context. He also commented approvingly on ideas of the "International Criminological Association". He showed sympathy with the conviction that the goal of any treatment should not be atonement but integration as far as possible into human society.

In 1928 however, when public opinion towards criminals and the mentally ill changed in Germany because of increasing social tensions (Weingart, 1988), Weygandt commented on the penal system of the time in an ironical and aggressive way. In 1933 Weygandt spoke of "the oppressive rule of equality", of "antisocial hedonism", of the English reformatory prison Camp Hill as an example for "exaggerating humanitarian measures". Three years later, Weygandt warned against the threatening rise of

“wretched ballast existences” and he added that this evil could only be “wiped out” by aggravating the public health laws (1936).

b) Articulation of national, antidemocratic ideas

In 1928 we can find outspoken national rhetoric for the first time in Weygandt's writing. In “Safeguarding, Curing and Preventing as tasks of the modern Psychiatry” (1928) he writes:

“Germany can only exist with a standard of high quality in work; for that, however, people bred towards maximum achievement are required, whose minds should be as free as possible from inferior tendencies of diathesis...” (p. 1535).

In 1933 Weygandt took this topic up again putting it more aggressively: “It is high time that action is being taken against those who out of exaggerated individualism are making Germany a paradise for inferior people” (1933, p. 79).

c) Racial Rhetoric

In the beginning of the Nazi-dictatorship, we encounter some racial diction for the first time. Weygandt denounces “that effeminate and non-germanic attitude” that led to the abolishment of the death penalty in the “Weimar System” (1933).

d) Increasing proposals for sterilization and castration

While speaking cautiously about sterilization and castration in his earlier publications, Weygandt was calling for both offensively in about 1928. He then accused the opponents of sterilization for “hereditary threatening individuals” of being weak sentimentalists, of being excessively thoughtful towards the well being of the individuum, and of supporting a “superficial hedonism”. He demanded instead “rational, race-improving” measures to “prevent the reproduction of the mentally abnormal” (1928, p. 1535). Since 1933 Weygandt did not stand only for a compulsory sterilization law containing an far-reaching indication catalogue (for transmitters of hereditary mental diseases, children of schizophrenic parents, “certain prisoners”, transmitters of hereditary abnormalities...), but he also claimed that degenerates should be castrated (subsequent sexual offenders, violent habitual criminals, those committing a crime under the influence of emotion, antisocial alcoholics, violent psychiatric patients) (1933).

Then he also started to give demographic details and to make prognoses. He spoke, for example, of 80.000 schizophrenics and 200.000 mentally deficient (1934). One year later he stated a figure of 400.000 persons to be sterilized, “this number will probably still increase considerably for mentally retarded and for schizophrenics” (p. 151). In two generations most of the “hereditary lines” of endogeneous mentally retarded people will be “wiped out” (1936).

e) The extinguishing of “worthless life”

In 1928 Weygandt had protested against the abolition of the death penalty, together with the Psychiatrist Hoche. He wanted it to be kept for eugenic reasons: so

that the degenerate diathesis of the criminal would be destroyed. Weygandt used the term "extinguishing of worthless life" for the first time in 1935. In this context, he mentioned there was a proposal to kill cases of severest degrees of idiocy; "but so far it has not been approved of. Weygandt did not plead for killing mentally deficient persons, but he also did not contradict to such proposals. From comments one year later (1936), however, I conclude that he did not oppose the killing of disabled people, in principle:

"...The *extinguishing of worthless life*, that is the elimination of such beings who without doubt are expected to remain idiots permanently, has been recommended by *Binding* and *Hoche* from the beginning, and more than once, but this has not been approved of yet... maybe a very extreme war or a famine could lead to this measure" (1936, p. 166).

CONCLUDING REMARKS

a) Obviously Weygandt's attitude towards eugenic programmes is not simply an adjustment to the Nazi-dictatorship, on the contrary, in some respect, he did use nazism as an instrument to put his own ideas on racial hygiene into effect. These ideas even exceeded the goals that the NS-health politics propagated until 1939 (van den Bussche, 1989).

b) We described that elements of social reform were more and more substituted by totalitarian and inhumane ideas in Weygandt's thinking. This process of value change, namely stressing racial hygienic measures, and neglecting educational and sociotherapeutic measures, is apparently correlated with sociocultural as well as biographical conditions.

c) Buss (1976) has shown that the liberal-democratic culture of Great-Britain in the 19th century with its "extreme optimism in the perfectibility of society and the individual" (p. 51) brought forth eugenics. The same culture, however, was so strong as to recognize totalitarian tendencies of eugenics and to stop their influence to a large extent. Obviously, Germany did not have such cultural correctives at her disposal to stop the popularity of eugenic ideology which had grown since the beginning of the world economic crisis in 1928 or so.

d) In view of the recent attractivity of eugenic techniques in Europe, we should examine sociocultural conditions of "expertocratic human breeding programmes" (Weingart, 1988) to be able to oppose inhumane developments you can observe in the debate with Singer (Löw, 1990). The Australian Bioethician expressed the opinion that severely handicapped people may be killed under certain circumstances:

"The killing of a disabled newborn child is morally not the same as the killing of a person. It is very often no wrong at all... The foetus, the severely retarded man 'merely vegetating', even the newborn child - all of them are members of the species *homo sapiens* without doubt, but none of them has self-awareness or has a sense for

the future or the ability to communicate with the other" (Singer, quoted in Krebs, p. 297).

Irrgang (1991) pointed out the "naturalistic false conclusion" inherent to the "ideology of eugenic improvement of mankind". In his excellent article, Irrgang shows how dangerous this ideology is for the humanity of our society.

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Skinner. Le problème du sujet psychologique

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ABSTRACT

The question of the "subject" represents the summit of every psychological system. This is why such a question summarizes the thought of their authors in the best possible way. The aim of this paper is to analyse Skinner's attitude towards the "psychological subject" issue, and to determine Skinner's place among all other objective psychologists in our century as a function of such an attitude.

I

La question du sujet constitue la pierre de touche de tout système psychologique. Elle sert à définir avec une plus grande exactitude le psychologue lui-même, et c'est là en même temps celle qui peut le mieux placer dans un schéma théorique des sciences humaines. Notre communication se borne uniquement à la psychologie et tâche de trouver la place qui doit correspondre à Skinner parmi ses collègues à propos du "sujet psychologique".

Le premier round sur la question du "sujet métaphysique" de la conduite -l'âme (soul)- eut comme protagoniste Locke (1690) avec sa théorie de la "tabula rasa". Cette théorie permettait de parler encore d'un "sujet psycho-empirique" -"l'esprit" (mind)-, dont Hume (1739-40) allait donner immédiatement une définition précise. L'attitude critique de Locke ouvrit la possibilité d'un traitement "scientifique" du sujet psychologique, que le XVIII^e siècle, "le siècle de l'éducation" par excellence (selon Ortega y Gasset), réouvrit en posant le dilemme "nature-éducation", aboutissant finalement, à cause de l'idée d' Helvetius (1758) -"l'éducation peut tout"-, au le triomphe de "l'éducation" sur la "nature": l'expérience détermine l'être et la façon d'être de "l'esprit humain". Le XIX^e siècle fut témoin d'une théorie (Spencer, 1854, Darwin, 1859) selon laquelle même l'espèce est le résultat de circonstances extérieures à l'organisme. Pendant les dix premières années du XX^e siècle, certains physiologistes (Pavlov, 1903; Bechterev, 1904) et psychologues (Thorndike, 1905) remplacèrent le sujet psycho-empirique par un "sujet neurologique". Dans la décade suivante l'exigence

de Watson (1913) d'un objectivisme radical conduisit le problème du "sujet psychologique" vers une tragique inflexion: quelle que soit sa nature -métaphysique, mentale ou neurologique-, n'importe quel "sujet interne" n'est pas vraiment intéressant pour un psychologue scientifique, car il n'est pas une entité "manifeste". Pour les objectifs de la science de la conduite -"prévoyance, contrôle et domination (de la conduite)" (Watson, 1930), l'exigence d'un sujet "mental" est inutile et inefficace, et le recours au sujet "organique" est insuffisant, étant donné l'insuffisance des connaissances des sciences biologiques. Si la thèse de Locke de la "tabula rasa" avait engendré une "psychologie sans âme", la théorie radicalement positiviste de Watson ouvrit la scène à une "psychologie sans sujet". Mais, autour de 1930, la science de la conduite se montra plus ouverte aux "explications théoriques", et dérivait vers l'admission de processus internes à l'organisme que l'on ne peut pas observer directement, qu'elle appela "variables intermédiaires" (Tolman, 1932) ou "constructions hypothétiques" (Hull, 1943), dont l'ensemble formait un "sujet de la conduite", fournissant une explication qui paraissait scientifique et suffisante du comportement. Quelle fut la position de Skinner devant ce panorama historique? Pour y répondre, nous utiliserons une double stratégie.

II

Suivant sa pensée positiviste radicale, Skinner exprima sa volonté d'approfondir la "description plutôt que l'explication": expliquer cette attitude est notre premier objectif. Les concepts de la science n'ont même pas, dans son opinion, le statut "d'hypothèse" et se définissent uniquement "comme des observations immédiates", comme de simples "groupes d'observations", ou bien comme "des représentations convenables de choses qui sont déjà connues" (1938). Derrière une telle pensée, on peut voir sans doute la position opérationnaliste du physicien Bridgman et le néopositivisme de Feigl. En appliquant cette pensée au "sujet psychologique" (1938), Skinner supprima, une fois pour toutes, toute "hypothèse" possible ou "théorie" scientifique, déjà formulée ou à formuler. Le but de la science du comportement -prédiction, contrôle, manipulation, domination de la conduite d'un organisme (1953)- peut être atteint indépendamment de n'importe quel intérêt théorique sur le "sujet" de celle-ci, car "l'abord empirique des variables manipulées (qui sont toujours extérieures à n'importe quel sujet supposé) dont la conduite est une fonction" (1970b), est tout ce dont le scientifique a besoin pour avoir "une bonne analyse expérimentale de la conduite". La célèbre question "les théories de l'apprentissage, sont-elles nécessaires?", recut de lui (1950) une réponse exacte, mais un peu frivole: "nous sommes peu préparés" pour les théories, et, si nous les admettons, c'est seulement "parce qu'elles sont amusantes".

Cette conception radicalement positiviste et descriptive de la science de la conduite impliquait un renoncement volontaire à toute investigation sur un "sujet" qui ne soit pas complètement réductible à des variables manipulables d'entrée et de sortie.

Skinner refusa expressément non seulement toutes les "entités situées dans l'organisme, appelées psychiques ou mentales" (1953, p. 59-61) -métaphysiques, comme l'âme, ou simplement empiriques, comme "l'esprit" ou "la conscience"-, mais aussi l'autre sujet interne, "neurologique", étant donné qu'il est peu utile "pour prévoir et contrôler une conduite spécifique" (1953, p.58). Sur ce dernier point, Skinner alla aussi loin qu'il put: "on étudie les processus génétiques et comportementaux d'une façon rigoureuse sans faire référence à la biochimie sous-jacente" (1969, p.168). La suppression de toute "théorie" sur un possible "sujet interne", dans l'opinion de Skinner, est évidente; son argument (1953) sur la conduite "boire un verre d'eau" est paradigmatique de toute sa pensée à ce sujet. Il conclut là: "on ne peut pas expliquer la conduite d'un système si nous sommes complètement situés dans son intérieur". Sans doute, Skinner ne voulait pas aller jusqu'à nier l'existence des "états internes" de l'organisme actif; il affirmait uniquement de tels états- "un sujet" parmi les variables d'entrée et de sortie- qu'ils ne sont pas significatifs pour l'analyse fonctionnelle scientifique de la conduite, et que, par conséquent, pour une compréhension certaine de cette conduite, il faut obligatoirement s'en tenir seulement "aux forces qui influent sur l'organisme et qui sont situées à l'extérieur".

III

L'autre clé pour comprendre la position de Skinner sur le "sujet psychologique" se trouve dans la révision de ses spécifications sur "le facteur critique" des processus de "conditionnement opérant". Elles servent en plus à mettre en relief sa position au sujet de la psychologie de son époque. Il y a quelque chose de nouveauté dans cette voie. Nous supposons maintenant que la pensée de Skinner sur le "conditionnement opérant" (1938) est déjà connue, avec ses "contingences de renforcement" corrélatives, de même que le schéma du comportement, où l'on peut placer en autres facteurs d'entrée, tels les stimulus discriminatifs, les stimulus aversifs, la privation, la satiété, l'âge ou l'espèce.

Le fait fondamental est le "renforcement". Mais nous sommes ici devant un phénomène qui ne peut pas être considéré comme absolu. Sa propre essence contient une liaison avec "quelque chose" où l'on doit placer le résultat de l'action renforçante: on pourrait dire, imitant Brentano, que l'idée de "renforcement" a la nature d'"intentionnalité". En une telle situation, la question de la spécification du destinataire final du renforcement prend une valeur décisive. Skinner lui-même fut explicite sur ce point, quoique d'une façon indirecte: étant donné, comme il le pense, que "l'unique caractéristique qui définit un stimulus comme renforçateur c'est que celui-ci renforce" (1953, p.102-103), nous devons poser la question qui en découle: pourquoi un renforçateur renforce? On pourrait s'attendre à ce qu'une réponse juste le reconduise à l'intérieur de l'organisme, pour y identifier la dimension "relationnelle" du phénomène du renforcement avec un, ou quelques uns, des "états intérieurs" -psychiques, psychologiques, neurologiques, glandulaires, musculaires, etc.- qui le constituent; et, d'une façon particulière, avec ses dimensions spécialement dynamiques, telles que les

besoins, les impulsions, les buts, les motifs, etc. Mais, Skinner arriva-t-il à récupérer vraiment ce "sujet", spécialement dynamique, de la conduite?

Dans la décade de 1920, l'investigation psychologique avait été particulièrement sensible au problème des "déterminants dynamiques" de la conduite. Les fonctionnalistes de Chicago, par exemple (Carr, 1925), avec leur idée de "motivating stimulus" intérieur (p.e., faim), n'oublièrent pas de placer dans leurs textes un chapitre relatif au "drive"; Dewey (1922) fit de même. McDougall (1930, 1934) n'abandonna jamais la psychologie "hormique". Dans la formulation (E-O-R) de Woodworth (1918, cf. 1930) "l'organisme" comprenait une dimension "impulsive" innée; et dans ces mêmes années, Troland (1928) était en train d'écrire sur "les origines de la motivation humaine". Parmi les plus experimentalistes, Lewin (1922) affirmait que le facteur de motivation est nécessaire pour la formation et pour la répétition des associations mentales; Tolman, qui écrivit pendant ces années (Tolman, 1920, 1925a et b) plusieurs articles sur le "but", l'introduisit (1932) à une place prééminente comme "déterminant" interne (variable intermédiaire) de la conduite. Leeper (1935) sentit le besoin de parler de la "motivation" pour expliquer les associations ou les habitudes de conduite, leur différence et leur sélection par les organismes. Et il en fut de même pour un grand nombre de scientifiques. Ils défendaient tous la nécessité d'un "sujet motivationnel" de la conduite. Même un auteur experimentaliste-né, comme eux, mais pas du tout suspect de subjectivisme, comme le neurologue K. Lashley (1934), avait fini par refuser le mécanisme dur de l'hypothèse associative, pour conclure que les faits de laboratoire conduisent à "caractériser la conduite comme intentionnelle" et à exiger la existence d'un "sujet neurologique" de la conduite qui ne soit pas réductible à un simple reflet.

La longue tradition historique sur le "conditionnement opérant" pouvait offrir à Skinner des attraits plus suggestifs et plus puissants, même, que les précédents, pour obtenir le retour à un "sujet interne" dynamique de la conduite. Le susdit sujet est supposé dans les travaux de Spencer (1854) et de Bain (1855), de même que dans l'oeuvre de Morgan (1896). Chez ce dernier il s'agit d'une "constitution organique impulsive" innée et acquise que certains faits servent à "confirmer". Ces faits ont alors le statut de vrais "renforcements" des connections corticales sous-jacentes aux connections de la situation-réaction qui conduit à l'accomplissement des attentes ou des intentions de l'organisme, lorsque d'autres faits sont inhibitoires. Thorndike (1905) défendit, au début, l'existence d'un substratum neurologique -plus exactement, l'écorce cérébrale- où "s'impriment" les connections "situation-réaction" renforcées, sous la forme de liaisons "synaptiques"; mais, en 1932 il adopta de plus une position plus fonctionnaliste où la récompense ou le "renforcement" était défini par sa condition d'"influence confirmante de tendances organiques", de telle façon qu'il finit par admettre une "intentionnalité" dans la conduite, relative à une sorte de "causalité biologique" qui la dirige. Enfin, il y avait encore la théorie unifactorielle de l'apprentissage de Hull, selon laquelle le "renforcement" -jouant le rôle de principe primaire- se définit par une corrélation à la "réduction de besoin", ou de la tendance

qui vient après lui (1943); par conséquent, "besoin" ou "tendance" constituent un fait essentiel du système théorique de Hull qui analyse le "sujet de la conduite", avec même une certaine tendance vers l'organique.

De la même façon qu'eux, Skinner fut un théoricien du "renforcement". Mais, quelle était la position vis-à-vis du sujet que sa théorie du renforcement paraissait être en train d'impliquer? Plus exactement, qu'était pour lui cet élément de relation par rapport auquel on définit un phénomène comme "un état de choses renforçant"? Skinner attaqua expressément les conceptions de ses prédécesseurs: celle de Thorndike, par exemple, parce qu'elle est trop théorique et subjective (1953); et celle de Hull parce qu'elle contient trop d'erreurs, car, même si on ne peut pas nier qu'il y a une étroite liaison entre la "privation" -et le s'ensuivent "besoin" et "tendance"- et le "conditionnement opérant", il n'est pas vrai que le processus du "conditionnement opérant" réduit toujours la "privation"; c'est le cas des sujets qui reçoivent uniquement le renforcement, salaire, qualifications, etc. à la fin d'une longue chaîne de conduites; tout ce qu'on peut dire à ce sujet -dit Skinner- c'est que "le genre d'événement qui réduit la privation est aussi renforçant" (1953). Dans une telle situation, la question "pourquoi un renforcement renforce-t-il?" est encore là.

La réponse, ajoutera Skinner, "doit être cherchée dans le processus évolutif". La sienne propre, plus exactement, est caractérisée par une référence à "l'importante signification biologique des renforcements primaires" (1953), l'eau, les aliments, le sexe. Certains événements arrivent à acquérir le caractère de "renforcement" précisément parce que leur action aboutit à l'accomplissement des conditions biologiques nécessaires au fonctionnement de l'organisme. Cette réponse, avec son implication "d'un sujet biologique" nous paraît vraiment correcte. Mais, du point de vue de la psychologie de l'individu, où l'on place son oeuvre, elle est clairement insuffisante à cause de sa généralité. Pour fournir une "explication théorique" de la conduite, Skinner devrait spécifier encore le contenu, réel ou supposé comme hypothèse, de ce "sujet", afin de pour rendre compréhensibles les relations entre les variables antécédentes et la variable conséquente dans le processus comportemental.

Le fait d'avoir écarté "les états internes" -psychiques et neurologiques- réduit sans doute considérablement la réponse. Il y a encore les variables "privation et satiété", "espèce" ou "les variables héréditaires" (1956). Mais, pour Skinner, la valeur scientifique de la variable "privation et satiété" dépend uniquement de ce qu'on la considère comme une variable "externe" à l'organisme. Son équivalent "interne" (par exemple, les déterminants "besoin" ou "tendance" des systèmes théoriques de Woodworth, Carr, Tolman ou Hull) se situe en dehors des conditions épistémologiques que son attitude positiviste radicale implique, et, par conséquent, n'est pas compatible avec l'explication scientifique. Il reste encore la variable "espèce" et les variables "héréditaires". Skinner accepte (1956) les dernières, mais seulement parce "qu'elles sont observables et supposées prouvables", comme le sont des variables "externe" (telles que la taille, la longueur des membres, la possession des doigts des mains, etc.) dont la valeur pour la prédiction et le contrôle de la conduite, quoique

“important”, est très secondaire, en face du rôle joué par la puissante variable “contingences de renforcement”; de ce côté, non plus, nous ne trouvons pas une spécification significative de ce “sujet biologique” de la conduite. D’une façon surprenante, dans l’oeuvre de Skinner, ne manquent pas les allusions à la variable “espèce” comme élément référentiel du caractère “renforçant” de certains événements pour l’organisme (1969); par exemple, si on met “l’objet artistique” en relation avec les “contingences de la survivance dans l’évolution du genre humain” (1970a), ceci devrait impliquer que les facteurs philogénétiques participeraient au processus de renforcement avec préférence sur les ontogénétiques. Mais cette nouvelle stratégie est également inopérante en relation à la spécification théorique d’un sujet du comportement: en premier lieu, parce qu’un sujet aussi abstrait que “l’espèce” sort des limites du positivisme descriptif, auquel s’auto-limite Skinner; et, en second lieu, parce que lui-même était conscient que sur les contingences philogénétiques de renforcement, “aussi bien celles qui sont expérimentales que celles qui sont conceptuelles” (1969), étant donné leur “éloignement”, nous en savons encore beaucoup moins que sur les contingences ontogénétiques. C’est pour celà que, pour Skinner, la sélection naturelle d’une façon déterminée de la conduite est encore une simple inférence.

Au vu de tout celà, je pense que le positivisme radicalement descriptif de Skinner ne permet pas une vraiment fondée référence au sujet psychologique intérieur, et que, dans sa pensée, l’élément relationnel interne de “le renforcement” n’est pas suffisamment spécifié. En fait, il n’est pas spécifié du tout, non comme “explication”, ni comme “description” de la conduite.

IV

CONCLUSION

Du point de vue du sujet psychologique, l’histoire de la psychologie montre un processus continu d’extériorisation du contenu du sujet déjà nommé. Voilà une tentative d’interprétation historique. Il y eut, en premier lieu, un macro-processus historique: *entité métaphysique*, dans la “psychologie de l’âme”, le sujet psychologique devint un simple *phénomène mental*, depuis Locke -une psychologie sans âme, oui, mais qui était encore une investigation sur “l’esprit” (mind) ou “la conscience”-, pour déboucher finalement, une fois celle-ci refusée sous le prétexte d’un objectivisme radical, en une “psychologie sans sujet psychique”. Il y eut, en plus, à l’intérieur de cette dernière psychologie, un micro-processus historique, également dans le sens de l’extériorisation du sujet: le sujet réel de Morgan fut réduit d’abord à une entité neurologique (Pavlov, Thorndike ou Lashley), puis à un ensemble de constructions hypothétiques sur les processus internes à l’organisme (Tolman, Hull), pour aboutir même à la suppression définitive de telles constructions dans l’oeuvre de Skinner. Avec son radicalisme, nous sommes non seulement devant une psychologie sans “sujet psychique”, mais aussi devant une “psychologie sans sujet”, prenant celui-ci

dans le sens le plus large de l'expression "sujet interne", réel ou supposé, placé parmi les variables manifestes. S'étant limité à la fameuse "boîte noire" parmi les variables antécédentes et la conduite, Skinner est devenu le plus fidèle interprète de "l'état positif" de Comte dans le champ de la psychologie, un positivisme radical auquel l'épistémologie de Stuart Mill et surtout de celle de Mach avaient fait perdre sa valeur pour justifier le phénoménisme conscientialiste de la fin du XIXe siècle et des débuts du XXe, mais auquel la nouvelle épistémologie, néopositiviste et opérationnaliste, qu'il adopta, permit d'être sauvé.

Skinner n'avait pas la mentalité d'un "théoricien" dans le même sens que ses contemporains, Hull ou Tolman. Son premier intérêt était "d'agir" sur la conduite; de là sa propre pensée sur l'investigation comportementale: prédiction, contrôle, domination. Etant donné que "la conduite est réglée et dirigée par des lois" (1953), cette investigation peut être accomplie en manipulant les variables externes dont elle est fonction, indépendamment d'"un hypothétique sujet interne" quel qu'il soit; et, en une telle situation, la "théorie" est de trop. Skinner proclame ouvertement ce renoncement volontaire à une telle investigation théorique; il le fait, par exemple, quand il affirme qu'aux concepts de la science de la conduite "on ne donne pas de propriétés physiologiques", qu' "un réflexe n'est pas un arc, une tendance n'est pas l'état d'un centre, l'extinction n'est pas l'épuisement d'une substance ou un état physiologique" (1938, p.60); ou lorsqu'il affirme qu'on ne peut pas définir le fait d'apprendre comme la modification d'un état de l'organisme, mais uniquement comme un simple "changement dans la probabilité de la réponse". Son point fort se situe du côté opposé, dans l'étude des dimensions pratiques de la conduite, celles qui sont naturellement en relation avec ses fins et exigences. De là ses nombreux travaux, certainement brillants, sur les "programmes de renforcement", la communauté utopique *Walden Two*, le projet de "communautés expérimentales", "le dessin de cultures", les "machines à enseigner" ou celles destinées à surveiller les enfants, l'élevage de colombes "avec des fins belliqueuses", ses conseils sur la vieillesse, etc. Au vu de cette opposition théorie-pratique, nous pensons qu'au sujet de l'investigation psychologique de Skinner, on devrait ouvrir expressément la vieille question "la psychologie: science ou technique" que Titchener avait déjà mise sur la table en 1914. Pourtant, la question spécifique, concernant le sujet du contenu de sa psychologie, de savoir s'il va ou non plus loin qu'une "technologie" ou "ingénierie" de la conduite, ne peut pas être contenue dans ce travail.

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Psychological instruments as the subject matter of the history of science and the history of psychology

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INTRODUCTION

The aim of this paper is to present some reflections about the utility the study of research instruments may have for the history of psychology. Although some work has already been done within our discipline dealing with this subject (Algarabel & Soler, 1991; Popplestone & McPherson, 1980; Sokal, Davis & Merzbach, 1975, 1976), there are still many possibilities to explore. As Leary points out (1990), "the complete and fascinating story of the dialectic between ideas and devices in psychology -how ideas lead to the invention of devices and how devices lead to the modification and extension of ideas- has not yet been adequately told" (p. 53).

One reason why studies on instruments are so scarce might lie in the comparative youth of the history of psychology as a discipline, and the essentially presentist bias that pervaded its development until recently. After a long time almost exclusively devoted to the conceptual evolution of psychology, current historiography tends to consider science as an integral system, in which social, technological, cultural, etc. factors intervene (Caparrós, 1984; Van Hoon, 1984), in addition to factors inherent to scientific organization: communication channels, internal structure (Carpintero, 1980), everyday research practices (Danziger, 1985), etc. A promising trend of studies on the models and metaphors embodied in scientific theories (Leary, 1990; Métraux, 1984), their origins and implications, has recently been initiated.

We believe that the study of psychological instruments can illuminate many of these aspects of the discipline. In order to support this thesis, we shall rely on the reflections already made on this topic from other spheres of the History of science.

The role of instruments in the history of science

In the close domain of the history of physical and natural sciences, an important effort to this respect has been made by the Institute of History of the Natural Sciences and Technology, in the Academy of Sciences of the URSS. A group of researchers, to

which Professor Serguei Kará-Murzá belongs, is attempting to elaborate "maps" of the structure of scientific communities through the analysis of their publications. It is assumed that scientific papers are linked through the bibliographical quotations they make to earlier works. Through the analysis of these quotations, we can find a "nucleus" of frequently quoted papers which, in addition, tend to appear together (i.e., they are "co-cited", or cited in the same paper). These highly quoted papers would represent the starting point or the avant garde of a research area, the rest of contributions becoming linked by them. A picture may be thus obtained not only of the current state of the field, but also of the way it has developed over time.

It has been usually found that many of these most cited papers are methodological in character. They may be even more cited than theoretical papers, due to their usefulness as a starting point for several research areas. Kará-Murzá is aware of the important role played by these works relating to methods and instruments, and concludes that they may have the following effects (Kará-Murzá, 1985):

- **The emergence of new lines of research.** The improvement of instruments may provide data not previously available, thus opening a new research line or allowing the retrieval of a previously discarded one because of the lack of an appropriate methodology.

- **The relationship between scientific communities.** Research fields dealing with different theoretical problems or even belonging to different disciplines can be related through the use of similar techniques. These methodological relationships cut across relationships of a theoretical or conceptual nature and contribute to the integration of science as a whole.

- **The delimitation of periods in scientific development.** Certain instrumental advances may cause internal revolutions within a research field or a reorientation in its approach; consequently, historians should take into account instrumental changes when considering the development of a given field.

- **The spreading of certain theoretical concepts.** When a scientific field adopts apparatus and techniques used in some other field, it may also implicitly adopt some theoretical points of view linked to the usage of those techniques. Instruments may thus contribute to spreading scientific theories to new research areas.

Of course, in these phenomena -emergence of new areas, exchange of ideas and concepts, etc.- many factors are implied. The role played by instruments may in some cases be minimal or nonexistent; in others it may be decisive. For this reason, the historian should take into account instruments as one possible meaningful pointer, among others, of the evolution of a discipline.

Instruments and the evolution of psychology

Let us now consider the possibilities of this approach for the history of our discipline.

To begin with, if we take a look at the general development of scientific psychology, we may see how the emergence of new research areas or schools, as well

as deep changes that have taken place in it, are linked to instrumental factors. In some cases, these might have been decisive; in others, they surely play only a secondary role. Here only a few most important landmarks will be pointed out; a more detailed historical research will be able to answer to the question of the preponderance of instruments.

Firstly, the very emergence of psychology as an independent discipline is related to the use of instruments. Most historians agree in symbolically considering the date of the foundation of the first psychological laboratory as the starting point of our science. Wundt's main contribution as founding father of scientific psychology does not so much lie in his long time forgotten or misunderstood theoretical contribution, but in the incorporation of apparatus and research techniques coming from physics or physiology, and in the research training of the first generation of psychologists, and their introduction to the various laboratory techniques.

In the first psychological laboratories, researchers became involved in the study of simple processes and reaction times, and adopted a lot of instruments from other disciplines. Most of these tools were of a mechanical nature, but some of them (i.e., the kymograph), functioned electrically. In any case, the basic function of these instruments was to provoke very precise stimulations: apparatus for presentation of stimuli, such as the stroboscope, the tachistoscope, or the pressure balance; or devoted to time measurement, such as the chronoscope; or devised to register the subject's responses, especially physiological responses (for example, the kymograph). The main research topics at this moment were sensation, perception, attention, mental chronometry and the search for physiological correlates of emotions.

The use of instruments at the end of the past century and even at the first decades of the present (an exhaustive account of instruments existing at the time may be found in Titchener's handbook; Titchener, 1901-1905) makes scientifically respectable a research field traditionally dealt with by philosophers. Undoubtedly, the earliest psychologists were aware of that (Popplestone & McPherson, 1980): in the earliest experiments much more importance was given to instrumental accuracy than to questions about experimental design and variable control, the relevance of which was not yet clear.

From the earlier decades of the twentieth century, the evolution of psychological instrumentation has followed very different routes. In the case of psychophysiology there has been a portentous instrumental progress (electrodes, microelectrodes, electroencephalography, etc.), parallel to instrumental development in medical sciences. In other psychological fields, the emergence of new theories trying to replace the Wundtian system has been also linked to changes in tools. Thus, the Gestalt school, in order to create complex stimuli for supporting its holistic theory of perception, favours instruments like ambiguous figures or the stroboscope.

On his part, the school that was to dominate the psychological arena, i.e. behaviorism, was provided with techniques and instruments by animal psychology. Watson's revolutionary proposal implied a radical methodological change. Traditional

research fields and, consequently, the tools related to them, were relegated to a secondary role, while problem-boxes and mazes, which had already been used by researchers in comparative psychology for some time (Boakes, 1984), became indispensable for the study of learning. Different kinds of mazes and boxes were elaborated, according to the problem under study.

In the field of animal psychology (particularly between 1920 and 1960) instrumentation was tremendously developed and became simpler, whereas tools more directly related with human research remained steady.

But we wish to emphasize especially two facts. In the first place, it is true that mazes and boxes were created at times as a function of the problem under study, that is, with an aim to create a problem situation. In other words, theoretical assumptions determine the kind of tool to use.

However, in the second place, the design of apparatus has sometimes theoretical consequences. This is the case of some boxes. Köhler's criticisms of Thorndike's box was at the time a well-known controversy on the kind of apparatus used. Köhler points out that the way Thorndike had set up the mechanism, it was impossible for the animal to show an intelligent behavior; the liberating mechanism was only visible for the experimenter, not for the animal, which was thus forced to behave in a trial-and-error manner. On the contrary, the instrument used by Köhler in his experiments with chimpanzees is a more open situation, where the animal has every element needed for the solution of the problem at its disposal and within its visual field. The resultant theory and interpretation, therefore, depended upon the instrument used by the researcher.

In a similar line, changes introduced by Skinner in Thorndike's problem-box allowed for a new theoretical elaboration of learning by reinforcement. By replacing a reinforcement administered within the box (food, etc.) for the earlier successful situation (getting out of the box), the animal could produce more than one "correct" answer in the same trial. This, together with the incorporation of an accumulative register, makes the response rate the main dependent variable, thus allowing an operational definition of the effect of reinforcement.

Finally, in this brief historical account it must be noted that, after the crisis of behaviorism, the incorporation of computers has undoubtedly been the most important instrumental innovation (Sidowski, 1975). The influence of computers has been basic in three aspects: in the laboratory realm, where they are used either for presenting stimuli or for measuring and registering responses; in data analysis; and finally, as we shall see later, as a model for theory construction, through simulation and artificial intelligence.

In the context of this paper, we would particularly like to emphasize that the use and development of the computer has made possible, from the mid fifties, the renewed study of psychological aspects that behaviorist methodology did not allowed for. We refer, obviously, to mental processes. In the case of the computer, this permits to

express such processes in a particular language, that of information processing. The first to develop this "computational model" was Miller in 1953, although the emergence of simulation models and artificial intelligence took place in the sixties.

Although the use of the computer was not within the reach of every laboratory, and in many occasions was rather used as a laboratory tool, -a role noticeable for the simplification and automatization it allows for-, it is the former usage that is the most important contribution of the computer. We will refer these ideas later, in another context.

The relationship between scientific communities

Another effect of the instruments, according to Kará-Murzá, is to relate areas with common methods with one another; areas otherwise separated in the science "map" for belonging to different disciplines or dealing with different subject matters. Thus, there seems to be a double network among scientific communities: one, conceptual and disciplinary relationships; the other, proximity in methods and techniques. On the other hand, methodological proximity favours the increasing closeness in language, hypothesis, etc. of areas initially isolated.

Is there something similar in psychology? As a whole, our discipline has an object which may be characterized in general as "human activity", and all its practitioners agree in acknowledging the need of scientific method for a serious approach to psychology. Except for these few points in common, however, psychology is divided into many fields: psychophysiology, clinical psychology, social psychology, experimental psychology... There are great many differences, both terminological and instrumental. This has resulted, at times, in situations of crisis or, at least, in a permanent awareness of a rich but worrying "diversity" (Mayor & Pérez, 1989; Yela, 1989). However, it is assumed that all these fields are placed in a common territory, defined as "psychological science", and it is hoped that, in the future, the differences will be reconciled.

At the same time, every field has wide links with others belonging to related disciplines, and shares instruments, language and some informations with them. Thus, psychophysicists share instruments and techniques with their colleagues in medicine; clinical psychologists resemble psychiatrists in terminology and tools; social psychologists exchange procedures with sociologists; and so on. If we analyzed the references of scientific literature related to each field, as Soviet historians are doing in other sciences, we would undoubtedly be able to detect those "nuclei" of significant works cited by colleagues from both sides, and there would certainly be relevant methodological contributions among them.

At the same time, one may question to what extent the use of different techniques and instruments has sometimes made more difficult the integration of different psychological research areas. The long controversy between the users of experimental method and those of correlational method has resulted in a mutual misunderstanding

for a long time, and may not be but a symptom; the use of different research techniques makes theoretical integration impossible or very difficult.

The influence of instruments in theories

Let us finally consider instruments as means of transmission of certain theoretical notions. As pointed out above, according to Kará-Murzá, the transfer of apparatus and techniques from one scientific field to another may imply -even in an unconscious way- the adoption of theoretical assumptions linked to such instruments.

In our case, the best example may be found in the impact produced in psychology by the introduction of the computer. We have already mentioned its methodological advantages and the possibility it offers to reactivate the long time abandoned study of mind. But surely its more noticeable influence lies in the way it has theoretically shaped (at least, until recently) cognitive psychology. On the one hand, the computer is used to simulate mental processes; on the other hand, terms from cybernetics are used to describe mental processes and structures. The so-called "computer metaphor" has become a way of doing psychology. The transfer of concepts from one science to another may be here clearly seen: in this case, from the field of artificial intelligence to psychology. The computer, only an instrument, has served as a model for general mental functioning (Fodor, 1968; Miller, Galanter & Pribram, 1960; Putnam, 1960) and for different specific aspects: perceptual processes (Hoffman, Cochran & Nead, 1990), memory (Métraux, 1984), neural processes (Pribram, 1990), etc.

In order to support the existence of an influence of instruments in theories, we may refer to examples not only of tools adopted from other disciplines, but of those created by psychologists themselves.

For instance, there is the case of attentional theories, analyzed by Leahey (1979). The topic of attention was dealt with by the earliest psychologists, and was recovered after some oblivion by current cognitive psychology. As we noted in earlier papers (García-Sevilla, Pedraja & Vera, 1988; García-Sevilla, Quiñones, Vera & Pedraja, 1990), the conceptualization of attention at the end of the past century had many aspects in common with today's theories; but also important differences subsist, if we compare Wundt's attentional theory with the approach to attention as a filter, advanced a few decades ago. Leahey thinks these differences are linked to the use of different techniques and apparatus. A century ago, the study of attention was based in a "visual paradigm" (for example, presentation of stimuli through a tachistoscope), and attention was conceived as an active process of focusing on a point by consciousness; filter models, however, used an "auditive paradigm" (most frequently, dichotic listening), and consequently emphasize the selective character of attention, which filters the received stimuli. Instrumental divergences may thus give place to different theories on a phenomenon.

Similarly, other contributions were those by Gigerenzer (1991) and Smith (1990). Gigerenzer has centered in an "intellectual", not a "physical" tool: the

techniques of statistical analysis. By means of what he calls the "tools-to-theories heuristic", he argues that instruments suggest certain theoretical metaphors which are accepted by the scientific community at the same time that the use of the instrument is assumed. This is exemplified by showing that inferential statistics (an analytical tool) generate numerous interpretations of cognitive processes which maintain the existence of an "intuitive statistician" inside us.

On his part, when dealing with the metaphors used by the most relevant neobehaviorist researchers, Smith (1990) has shown, in the case of Tolman, a continuity between the mazes he employed (his favourite tools), his way of conceptualizing learning (as a spatial representation of the environment by the organism) and even his wider thoughts about science and the world in general (for example, he spoke about the world as a huge "God-given maze"). In a few words, the maze has gone beyond its utility as a tool, and become an essential metaphor in his scientific theory.

The relevance of instruments as a generators or transmitters of theoretical metaphors and models is very promising, and fits quite well into current efforts to understand how scientific theories are generated.

FINAL COMMENTS

It has been our aim in this paper to suggest the potential usefulness the study of scientific instruments may have for the historian of psychology. We hope that the mentioned examples are enough to show that scientific apparatus and tools deserve attention not only as vestiges from the past, but also because of their interaction with other more visible aspects of science.

As was eloquently put by Sokal, Davis & Merzbach (1975) "in fact, like any good historical source, the only limits of what can be learned from psychological instruments of the past are those of the imagination of the psychologist or historian working with them" (p. 286). We believe the new psychological historiography developed in the last fifteen years is much more sensitive to contextual aspects of science and will be able to make a very extensive use of these possibilities.

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The study of emotions in Spanish Renaissance: M. Sabuco's work

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ABSTRACT

In the fifteenth century, some Spanish Renaissantists were dedicated to the study of the emotional and volitive aspects of human nature. One of these humanists was the apothecary Miguel Sabuco, author of the work New Philosophy of Man's Nature which deals with personal dispositions. In it, we find anticipations of clinical and therapeutical approaches which form part of the psychological baggage of every epoch.

INTRODUCTION.

In 1587, the work *New Philosophy of Man's Nature* by Miguel Sabuco ¹was published. Miguel Sabuco (1525?-post 1589), belongs to a group of authors, a characteristic of Spanish thinking in every era, whose contribution goes beyond their own specialization, being attracted to philosophy. Juan Huarte de San Juan and Francisco Vallés, our Renaissancists, also belong to this group.²

The objective of the book is the knowledge of our personal dispositions. This knowledge will permit us to keep healthy and avoid illness. It adopts the form of a dialogue between shepherds, having a structure similar to that of the Socratic dialogues: someone who expounds an idea, someone who questions and raises objections, and someone else who listens. It is divided into four parts: a colloquy referring to the knowledge of one's self, a colloquy which deals with the contexture of the world, a colloquy about the things that improve this world, and the last one about the relief and remedies of medicine.³

Contrary to what happened with Huarte de San Juan and his *Examination of Talents*⁴ few references to this chemist from Alcaraz in works about psychology in Spain, although he attained certain renown among scientists in the nineteenth century,

when his theory of *nervous juice* was defended as an example of the Spanish capacity for innovation.⁵ In it he formulates an innovating physiological hypothesis; the nervous juice, originated in the sap, which the brain takes from food, will later be distributed by the nerves which go out from the spinal cord:⁶

"This principal root of the brain and the posterior part sends its channel or trunk downwards (...) which is the spinal cord (...) (From here) other shoots of this tree branch out, these being the nerves (...) Thus the principal root of the brain takes its juice from the small roots (...) and through the nerves of the duramater (...) and goes out of the vertex (...) and (from) there diffuses itself to the exterior surface" (pp. 234-235)

⁷Sabuco starts from Galeno's classical concept about health and illness as a lack of balance and harmony in the body and its humors, emphasizing the relation between the psychological and the physiological. His philosophical basis is scholastic⁷ and really belongs to a more speculative current, for which experience is not the touchstone for proof, but a corroborating didactic resource by way of examples of that which is explained: he uses his scientific knowledge as a support for his philosophical ideas.⁸

In the primacy that he concedes to the soul, Sabuco comes close to the Platonic opinion:

"There is a prince, who is the cause of all acts, effects, movements and actions (...) which is the soul, that descends from the brain, which resides in the head (...) because this understanding and will, neither are nor consist of a corporal organ, as are the cells of the brain, which serve the soul as house servants" (p. 209).

However, he points out that, for learning to take place, the species must enter by means of the senses, and then understanding judges, will evaluates and memory retains. Man has a vegetative, a sentient, and an intellectual part, owing the affections to the sentient part together with the passions, which are at basis of health and illness.

"(...) man is either in increment, which is health (...) or in decrement and illness, leaving and throwing out what he has received, because of the falls, catarrh and flows of the prince of this house (...) for health and illness, virtues and vices, no moderation or lack of moderation are of greater importance than those of the soul with the body itself" (p. 218).

Emotional and volitive aspects of human nature.

Guided by the maxim *nosce te ipsum*, Sabuco deals with the affections, causes, according to him, of all kinds of illnesses, and which have no apparent basis:

"By will one hates (...) and loves and desires; fears and hates; hopes and enjoys; joy and pleasure; anger and grief, terror, carefulness and anguish (...) Because of all this, so many kinds of illnesses arise (...) and if it is minor (...) it leaves, because of the same reasons, the mood for illness in the body" (p. 83).

The fundamental supposition is that the origin of the complaint is not due to the differences in nourishment nor the imbalance of moods: the origin of the disturbance is mental and it is produced by the imbalance of our psychic faculties, which release the greater decrement of the nervous juice.⁹ The procedure used by the author in his explanation is to start from the diagnosis of the complaint and the analysis of its causes, followed by the specific treatment to moderate its effects. Sabuco's position might be called, keeping distances, psychodynamic, as he situates the origin of this kind of complaint in the psychic. So, the imagination can be as much of a help in the solution of problems as a factor which potentially is a generator of these problems, since M. Sabuco refers to the prejudicial effects of anger and grief, both real and imaginary:

"Man should know this, that not only when anger and grief are true they kill him, but also when they are false and make believe, with just the suspicion" (p. 88).

M. Sabuco also studies those small losses and injuries which create everyday tensions in people, and when accumulated, provoke illness, madness and even death. All of this with a hypothetical approach and considering the individual differences: "Loneliness is bad for sad and melancholy people, and cause them more harm than to others" (p. 134). And also: "This feeling of anger and grief, is more present in women and more so in pregnant women" (p. 87).

The therapeutic intention is shown throughout the work, and the techniques recommended could not be more useful and more up-to-date. Guided by understanding and reason, they seem to us, to be true anticipations of modern cognitive strategies.¹⁰

Thus, when dealing with anger and grief (in the chapter 3), he proposes the rational analysis of the problems in a form which appears similar to the way it is done in Rational Emotive Therapy¹¹, or the Rational Approach¹², and which M. Sabuco condenses into one sentence: "To do willingly and with pleasure what one is obliged to do" (p. 91).

Within the cognitive line, we could include the remedy which he offers when faced with sadness: "When the hope of what you desire dies, then search, inquire and imagine another one". (p. 96).

The imagination plays an essential role in the improvement of complaints and the solutions of problems, and today it is used as a determining factor in the techniques of emotional self-control¹³ and systematic desensitization. In the same way Sabuco identifies it as a factor that is potentially a generator of problems. His reflections are very close to the role which today has been conceded to the imagination in psychopathological perturbations¹⁴. He even points out certain defense mechanisms (in the most up-to-date version of coping strategies) of the displacement and negation types which provide adequate mechanisms in managing stress: "The thing that always annoys you in it, take it away or make it distant" (p. 96). And also "small losses and injuries" (p. 86) which provoke changes in the balance of human beings, as well as atmospheric changes, "changes of heaven and earth, and of the weather and the air"

provoke complaints. The conceptualisation of stressing situations in the nosological classification today, such as the DSM, include these factors. In the DSM III, vital events are mentioned, as related to the level of stress of the axis V, and we find ideas about this in M. Sabuco too: "the affection of pleasure and of happiness, when unexpected and big, can kill suddenly" (p. 105).

The therapeutic value of words is cited by M. Sabuco over and over again: "...the best medicine of all is forgotten and unusual in the world, it is words: these will be according to the case in question." (p. 92).

Conversation can also serve to instil hope, albeit if imaginary (p. 107) very similar to the idea of positive expectations in the theories of learning (e.g. Bandura) and attribution (e.g. Weiner).

CONCLUSION

From what we have shown here, we want to emphasize some ideas which are the core of the thinking of M. Sabuco. His conception of human nature lies within the current of christian humanism with all that implies: voluntary self-control of actions and moral authority.

He analyses the soul according to the classic Aristotelian doctrine, with its constituent parts -vegetative, sensitive and rational- and makes the passions depend on the sensitive; but he is Platonic in the predominance of the psychic over the corporal. The philosophical unity resides in the cerebrospinal system.

His concept of human nature derives from his personal vision of the world, which is presented as a model of order and integration of everything in nature and, from here, in the great work of the Creator.

All the philosophy of these dialogues is subordinated to self-knowledge; this is a philosophical book which uses current scientific knowledge to support the philosophical concepts defended in it.

Finally, we want to point out that when presenting the contributions of M. Sabuco, we cannot forget that they were created in a completely different historical context. However, we cannot either omit to bring out how these theories and therapeutic techniques anticipate many of the methods of present day psychological intervention. This is a necessary task that historians can contribute to current psychology.

NOTES

- ¹ Although the book was published as a work by Oliva Sabuco de Nantes, Miguel Sabuco's daughter, historical research seems to favour the attribution to the father. About this, see J. Marco-Hidalgo (1903) "Doña Oliva Sabuco No Fue Escritora," *Revista de Archivos, Bibliotecas y Museos*, 7, - 1-13.
- ² A clear and documented exposition of the intellectual context in which Miguel Sabuco developed his work can be found in José L. Abellán (1979), *Historia Crítica del Pensamiento Español* (2 vols.). Madrid: Espasa-Calpe.
- ³ Sandalio Rodríguez-Domínguez (1988) "Grandes Temas Psico-Antropológicos de la Cultura Española Humanístico-Renacentista", in A. Rosa, J. Quintana & E. Lafuente (eds.), *Psicología e Historia*. Madrid: Univ. Autónoma de Madrid (pp. 61-74).
- ⁴ Juan Huarte de San Juan (1988), *Examen de Ingenios para las Ciencias*. Barcelona: PPU. The original edition dates back from 1575. This new edition has been introduced and commented by Esteban Torre.
- ⁵ The question about Spanish scientific past can be found in M. Menéndez-Pelayo (1953) *La Ciencia Española*. Madrid: CSIC. See specially the chapter "La Antoniana Margarita de Gómez Pereira".
- ⁶ José M. López-Piñero, Thomas F. Glick, Víctor Navarro-Brotons & E. Portela-Marco (1983) *Diccionario Histórico de la Ciencia Moderna en España* (2 vols.). Barcelona: Península.
- ⁷ Every literal quotation in this paper belongs to the following edition of Sabuco's work: Oliva Sabuco de Nantes (1981) *Nueva Filosofía de la Naturaleza del Hombre*. Madrid: Editora Nacional. Prepared by A. Martínez-Tomé.
- ⁸ A. Martínez (1987) "Los Orígenes del Mito de Oliva Sabuco en los Albores de la Ilustración," *Al-Basit*, 13, 140-143.
- ⁹ A. Guy (1987). "Miguel Sabuco, Psicólogo de las Pasiones y Precursor de la Medicina Psicosomática", *Al-Basit*, 13, 115-120.
- ¹⁰ We do not try to present Sabuco's work as a "foundation" but as an "anticipation", according to the distinction drawn between the two terms in G. Sarup (1978). "Historical Antecedents of Psychology: The Recurrent Issue of Old Wine in New Bottles," *American Psychologist*, 33, 478-485.
- ¹¹ A. Beck (1976) *Cognitive Therapy and the Emotional Disorder*. Nueva York: International Universities Press.
- ¹² Albert Ellis (1962) *Reason and Emotion in Psychotherapy*. Nueva York: Lyle Stuart.
- ¹³ See, for example, M. McKay, M. Davis & P. Fanning (1985) *Técnicas cognitivas para el Tratamiento del Estrés*. Barcelona: Martínez Roca.
- ¹⁴ A. Beck (1972). *Depression: Causes and Treatment*. Philadelphia: University of Pennsylvania Press.

What is the place of the history of psychology?

Some thoughts about methodology

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ABSTRACT

This paper sketches some methodological tools for the development of a methodology of History of Psychology. It begins stating the functions and subject of this discipline. After an examination of the nature of Science, History and Psychology, three different levels of analysis are proposed. Units of analysis, explanatory principles and conceptual tools are also offered for each of these levels.

One of the first issues to be addressed by any scientist is that of the relevance of his/her intellectual inquiry. In the case of the Historian of Psychology, who is also a Psychologist, it means that questions such as: What do we work on History of

Psychology for?, or, What can Psychology expect from the study of its History? must be answered. In this paper we attempt to respond to these questions as well as presenting a methodological approach to the study of this discipline.

A review of the literature on the issue of the function of History of Psychology offers an array of different opinions. Some authors (Henle, 1976; Sarton, 1960; Watson, 1962; Wertheimer, 1969) emphasize the descriptive and educational role this discipline can perform, as well as contributing to the development of an independent and critical spirit among students (Crutchfield & Krech, 1962; Raphelson, 1982; Woodward, 1980).

Others (Krantz, 1965; Watson, 1962) point out that the History of Psychology is a rich source of guidelines and suggestions for psychological research. However, all these functions, if considered in isolation, assign a mere ornamental or accessory role to History of Psychology in relation to Psychology.

Other authors defend the need to take into account a historical approach in order to place psychological ideas within the context of the specific socio-historical

conditions that determine the emergence of those ideas at that time (Danziger, 1984; Wertheimer, 1980; Robinson, 1982; Ross, 1969).

Finally, there are positions which emphasize that History is a means of placing the present as a tension between past and future so that the knowledge of History conditions current actions and therefore the future (Esper, 1964; Pinillos, 1962; Pongratz, 1967; Raphaelson, 1982; Watson, 1960).

In summary, the History of Psychology places and justifies psychological ideas within the framework of its socio-historical conditions. However, in addition to this, it offers approaches and methods that allow one to go deeper into the study of the human phenomenon from which Psychology might benefit. But in order for this role to be performed, it has to rely on a methodology which makes it feasible. First, a metatheoretical approach which makes Psychology commensurable with the Philosophy of Science and History is required, together with theoretical and methodological tools which allow for the transference of concepts from any of these disciplines for use in any other. The remainder of this paper will concentrate on our ideas on this issue.

Some questions have to be clarified before proceeding further. The History of Psychology is not a psychological, but rather a historical discipline. It belongs to the field of the histories of sciences. How can it then collaborate from outside Psychology in the process of building psychological knowledge?

Lakatos (1970) said that History and the Theory of Science have to go together, the former providing empirical evidence for the latter, which in turn means that the History of Science has to borrow conceptual tools from the Theory of Science. Transporting these ideas to the field of Psychology, we can state that the History of Psychology relates to Psychology through a general theory of Psychology - what Vygotski (1982) called "General Psychology", or, in a more contemporary term, a Philosophy of Psychology. Such a discipline should interrelate the knowledge offered by all the psychological disciplines not only among themselves, but in relation to the other social and natural sciences. In order to do so, General Psychology has to provide a general metatheoretical framework within which the History of Psychology, as we said before, has to provide empirical historical material to support it. That means that the theoretical tools of the History of Psychology have to be coherent with those of General Psychology and the psychological subdisciplines, though this does not mean that they have to be identical. Therefore, the History of Psychology cannot directly affect psychological research in a particular field, but it can do so indirectly through the Philosophy of Psychology. Namely, it performs a critical role in relation to Psychology. Our attempt here is to risk some conceptual tools in order to work on History of Psychology along these lines.

From our position, scientific knowledge offers systems of rules about the functioning of parts of the world which are historically conditioned. Following Hanson (1958) and Chalmers (1976) scientific data are built by the scientist. Construction of data is a result of a dialectical relationship between the theoretical

assumptions of the theory adopted and the phenomena as represented by the individual. Scientific knowledge is then a human construction, the result of structured human activity.

Chalmers (1976), following Althusser (1965) considers science as a practice, i.e. human labour which intentionally transforms raw matter into a manufactured product. In the case of Science, raw matter can be interpreted as previous systems of knowledge, while products are the new elaborations. Science, as any other practice, is related with and interdependent on the system of practices which constitute a particular society. Each practice fulfills a social function and is regulated by a set of formal or informal rules for the action of the individuals that practice that form of labour.

Social practices and their rules have evolved historically. As social needs and requirements have changed, so have scientific practices and ways of working in science have evolved. By this we do not mean a type of social reductionism with regard to the evolution of science. The idea we are trying to convey is that in a particular historical moment social conditions create certain conditions of possibilities for human action, i.e. they set a scenario for action but do not determine absolutely the type of actions human actors will perform. However there are ways of explaining the rationale of those actions: History, and the History of Science, are not random or arbitrary processes.

A particular Science is then the result of a practice; its subject-matter, its methodology and the knowledge it offers is contingent on the actions carried out by human individuals and groups in the historical conditions in which they have evolved.

The History of Science has then the possibility of offering a conceptual and methodological critique of that science, together with some guidelines for its further development.

From what has been said so far, it follows that the subject matter of History is human action and its results, issues which are not foreign to the study of Psychology. This means that both History and Psychology deal with the same subject of action - man. Therefore, the units of analysis and the explanatory principles both disciplines use have to be commensurable. Each will need the other as an auxiliary science to perform its role.

The goal of the History of Psychology, as for any History of Science, is to provide a theoretically-founded account of the development of psychological productions, i.e. ideas, theories, methods, techniques, etc.. To be able to attain this goal data sources have to be specified together with the methods with which to build and explain data. In order to do so, different levels of analysis, with their respective units of analysis and explanatory principles, have to be specified.

We distinguish three levels of analysis: theoretical, individual or biographical, and social. The theoretical level of analysis refers to the products of Psychology through time. But products by themselves cannot offer an explanation of their

production. This is the reason why the other two levels of analysis are needed. They can offer an explanation of the production processes. Let us elaborate briefly on some methodological ideas about these three levels of analysis.

The theoretical level of analysis

As has been said, this level refers to the products of psychological inquiry through time. These products usually take the form of written texts. Texts, by definition, are the result of a communicative action through language which transport meaning relative to the world. Meaning is then a way of representing the world to make it understandable.

Theoretical productions belong to different groups: some try to explain the functioning of the world (theories), others offer guidelines for scientific action (methodologies), while some others provide intervention techniques to control behavior (technologies).

Following Bakhtin (Wertsch, in press) the *unit of analysis* we consider is the *utterance*. An utterance is a unit of speech which transports meaning. It has a communicative, pragmatic character and it is always included in a chain of communication in which several voices interact. In any scientific text a sort of poliphony can be found, a dialogue among different voices. The voice of the writer together with utterances taken from other texts and transformed by the author (something that Bakhtin called ventriloquization). A text is then a type of dialogue in which the addressee also has to be taken into account; the latter has to carry out a dialogue with the text in order to understand it.

A historian of Psychology who approaches a text has to identify the different voices included if s/he wants to follow threads that have created the fabric, but, also, s/he has to try to reconstruct the theoretical system enclosed in the text.

Hübner (1983) proposes the use of what he calls a priori "precepts", which are systems of rules that guide the process of building scientific theories. Any scientific theory can then be reconstructed looking for two different sets of rules: a) the precepts, i.e. the explicit or tacit rules which have regulated the process of building the theory (rules that regulate the actions of the scientist); and b) the rules which shape the knowledge of how the part of nature studied by that particular discipline works (scientific theories). It has to be noted that concepts such as utterances and voices constitute the source of data, while precepts and rules are elaborations added by the historian in order to make sense of his/her analysis. The type of work carried out by the historian is then empirical, on the one hand, and theoretically and methodologically founded on the other. In the case of the History of Psychology there is a possible source of confusion, since Psychology deals with rules for the behavior of humans (as well as other organisms). The historian of Psychology has to be aware of not mixing up rules which try to comprehend the behavior of humans (as subject of Psychology) and rules which regulate the behavior of psychologists (as subjects of the History of Psychology).

However, a very interesting issue appears when the second question is taken into consideration. Both types of rules cannot be incompatible. As a matter of fact, we believe that this consideration can be a valuable contribution from the History of Psychology to the History of Science.

How can we explain the production of precepts and rules? As Davidov and Radzikhovski (1985) point out, an explanatory principle cannot be placed on the same level of analysis as that of the unit of analysis considered. This takes us away from the theoretical level of analysis and places us within the others.

The biographical or individual level of analysis

This level of analysis deals with the causes which have influenced individuals to produce the texts studied in the other level of analysis. The *unit of analysis* to be considered here are the *actions* carried out by the individuals. The concept of action stated here has a precise meaning, that which derives from Leont'ev's activity theory (Leont'ev, 1978). An activity is characterized by one motivation and each action has a conscious goal within the activity. One of the characteristics of actions is that they are mediated by cultural means. This means that an individual scientist's action is mediated in two senses: a) through conceptual and methodological tools acquired via formal academical training and research practice; and b) through individual motives and goals internalised from the macro and microcultural milieu. The unit of analysis at his level acts then as the *explanatory principle* for the theoretical level of analysis above examined. However, this individual level of analysis does not exhaust the explanation needed, it takes us to the social level, since it is within human groups where the conceptual tools and motives for action are generated.

The social level of analysis

Scientific activities are carried out within society and most of the time, inside research institutions. As we said before, a society can be characterized as an assemblage of practices (Althusser) or as a system-assemblage (Hübner). A Science emerges as result of a practice, and any practice responds to social needs, as considered in that particular moment of history, and is regulated by rules of behavior. These rules, however, are not only rules concerning the building of theoretical theories as mentioned above, there are also rules concerning the founding of research, publication, laws and regulations of institutions and academical promotion, etc. That is, a sort of infrastructure for the development of science, which has been the focus of attention for sociologists of knowledge, who have produced techniques to deal with these issues, from which the historian of science can benefit. Other phenomena such as distribution of roles, motives and goals in a particular microgroup of researchers are also relevant. The *unit of analysis* within this level is *practice* specified in systems of rules, and the *explanatory principle* is the social needs as considered by the group, either spontaneously or imposed from outside, in a particular historical moment. Finally, it has to be taken into account that this social level of analysis can be further divided into several

sublevels, corresponding to different aspects which are relevant to the aims pursued. For example, a research group is a subgroup within the scientific community that performs a certain practice, etc.

CONCLUSIONS

The above sketch aims at being non-reductionist, since one level of analysis cannot be reduced to the next - the idea of mediated action through the available tools prevents us from doing so -, rather it attempts to offer an open systems approach in which scientific production is the output of human action performed in an environment which sets conditions of possibility for these actions, not objective conditions which unavoidably gear us to a definite end. The future is open for action, but this is neither absolutely free nor inevitably conditioned.

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Die Fortsetzung der Forschungsrichtung der Wundt'schen Psychologie anhand des Archiv für die gesamte Psychologie (1903-1914)

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Die experimentelle Psychologie, wie wir wissen, ging aus dem deutschen Universitätsgefüge hervor. Dort machte die Vereinigung günstiger sozialer, institutioneller und wissenschaftlicher Faktoren die Entwicklung eines Laboratoriums und die Anerkennung ihres wissenschaftlichen Status möglich. Man kennt Wundt als Initiator dieser neuen Psychologie und diese Anerkennung basiert in der Fähigkeit die wichtigsten Schritte zum Aufbau dieser Entwicklung vollzogen zu haben. Das bedeutet, daß er der Psychologie ihren Namen gab, ein spezifisches Studienobjekt, einige Vorgehensmethoden, eine theoretische Interpretations- und Erklärungsbasis, einige Forschungsprobleme, eine gewisse institutionelle Infrastruktur und ein Kommunikationsorgan (Caparrós, 1980, 1984; Tortosa, 1989, unter anderen).

Das 1879 von Wundt in Leipzig gegründete Laboratorium wurde sehr bald zu einem Forschungszentrum und Vorbild der neuen Laboratorien, die Ende des 19. Jahrhunderts entstanden sind (Bringman und Ungere, 1980; Cattell, 1888; Meischner und Eschler, 1979; Peiró und Carpintero, 1978; Titchener, 1879; unter anderen).

Jedoch war die Wundt'sche Psychologie nicht die einzige deutsche Forschungsrichtung innerhalb der wachsenden wissenschaftlichen Psychologie. Auch veranlaßte sie nicht die sofortige Integration anderer philosophischer und physiologischer Haltungen, die auch ihren Beitrag leisteten, parallel zu den Arbeiten der Leipziger Schule. Es ist aber richtig, daß alle Richtungen zu irgendeinem Zeitpunkt die von Wundt und seinen Schülern verteidigte Psychologie als Bezugspunkt hatten, ob zur Kontrastation oder zur Angleichung.

Die Wundt'sche Psychologie kann also als Zentralpunkt der experimentellen Psychologie betrachtet werden. Mit Hilfe einer Analyse der Forschungsthemen und Autoren, die in diesem Model arbeiten, dann man einige klare Dimensionen des damaligen Interessengebietes der ersten wissenschaftlichen Psychologie entdecken und das fundamentelle Gerüst dieser ersten wissenschaftlichen Psychologie kennenlernen.

Eine der möglichen Formen, die Themen herauszufinden, die diese ersten Psychologen beschäftigten, ist eine Analyse ihrer Zeitschrift anzustellen.

Wenn wir berücksichtigen, daß die Allgemeinheit die wissenschaftlichen Errungenschaften mit Hilfe ihrer Veröffentlichungen aufnimmt, dann versteht sich, daß die spezialisierten Zeitschriften nach und nach zu spezifischen Kanälen der wissenschaftlichen Verbreitung geworden sind.

So behaupten die wissenschaftlichen Veröffentlichungen eine besondere Stellung als Kommunikationsorgane und Verbreiter der Information in einer wissenschaftlichen Gemeinschaft. Auf der einen Seite sind sie, laut Carpintero und Peiró (1980), ein Ausdruck der Errungenschaften und, auf der anderen Seite, ein Resultat der organisierenden Strukturen der Wissenschaft.

Die Artikel, die in den Zeitschriften erscheinen, sind nach Garvey (1979) ihre fundamentelle Einheit und somit, wie Price (1978) deutlich macht, ein Endprodukt der wissenschaftlichen Produktion. Die Analyse der Tendenzen, Autoren und Zitate macht es möglich, sich den organisatorischen, sozialen und institutionellen Aspekten zu nähern, sowie den konzeptuellen und theoretischen Aspekten, die in einer Disziplin vorherrschen, und ihrer Richtlinien und Methodik.

Wundt hatte das deutliche Bedürfnis die neue Wissenschaft zu verbreiten und stattete sein Laboratorium bald mit einer Zeitschrift aus, in der die dort realisierten Arbeiten veröffentlicht wurden. Diese Funktion hatten die von Wundt 1881 gegründeten "Philosophischen Studien" bis zu ihrer Endung 1903. Somit werden in ihr die ersten Jahre des Aufbaus und Tätigkeit des Leipziger Laboratoriums wiedergespiegelt. Später überlies Wundt die Fortsetzung dieser Veröffentlichungen in Händen von E. Meumann, die unter dem Namen "Archiv für die gesamte Psychologie" weitergeführt wurden.

In einer vorausgehenden Arbeit (Saiz, Saiz und Mülberger; 1990) analysierten wir bibliometrisch die Zeitschrift "Philosophische Studien", wobei die häufigst behandelten Themen und repräsentativsten Autoren der Wundt'schen Schule offensichtlich wurden. Die Absicht dieser Arbeit ist, das "Archiv für die gesamte Psychologie" von Anfang (1903) bis zum Beginn des 1. Weltkrieges zu analysieren; ein Zeitabschnitt, der genau mit der Meumann'schen Leitung der Zeitschrift übereinstimmt. Diese Analyse wird es uns ermöglichen, die Arbeitsrichtungen der deutschen Schüler Wundt's außerhalb der direkten Überwachung ihres Lehrers nachzuvollziehen. Die Fortsetzungsfunktion des "Archivs" macht Külpe in seinem Artikel zu Ehren des 80. Geburtstages Wundts, der mit dem zehnten Jubiläum der Zeitschrift übereinstimmt deutlich:

"...und so ist unsere Zeitschrift namentlich ein Publikationsorgan für diejenigen psychologischen Institute geworden, die sich als unmittelbare Sendlinge und Tochteranstalten des Leipziger Instituts betrachten durften." (Külpe, 1912; S.107).

Wilhelm Wundt beginnt die Leitung seiner "Philosophischen Studien" 1881, als das Leipziger Institut für experimentelle Psychologie schon seit 2 Jahren funktionierte. Die Absichten dieser Veröffentlichungen macht Wundt in seinem Schlußwort (1903) deutlich:

"Die Studien sind (...) im wesentlichen der Veröffentlichung der Arbeiten des Leipziger psychologischen Laboratoriums bestimmt gewesen." (Wundt, 1903, S. 795).

22 Jahre lang wurden sie herausgegeben und sammelten Arbeiten von insgesamt 121 Autoren und 325 Artikeln. Von diesen entsprechen 76.6% (249 Artikel) Veröffentlichungen von Wundt selbst, und Autoren, die mit dem Laboratorium verbunden waren (Schüler, Mitarbeiter und Assistenten von Wundt). Dies zeigt klar den endogamischen Charakter dieser Philosophischen Studien (Saiz, Saiz und Mülberger, 1990).

Die Veröffentlichung der "Philosophischen Studien" wurde 1903 eingestellt, nachdem sie insgesamt 18 Bände beinhaltete, zu denen man die beiden speziellen Ausgaben (Bände 19 und 20) zählen muß, die ihm von seinen Schülern am 16. August 1902 zu seinem 70. Geburtstag überreicht wurden.

Den Grund für die Aufgabe dieser Zeitschrift erklärt Wundt (1903) in seinem Schlußwort:

"Indem die Philosophische Studien ihr Erscheinen einstellen, geschieht dies daher nicht in der Absicht, damit dem Programm zu entsagen, dem sie bis dahin treu geblieben sind, sondern es geschieht vielmehr in der Überzeugung, dass es an der Zeit ist, das was hier in kleinerem Maßstabe und mit beschränkten Mitteln begonnen wurde, in vollrem Umfang und unter der Beteiligung zahlreicherer gleichgesinnter Mitarbeiter forzusetzen... Das "Archiv für die gesamte Psychologie", welches nunmehr unter der Redaktion von Prof. E. Meumann in Zürich in dem gleichen bewährten Verlag an ihre Stelle treten soll, wird es versuchen, einen größeren Kreis gleichgesinnter Mitarbeiter heranzuziehen. Es wird sich auf die Psychologie beschränken, diese aber in ihren verschiedenen Zweigen umfassen und sich namentlich, soweit dies bei dem heutigen Standpunkt der Wissenschaft möglich erscheint, neben der experimentellen auch auf die Völkerpsychologie erstrecken. Es wird endlich durch zusammenfassende Litteraturübersichten, Referate und Besprechungen größere neuere Werke aus dem Umkreis ihrer Gebiete eine möglichst vollständige Übersicht über Zustand und Fortschritte der Psychologie zu geben bemüht sein." (Wundt, 1903; S. 795).

Es klingt überzeugend, daß Wundt nach einer langen arbeitsreichen Zeitspanne, im hohen Alter die Leitung der Zeitschrift abgibt und Meumanns Archiv die Aufgabe überläßt, dich einem breiteren Sektor der Psychologie zu öffnen. So schreibt Meumann (1903) selbst zur Einleitung dieser neuen Zeitschrift:

“Als Dokument dieses wachsenden Bewußtseins der Gemeinsamkeit der Arbeit und der Gleichheit der Ziele will das Archiv für die gesamte Psychologie betrachtet sein. Es tritt nicht als eine neue Zeitschrift unter zahllosen andren auf den Plan, sondern als die erweiterte Fortsetzung der Philosophischen Studien, die lange Zeit als das alleinige Organ für die Veröffentlichung experimentell-psychologischer Arbeiten dastanden. Die “Studien” Wundts waren, entsprechend der Entwicklung der experimentellen Psychologie, anfangs ein Kampforgan, das der neuen psychologischen Methode die Wege bahnen sollte und gebahnt hat. Durch die Erweiterung ihres Programms zu einer allgemein psychologischen Zeitschrift möchten die Herausgeber begunden, daß sie die Zeit für gekommen erachten, um auf dem einstigen Kampfesboden zahlreiche Psychologen zu gemeinsamer Arbeit zu vereinigen. Zu dem Ruf nach Einheit und Verständigung gesellt sich so das lebhafteste Verlangen nach Konzentration der Forschung! Die Zusammenfassung der gesamten psychologischen Arbeit wird allmählich eine Lebensfrage der Psychologie.” (Meumann, 1903, S.7-8).

Wundt, jedoch, blieb mit dem “Archiv” eng verbunden. Einerseits durch seine Aufgabe als Mitherausgeber der Zeitschrift, und, andererseits durch den direkten Einfluß den er über Meumann ausübte. Wie Wirth (1920) feststellt, handelt es sich dabei um die einzige spezialisierte Zeitschrift die mit Wundt als Mitherausgeber zählte:

“...unsere Fachzeitschrift aber, der einzigen, die Wundt unter ihre Mitherausgeber zählen durfte (...). Auch blieb er selbst bis zuletzt unser Mitherausgeber, zumal Leipzig nach Meumanns ausdrücklichen Wunsch schon damals mehr Einfluß auf die Schriftleitung eingeräumt wurde.” (Wirth, 1920, W.1-2).

Trotz diesem Wunsch die Verantwortung abzugeben, hatte Wundt bald wieder das Bedürfnis eine eigene Zeitschrift herauszugeben, ein Publikationsorgan für die Arbeiten seines Laboratoriums. So entstehen 1905 die “Psychologischen Studien”, parallel zur Herausgabe des “Archivs”.

In seinem Vorwort schreibt Wundt, daß die “psychologischen Studien” praktisch eine Fortführung der “philosophischen Studien” sind:

“Das Programm dieser Richtung, das einer vorurteilslosen, die Hilfsmittel verwertenden, und dabei zugleich der Selbstständigkeit der psychologischen Aufgaben überall Rechnung tragenden Forschung, dieses Programm ist heute kein anderes wie ehemals.” ...“Nur die Ausführung desselben wird insofern eine veränderte sein, als diese Studien in Zukunft ausschließlich zur Publikation von Arbeiten aus dem psychologischen Institut zu Leipzig bestimmt sind,...” (Wundt, 1905; S.2).

Im Bezug zum neugegründeten Archiv rechtfertigt Wundt die Herausgabe seiner "psychologischen Studien" mit folgendem Argument:

Das "Archiv für die Gesamte Psychologie"

"...hat sich mehr und mehr in ein Archiv für reine und angewandte Psychologie umgewandelt, welches durch möglichst umfassende Sammelreferate über die verschiedenen Teile der Psychologie allen, die sich für die Psychologie interessieren, nützlich zu werden sucht,..." (Wundt, 1905; S.2).

"Im selben Maße macht sich aber zugleich das Bedürfnis geltend, der Pflege der experimentellen Psychologie in jenem rein theoretischen Interesse, dem die "Philosophischen Studien" zu dienen versucht hatten, wieder eine Stätte zu schaffen." (Wundt, 1905; S.3).

Nachdem 10 Bände dieser Zeitschrift mit insgesamt 100 Artikeln veröffentlicht wurden, von denen 5 Berichte und 5 kleine Mitteilungen von Wundt selbst erschienen sind, hält er den Moment für gekommen, die Herausgabe der Zeitschrift wieder einzustellen mit folgender Begründung:

"Beide Folgen" - "philosophische Studien" und "psychologische Studien" - "waren dazu bestimmt, die Arbeiten des von mir seit dem Jahre 1879 geleiteten Instituts für experimentelle Psychologie zur Veröffentlichung zu bringen. Mit dem Rücktritt von meinem Lehramt am 1. Oktober 1917 ist auch dieser Veröffentlichung ihr Ziel gesetzt." (Wundt, 1917; S. 571).

Wie Wirth (1920) im Archiv schreibt, veröffentlichte Wundt in seiner Zeitschrift ausschließlich Arbeiten des Leipziger Instituts, um dem Archiv nichts wegzunehmen. Dies stimmt mit den Angaben Wundts überein und könnte die geringe Beteiligung seinerseits und der Mitarbeiter des Leipziger Instituts bei den Veröffentlichungen des Archivs erklären.

Wenn wir uns auf die 292 Artikel des "Archivs" konzentrieren, innerhalb des Zeitabschnittes von 1903 bis 1914, dann kann man feststellen, daß die Zeitschrift sich wirklich anderen Autoren öffnete, die nicht direkt mit dem Leipziger Laboratorium in Verbindung standen. Im Gegensatz zu den "philosophischen Studien" deren Veröffentlichungen zu 76, 6% von Autoren aus dem Laboratorium stammten, sinkt um Archiv dieser Prozentsatz auf 14.9%.

Man kann auch eine größere Produktionsverteilung im "Archiv" beobachten, im Vergleich zu den "philosophischen Studien", in denen Wundt der produktivste Autor gewesen ist, -er veröffentlichte 53 Artikel (16.3% der Gesamtproduktion)- während der ihm folgende Autor mit 16 Veröffentlichungen auf 4.9% sinkt. In der Tafel der produktivsten Autoren kann man diese

Änderung der Veröffentlichungsrate im Archiv deutlich erkennen. Während in den "philosophischen Studien" alle produktiven Autoren mit dem Laboratorium verbunden waren, befinden sich im Archiv nur 5 Autoren (Meumann, Kiesow, Kirschmann, Wirth und Störing).

Die Analyse der behandelten Themen in beiden Zeitschriften macht auch differenzielle Aspekte deutlich. Die "philosophische Studien" zeigen eine größere Vorliebe für Themen wie Empfindung und Wahrnehmung (32.1%) und an zweiter Stelle die mit der Methodik verbundenen Aspekte, sowie Instrumente und Apparate (19.9%). Dies ist verständlich, wenn man bedenkt, daß zu diesem Zeitpunkt die Verteidigung der Absichten und Methodik der wachsenden experimentellen Psychologie von großer Bedeutung war. In der Analyse des Archivs wird jedoch eine klare Minderung des Interesses für diese beiden Themen auf 22.3% und 5.5% deutlich, obwohl sie immer noch herausragende Positionen einnehmen. Abgesehen von dieser Abnahme, kann man eine Zunahme anderer Themen feststellen, die nur wenig in der ersten Periode ("philosophische Studien") angesprochen wurden. So erscheinen plötzlich in hohem Maße Arbeiten über das Denken, Gefühle und Affektivität.

Daraus läßt sich schließen, daß in der ersten Phase, der "philosophischen Studien", vorwiegend wissenschaftliche Forschungsarbeiten oder theoretische Überlegungen auftauchen, während in der zweiten Phase ("Archiv") Arbeiten auch aus den angewandten Gebieten der Psychologie zu erscheinen beginnen (Erziehungs- und Kinderpsychologie, justische und forensische Psychologie, Arbeitspsychologie...).

1. Tafel: LISTE DER PRODUKTIVSTEN AUTOREN DER ZEITSCHRIFT "ARCHIV FÜR DIE GESAMTE PSYCHOLOGIE"

AUTOR	ANZAHL DER WERKE	PRODUKTIVITÄTS- INDEX
URBAN, F.M.	11	3.7
MEUMANN, E.	9	3.0
BENUSSI, V.	8	2.7
KIESOW, F.	7	2.4
KIRSCHMANN, A.	5	1.7
LIPPS, T.H.	5	1.7
MESSER, A.	5	1.7
PONZO, M.	5	1.7
WIRTH, W.	5	1.7
ANSCHÜTZ, G.	4	1.3
RIGNANO, E.	4	1.3
STÖRRING, G.	4	1.3
GESAMTPRODUKTION	72	24.3%

Die angefertigte Analyse des Archivs bestätigt insgesamt, daß sich die Vorsätze der Zeitschrift erfüllt haben, und daß die zweite Periode der Wundtschen Psychologie sich im Archiv anderen Themen und Autoren öffnete.

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Die Anwesenheit von B. F. Skinner in der spanischen Psychologie: Eine Analyse seines Einflusses über die Zeitschrift *Revista de Psicología General y Aplicada*, Kommunikationsorgan der spanischen Gesellschaft für Psychologie

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Die Analyse der Einführung Skinners Werke in Spanien kann nicht ohne einen kurzen Kommentar über die sozial-institutionellen Gegebenheiten der spanischen Psychologie vollzogen werden. Die späte Aufnahme seiner Werke Ende der sechziger Jahre, während inzwischen in den Vereinigten Staaten schon andere Richtungen aufkamen, ist eher eine Konsequenz der Situation der Psychologie in unserem Land, als durch den Wert Skinners Werke selbst bestimmt.

Die Psychologie in Spanien vor dem Bürgerkrieg (1936-1939), nach den wichtigen Lebenswegen der großen Meister, wie Turró (Vater der experimentellen katalanischen Psychologie) oder Ramón y Cajal (mit seiner außerordentlichen Entdeckung der Nervenzelle), hatte ein gutes Entwicklungsniveau erreicht, wichtige ausländische Werke hat man ins Spanische übersetzt, eine Arbeit die von "Institución Libre de Enseñanza" und ihre Anhänger in Bewegung gesetzt wurde; einige Autoren mit Namen haben in Spanien Vorträge gehalten, wie z.B. Köhler, Michotte, Pieron und Pavlov; verschiedene internationale Kongreße wurden organisiert und der XI Internationale Kongreß der internationalen Vereinigung der wissenschaftlichen Psychologie sollte 1936 in Madrid stattfinden, die physiologische Schule aus Barcelona von Pi i Sunyer und Miras Psychotechnische Schule von Barcelona wurden auf internationaler Ebene anerkannt; moderne psychologische Ansichten waren eingedrungen, Mira hat, zum Beispiel, 1921 das Buch von Watson *Psychology from the standpoint of a behaviorist* von 1919 (Mira, 1921a) kommentiert und einen, in zwei Abschnitte geteilten Artikel über die neue behavioristische Psychologie von Watson geschrieben (Mira, 1921b).

Als 1939 die Regierung Francos aufkam, wurde diese sich öffnende Tendenz der spanischen Psychologie abgebrochen. Ein hohes Gremium für wissenschaftliche

Forschungen (C.S.I.C.) wurde gegründet "... deren Gründungsweise man durch die Programmklärung erkennen kann, in der eines der zu erreichenden Ziele lautet 'die Restaurierung der klassischen und christianischen Einheit der Wissenschaft'" (Campos und Aguado, 1977). Die Tatsache, daß die Leitung dieses Gremiums dem Pater Arbareda anvertraut wurde, gibt uns den ersten Eindruck der neuen Einstellung der spanischen Psychologie, die in vielen Fällen von Religiösen betrieben wurde. Auch endete der Krieg mit der Infrastruktur vieler Institutionen und es kam zum Exil vieler wichtiger Wissenschaftler, unter ihnen auch Psychologen (Carpintero, 1984; Encinas und Rosa, 1990).

"Der Krieg entscheidet (...) wer geht und wer bleibt, welche Ideen weitergegeben werden dürfen und welche nicht. 1939, in Spanien, das Schicksal der offiziellen und akademischen Psychologie wurden wieder an das alte eskolastische und traditionelle Gedankengebäude gebunden (...). Der Krieg war, jedenfalls, nichts gegenüber neutral (...) und auch nicht der Psychologie gegenüber". (Carpintero, 1980 S.51).

Diese Situation zusammen mit der spanischen Isolierung verursachte, daß sich die Zeitperiode der Rekonstruktion der spanischen Psychologie ab diesem Bruch in einen allzulangsamem Prozeß verwandelte.

Mit der Entstehung des Departaments der experimentellen Psychologie des C.S.I.C., zusammen mit der Figur von Germain und seinen Mitarbeitern beginnt eine Verbindung zwischen Vergangenheit und Zukunft deutlich zu werden, aber vor allem in dem Bereich der angewandten Psychologie. Durch sie wird die Zeitschrift *Revista de Psicología General y Aplicada* (Zeitschrift der allgemeinen und angewandten Psychologie) gegründet, die sowohl spanische, sowie ausländische Arbeiten aufnimmt, und sich so in das Forum der spanischen Psychologen verwandelt. Die Festigung dieser langsamen Institutionalisierung beginnt in den fünfziger Jahren einige ihrer Früchte zu tragen, als 1952 die spanische Gesellschaft für Psychologie gegründet wird und 1954 eine psychotechnische Schule in Madrid; obwohl erst 1968 Psychologieabteilungen in den spanischen Universitäten entstanden. Die Aufnahme der Psychologie in der Universität und die Bildung von Arbeitsgruppen, sind Ereignisse die ohne Zweifel der Entwicklung der Psychologie und der progressiven psychologischen Forschung in Spanien geholfen haben. Der sozial-institutionelle Umkreis fängt an mehr die Aspekte, die in der experimentellen Psychologie in anderen Ländern entwickelt werden aufzunehmen, und es werden ausländische Werke dieser Richtung übersetzt, jedoch mit einer großen zeitlichen Verspätung.

Obwohl es schwierig ist, den genauen Anfang der Einführung von Skinner in Spanien festzustellen, können wir davon ausgehen, daß es Ende der 60iger Jahre geschah, als ein Kreis von Personen aus der barceloneser Psychologie Kontakt mit den Konzepten der experimentellen Verhaltensanalyse aufnimmt.

Die Figuren wie José Fernández de Castro, der 1968 in der barceloneser Universität erstmals eine Doktorarbeit aus einer operanten Perspektive präsentierte ("La aportación de B.F. Skinner al origen y primer desarrollo de la enseñanza

programada (1953-1963)“) und Ramón Bayés der die Übersetzung des ersten Werkes von Skinner in Spanien möglich machte (*Walden II*, Fontanella, 1968), können als Einführer des Denkens von Skinner betrachtet werden. Auch ist es angemessen, Pere Julià als einen der Vorkämpfer herauszuheben, der sich unter Stanley Sapon in der Rochester Universität promovierte, bei einem Spezialisten in verbalem skinnerischen Verhalten. Im Jahre 1971 errichtete Julià wahrscheinlich das erste operante Tierlaboratorium in Spanien in der Universität Autònoma de Barcelona (zur Erweiterung dieser Daten siehe Bayés, 1974; Bayés und Garau, 1982; Cruz, 1984; Zaiter, 1977).

Der Aufenthalt von Stanley Sapon 1970 in Barcelona und seine Konferenzen in der Universität Autònoma de Barcelona über experimentelle Verhaltensanalyse, förderten das Interesse für diese Forschungsrichtung (Bayés, 1974). Zwischen 1970 und 1975 entstehen in Spanien einige Arbeitsgruppen im Gebiet der operanten Konditionierung und einige Seminare und Kurse wurden organisiert. Aber nach unserem Kriterium kann man das Jahr 1975 als das entscheidende Jahr für die Verbreitung des Skinnerschen Modells in Spanien festhalten: in Palma de Mallorca findet der IV Kongreß der Gesellschaft für Verhaltenstherapie und Analyse statt; in Madrid findet die erste Sitzung über Lernen und Verhaltensmodifikation im Erziehungsbereich statt; die Zeitschrift *Análisis y Modificación de Conducta* wird erstmals herausgegeben, die, obwohl sie sich nicht exklusiv dem skinnerischen Modell widmet, Arbeiten in dieser Richtung beinhaltet, und einige Werke Skinners werden auf spanisch veröffentlicht, *Cumulative Record*, *The Behavior of Organisms* und *About Behaviorism* (bemerkenswert, daß dieses letzte Werk nur mit einem Jahr Verspätung herausgegeben wird, im Gegensatz zu anderen wie *The Behavior of Organisms* das 37 Jahre später erst übersetzt wurde, oder „Walden II“ mit 20 Jahren Verspätung oder *Science and Human Behavior* mit 16). Ab 1975 beginnt eine progressive Festigung des skinnerischen Modells.

Eine der möglichen Formen sich der Entwicklung der Werke Skinners in der spanischen Psychologie zu nähern ist mit Hilfe der Analyse der Zitate und Artikel die in der Zeitschrift *Revista de Psicología General y Aplicada* erschienen, welche von 1946 bis 1980 eine der wichtigsten spanischen Spezialzeitschriften war. Abgesehen davon, daß sie das Kommunikationsorgan der spanischen Gesellschaft für Psychologie bildet, und, ohne Zweifel, wie wir schon sagten, ein klares Forum der spanischen Psychologen der Epoche waren. Die Veröffentlichung der Zeitschrift, wie ihr Direktor Germain angibt, entsteht mit dem klaren Ziel die Psychologie in unserem Land zu unterstützen:

“Ich glaube es ist angebracht (...) eine neue psychologische Veröffentlichung in unserem Land unter dem Kriterium stehen zu lassen, das Theoretische mit dem Praktischen zu verbinden, und das Allgemeine mit der psychologischen Anwendung (...). Die Zusammenfassung von Arbeiten aus verschiedenen Gebieten der Psychologie in einem Veröffentlichungsorgan (...) wird zur Folge haben, daß man viele der Forscher kennt und schätzt (...).“ Die Aufgabe die sich die Zeitschrift *Revista de Psicología General y Aplicada* stellt, ist nicht nur die Zusammenarbeit zu stimulieren und die

Veröffentlichung psychologischer Arbeiten zu vereinfachen, sondern auch einen fruchtbaren Kontakt zwischen allen herzustellen, die sich der Psychologie widmen" (Germain, 1946, S.7-8).

Seit ihres Beginns versucht die Zeitschrift *Revista de Psicología General y Aplicada* sowohl die Aspekte der experimentellen Psychologie, als auch der angewandten Psychologie zu umfassen, was dazu führte, sie für einen guten Ausgangspunkt für unserer Analyse des Einflusses Skinners in unserem Land zu halten, da er in verschiedenen Gebieten aufgenommen und zitiert werden könnte (im experimentellen, klinischen und erzieherischen Bereich). Außerdem, da die Zeitschrift mit einer weiten Kommentarsektion und vielen Artikeln zählt, sowohl von spanischen als auch von ausländischen Autoren, kann man feststellen, ab wann es Kommentare über die Werke Skinners gab.

Unsere Analyse wurde über die gesamte Zeitschrift durchgeführt, von ihrer Gründung 1946 bis 1990. Wir haben keine spezifische Literaturangabe oder Kommentar eines seiner Bücher oder Artikeln vor 1967 finden können, obwohl 1966 in einem Kommentar des Werkes von Marc Richelle *Le condicionament operant* Skinner als Einführer dieser Terminologie genannt wird und als Urheber der programmierten Fortbildung.

Die erste Arbeit, die direkt mit der operanten Konditionierung verbunden ist, erscheint 1967 mit dem Titel: "Die Belohnung und Strafe in der aktuellen Psychologie" von Ruben Ardila. Sie beinhaltet die zwei ersten Zitate von Skinner, genauer gesagt von *The Behavior of Organisms* (1938) und *Science and Human Behavior* (1953). Jedoch, wie wir wissen ist Ruben Ardila ein kastellanisch schreibender Autor, aber kein spanischer Psychologe. Das erste Zitat eines spanischen Autors erfolgt 1969 anhand von J.L. Pinillos in seiner Arbeit "Sprache, Individuum und Gesellschaft", in der er *Verbal Behavior* zitiert. Andererseits, in der Abteilung für Buchkommentare und Artikel, erscheint keine Literaturangabe von Büchern von Skinner bis 1970; es handelt sich konkret um einen Kommentar über die kastellanische Übersetzung von *Science and Human Behavior*.

In der Analyse aller Zitate die zwischen 1946 und 1990 erscheinen kann man beobachten, daß sich Skinner zwischen den 25 Autoren deren Werke am häufigsten in den Literaturangaben befindet, obwohl er reichlichen Abstand zu H.J. Eysenck, J. Piaget und M. Yela wahr, die die ersten Positionen einnehmen. Skinner wird 84 Mal zitiert innerhalb von 40 Artikeln. Die Verteilung der Zitate der verschiedenen Jahre kann man in der Tafel und in der Grafik im Anhang verfolgen, die zur Verdeutlichung in 5 Jahresabschnitte aufgeteilt worden ist. Diese Daten machen deutlich wie nach einer langen Zeitspanne ohne jedes Zitat (erinnere man sich daran, daß das erste 1967 erscheint) erfolgt eine progressive Zunahme die ihr Maximum in den 5 Jahren zwischen 1980 und 1984 erreicht (40 Zitate, 47% der Gesamtzitate). Ab hier wird eine Abnahme deutlich, die man in den kommenden Jahren bestätigen sollte.

Die meist zitierten Werke Skinners sind *Science and Human Behavior* (18 Zitate, 21.4%), *The Behavior of Organisms* (12 Zitate, 14.3%) und *Verbal Behavior* (8 Zitate, 9.5%).

Obwohl man die Anwesenheit Skinners in der Zeitschrift *Revista de Psicología General y Aplicada* feststellen kann, ist diese nicht allzu hoch, was an 2 Faktoren liegen kann: Als ersten und vielleicht wichtigste Faktor ist die Tatsache, daß Skinner erst so spät in Spanien aufgenommen wurde, und, als er ankam, hatte sich das skinnerische Model schon weiterentwickelt und seine Gegenwart überlappt sich daher mit der Ankunft anderer Autoren im Rahmen der Entwicklung und Anwendung des operanten Models. Der zweite Faktor beinhaltet, daß kurz nach seiner wirklichen Aufnahme, eine spezialisierte Zeitschrift (*Análisis y Modificación de Conducta*) herausgegeben wird, die für speziell diesen professionellen Sektor, der an diesem Thema interessiert ist, bestimmt ist.

Zum Abschluß können wir sagen, daß wenn wir die Zeitschrift *Revista de Psicología General y Aplicada* als eine der größten Vertreter der spanischen Psychologie erklären, wird durch die Analyse dieser Zeitschrift die langsame und späte Asmiliation des Werkes von Skinner in Spanien deutlich und, seine Aufnahme in einigen spezieifischen Sektore (er wird nur von 5.3% der gesamten Autoren, die in dieser Zeitschrift veröffentlichen, zitiert), was mit der Offenkundigkeit aus anderen Medien, die wir am Anfang dieser Arbeit angesprochen haben, übereinstimmt und erweitert.

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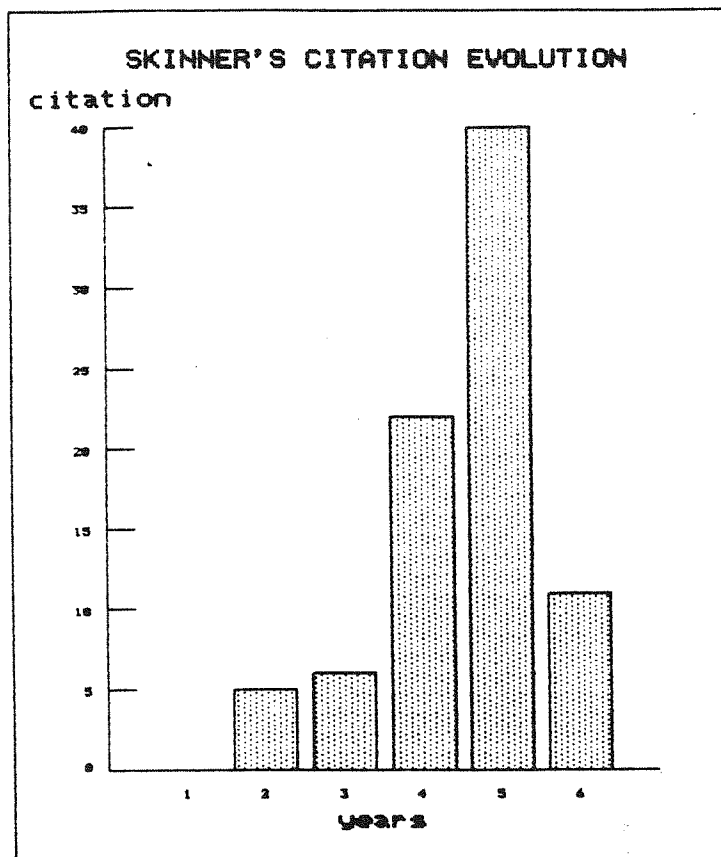


TABLE : SKINNER'S CITATION EVOLUTION .

YEARS	No. CITATIONS
1946-1964	0
1965-1969	5
1970-1974	6
1975-1979	22
1980-1984	40
1985-1990	11
TOTAL CITATION	84

Reaching understanding in the history of the social sciences

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ABSTRACT

This paper discusses how we should proceed in attempting to conceptualize a field of inquiry called the history of the social sciences. Contemporary theoretical developments in the social sciences, particularly those surrounding the debates on modernism and post-modernism, have drawn attention to the way in which we interpret social science practice. This essay suggests that how we construct our historical sensibility depends in part on historiographical issues emerging from current social and political theory. The paper endorses the view that at the level of method the historian of the social sciences considers adopting the position of the ironist or reflexive methodologist while remaining sensitive to the importance of the role of social and political theory and the process of modernisation and modernism in the construction of our historical sensibility. The essay recommends certain routes current history of the social sciences may travel in order to provide an objective account of social science practice

PREFACE

Studying the history of the social sciences raises theoretical and methodological questions on what appropriate interpretative schemes we need to adopt in order to understand the discontinuities, changes in intellectual and institutional practice. Are there suitable methodological idioms that we can adopt to chart changes in the practice of the social scientist? What methodological innovations are needed in order to investigate the style and content, the institutional and intellectual perspectives cultivated in educational practice? Is it the case that as historians of the social sciences, interrupting, intervening in the ongoing practice of the academy, we need to confront current political issues on the organization of this practice?

Our first suggestion is that human understanding is in part an outcome of talking and writing within contextually contingent cultural practices. We learn to speak and write within changing constellations of expectations, aspirations, which limit and determine our historical sensibility. In order to study the history of social science practice **we therefore must be aware of the historical sensibility we bring to that inquiry.** We are led to reflect on the contingent and fragile nature of our own and others interpretative schemes.

The reflexive historian of intellectual and institutional practice is in part the ironist who has radical and continuing doubts over the conceptual vocabulaires used to justify those schemes of practice. The ironist holds the view that nothing has an intrinsic nature, an essence, so that the terms truth, science, rational, in the vocabulary of justification of the day are merely language games of one's own time. "The ironist spends her time worrying about the possibility that she has been initiated into the wrong tribe, taught to play the wrong language game... But she cannot give a criterion of wrongness and so uses terms like perspective, conceptual framework, historical epoch, redescription, vocabulary and irony" (R. Rorty, *Contingency, Irony and Solidarity*). The reflexive historian of social science practice is in an important sense an ironist who attempts to identify the contradictions, the multi-layered discourse that typifies social science inquiry - a world of interpretative and institutional multiplicity.

The essay endorses the view that at the **level of method** the historian of the social sciences considers adopting the position of the ironist or the reflexive methodologist. However, it is also the case in writing a history of political science as a social science we can not escape from social and political reflections on current circumstances surrounding the practice. Historical analysis is a process of selection and emphasis, containing a perspective of importance (A. N. Whitehead, *Adventures of Ideas*), through which we argue have a care here is something that matters to me. Therefore it would be inappropriate to deny a role to social or political theory, through which we construct our sense of historical sensibility, our understanding of the limitations and determinations of the interpretative schemes we employ. Indeed contemporary social theory has drawn attention to the way we write about the history of the social sciences and the interpretative schemes we embrace. **Consequently we need to take account of the wider limitations and determinations of knowledge in relation to the social or political theory we adopt,** especially theory that attends to issues on the nature of the state and the rise of a professional and industrial society.

The core of the discussion concerns the issue of what is entailed in conceptualizing a field of inquiry called the history of the social sciences. This essay considers how in the light of radical changes in the current intellectual climate surrounding the social sciences we can form a historical sensibility of social science practice. What clear objects of inquiry are there for the investigator?

History of the social sciences

How then should we proceed in attempting to conceptualize a field of inquiry called the history of the social sciences? What style of approach should we adopt? How do we go about writing a history of the social sciences that takes sufficient account of

internal intellectual and institutional changes but which also endeavours to trace these changes in terms of broader alterations in social economic structure?

It should come as no surprise to anyone conversant with recent studies in the history of the social sciences from the standpoint of practitioners as well as historians, sociologists and philosophers of that practice, that the issue of re-formulating the basic conceptual apparatus of scientific and social scientific inquiry is firmly on this agenda for the 1990s. Current debates on post-modernism, post-empiricism, post-fordism, etc. reflect the confusion and multiplicity of interpretative procedures being practised within disciplines in the social sciences. The history of the Social Sciences in America is certainly recognised as a discrete field of inquiry (*Journal of the History of Behavioural Sciences*, 1965) and in Britain it is slowly emerging as an autonomous area for research (*History of the Human Sciences Journal*, 1988) though it must be admitted such moves in America and Britain to professionalize the field of inquiry seem to have been colonized by interests in the history of psychology rather than political science, sociology or economics. Current interest in the history of the social sciences of course does not just stem from attempts to institutionalize its own practice. It is our view that the history of the social sciences is a very broad field of inquiry encompassing both practitioners accounts within the disciplines as well as philosophers, historians, sociologists reflections on interpretative strategies employed within the social sciences. The history of the social sciences we argue is an area of study which includes accounts of the 'present' state of the disciplines and potential future directions they should take as well as detailed historical accounts of the changing nature of social science practice. Consequently any history of the social sciences is bound up with current practice but is distinctive in its attempts to historically ground this practice through intellectual and institutional analysis.

Historical analysis is a process of 'selection and emphasis', containing a 'perspective of importance' (Whitehead), through which we constantly argue 'have a care, here is something that matters to me'. Consequently, it would be inappropriate to deny a role to social theory in current practice of the history of social sciences or ignore the function of polemic or narrative in those accounts. The history of the social sciences is written both by practitioners and historians or philosophers who reflect on that practice which makes it a rich and somewhat eclectic panorama of interpretations on the historical and social constructions of knowledge. Ashmore (Sociology of Scientific Knowledge), McCloskey (Economics), Giddens and Mulkay (Social Theory), Platt and Bulmer (Sociological Methods), Gunnell (Historiography of Political Science), Habermas, Bernstein, Rorty, Gadamer, Thompson (Philosophy) all confront the problems of employing static historical accounts of naturalistic models (species of empiricism and positivism) in the practice of theoretical inquiry in the social sciences. Giddens, Gunnell, Bernstein, Thompson, have pointed to: the 'rapprochements' between Anglo-Saxon philosophy and Continental traditions of thought; the current accentuated introspectiveness within the social sciences which has also involved an increase in historical reflection; the popularity of hermeneutics and other post-

modernist philosophical approaches that has legitimised history and narrative as a mode of critique; the reflective and eclectic atmosphere of the post-behavioural era; the idea of the rediscovery of hermeneutical dimensions in the construction of scientific knowledge (J. G. Gunnell, *The Historiography of American Political Science*).

These contemporary theoretical developments within the social sciences have drawn attention to the way in which we write about the history of the social sciences and the nature of interpretation in social science practice. Nietzsche, Freud, Marx, Weber were the 'Front Generation' of modernists who argued for a reformulation of our basic conceptual apparatus in understanding the social world and a reconstruction of the forms of measurement necessary in a modern social science.

Subsequent accounts as diverse as those contained in the writings of Mannheim, Wittgenstein, Collingwood, Marcuse, similarly reflect a general modernist concern with the social and historical contingency of the production of knowledge. Although attention must be given to often wretched current comments on post-behaviouralism, post-empiricism, post-marxism, post-fordism and the like, one of the issues for current practice in the history of the social sciences is simply how it is possible to capture the nature of social science practice given reflections over the last two decades on the need to reformulate our basic *historical sense* of understanding the composition of intellectual practice?

How we construct and develop our historical sensibility, depends in part on historiographical issues and problems upon the particular social/political theory we favour in assessing the constellations or patterns of the historical and social production of knowledge. The kinds of objects that we pick out as analysts and participants in the social world are grounded and articulated in cultural, institutional and economic settings which need to be taken into consideration. In the social sciences we do not deal with objects directly given to us nor with representations which are the product of a transparent disengaged subject. The object of inquiry for the history of the social sciences is generally speaking the discourse of the academy. Following Gadamer and Ricoeur, we are linguistic beings who inherit already established linguistic interpretations of the world which are constructed in particular institutional and social settings. The discourse of the academy spans the world of the speaker and the text and represents a **form of practice** - a cognitive and social process within which the production of knowledge takes place. The discourse rather than the language of the academy is consequently considered as practice, a practice which includes the intellectual production of texts, the social and political context, and the social science communities themselves. **The historicity of the discourses of the social science communities is a clear object of inquiry for the history of the social sciences.**

Kuhn argues in *The Structure of Scientific Revolutions* that what is distinctive about natural science knowledge production is the existence of a research/scientific community governed by a paradigm containing a well established practice of normal science and an equally well established training process for new entrants into the

community. Kuhn is concerned to convey the view that the production of scientific knowledge is a form of practice, a practice which can be understood conventionally as an intellectual/cognitive product and more radically as a social product. Kuhn has stated in *Reflections on Receiving the Bernal Award*, that the 'Structure' is sociological only in so far as it 'emphasizes the existence of scientific communities, insists that they be viewed as the producers of special product, scientific knowledge, and suggests that the nature of the product can be understood in terms of what is special in the training and values of these groups'.

While not wishing to suggest that the history of the social sciences adopt a Kuhnian historiography, Kuhn is relevant in this discussion on historiographical issues since he gives support to the view that an any history of knowledge production need not be exclusively cognitive or singularly contextual. Kuhn suggests that in order to understand the production of scientific knowledge we attempt to recover the training of educational practices and values of the particular academic community. Translating these views for our purposes, these arguments give support to the viewpoint advanced here that the history of the social sciences deals with a form of practice, a practice that takes place within a particular social science community, involving the world of the speaker and the text, and often typified by principal educators in a particular discipline or field of inquiry.

These historiographical assumptions shadow the recent work of J. G. Gunnell on *The Historiography of American Political Science* who suggests approaching the history of political science in terms of the evolution of discursive practices, emphasizing the internal development of arguments within the discipline. Gunnell expertly argues that the history of political science needs to develop a more adequate historical sense, which focuses on excavating the details of the structure and content of the discipline and its actual context, while remaining alert to the excesses of overarching contextualist accounts that are imposed rather than discovered within the internal dynamics of the discourse of the academy.

In conclusion, reaching understanding in the social sciences in the 1990s depends in part on a critical awareness of what constitutes social science practice. Social science practice is largely contained within what we would call an Academy of Social Scientists, a somewhat loose or flexible organisation of individuals and groups concerned to develop intellectual fields of inquiry in a changing socio-economic environment. It is the academy as a community of discourse and action which is the prime object of investigation for historians of the social sciences. The historian interrupts and intervenes in that practice while attempting to develop adequate interpretative strategies which aim to be critical and informative of the ongoing practice of social science. The objects of inquiry are rooted in both intellectual production and institutional or organisational contexts and attention is directed to individual social science educators and collective research networks giving authority for an intellectual field of inquiry.

Reaching understanding in the social sciences is often a case of identifying which individuals or groups are attempting to gain authority in a particular field or what constellation of social, economic and political circumstances favour and support one field of inquiry over another. Social science practice is composed of intellectual products (texts, articles, addresses, lectures) and institutional contexts (professional groups, foundations, university administration, industrial and governmental influences). Consequently any historical analysis of the social sciences seeks to outline the connections between intellectual developments and institutional settings. The history of the social sciences has a potential to develop a critical awareness of what has in the past constituted social science practice, and so inform us on present and future directions in various fields of inquiry.

The history of the social sciences suggests that we identify different levels of generality in the process of inquiry, in order to take account of various forms of practice, the theoretical and the actual everyday moments of social engagement in an insitutional setting. In sum,

1. There is a need in the current climate of intellectual change for a critical analysis of what constitutes social science practice.
2. There are clear objects of inquiry for the history of the social sciences, the academy of social science. In particular attention is given to the historicity of the discourse of the academy and the changing idioms adopted by practitioners.
3. Social science practice is to be understood as a practice of theory and action which takes place in a variety of institutional settings in particular periods of social and economic change.
4. Consequently, a history of the social sciences must operate at the levels of:
 - a) internal history examining the changing pattern of intellectual thought and institutional context.
 - b) external history examining the relations between the practice of the academy and the issues of industrialization, professionalization, and the state in the 19th and 20th centuries.
 - c) interpretative multiplicity, a form of methodological pluralism where the discourse of the academy is examined in terms of its analytical and rhetorical forms of expression.
 - d) social and political reflection on the current intellectual and institutional practices of the social sciences.

Any history of the social sciences has to encounter the successes and failures within social science disciplines and embrace both rhetoric and analysis constitutive of its multi-layered practice. Claims for objectivity in the history of the social sciences are grounded in the engagement of a historiographical attitude, a historical sensibility, which celebrates interpretative multiplicity as a resource for assessing the historical development of social science practice. In order to catch the diverse transformations and continuities in the academy of social science different approaches, intellectual and

oral histories, biographical and autobiographical accounts, institutional analyses, textural examinations, are taken into consideration. Consequently the historian of the social sciences is confronted with different histories, divergent interpretations, which must be identified and assessed according to the historiographical disposition adopted. Furthermore, a history of the social sciences which does encompass the world of the speaker and the text, rhetoric and analysis should perhaps consider introducing alongside the now familiar world of data collection, a style of analysis that is sensitive to different forms of social science practice.

An awareness of what constitutes educational practice in the social sciences can not avoid attending to general issues on connections between industrialisation, bureaucratic organisation and the role of the state in relation to that practice. Issues on the historical process of modernity and the nature of social science education within that process are inescapable, especially in relation to both affirmative and critical attitudes adopted by practitioners on the interpretation of the process of modernity and modernisation in the 20th century.

Recent intellectual impulses in the social sciences have come from a tradition of critical modernism, a critical modernism that questions the enlightenment idea of scientific rationality, unlinear progress and change. Nietzsche, Freud, Marx interrogate the very idea and belief in progress, order, rationality which forms the kernel of the enlightenment project which is further advanced by liberal political and social theory. In modernism we can initially identify two competing vocabulaires, the liberal modernist and the critical modernist discourse. It is also the case however that this 'front generation' of critical modernism are used as intellectual resources for critical post modernism (Foucault, Derrida, Baudrillard).

The theoretical positions within the rubric 'post-modern' reflect new positions emerging from the rubric of modernism. Whereas the term 'modern' expresses a transition from the old to the new, so post modern reflects a developing out or reaction against of (other) modern positions. Critical Post-Modernism and Conservative Antimodernism are two such positions. A third stance can also be identified in the attitude of 'aloof disengagement' which is predominant in both of the aforementioned positions and leads to a more accentuated dehistoricisation.

These current debates on modernism and post-modernism are important contexts for appraising the historical sensibility we bring to understanding the composition of current intellectual practice in the social sciences. In critically assessing the rubrics of modernism and postmodernism we are in effect evaluating particular interpretative schemes which direct us into different conceptual vocabulaires for reaching understanding in the history of the social sciences.

'Human Nature' as an Enlightenment category

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ABSTRACT

I am concerned to find ways to characterise the study of the human Subject in the eighteenth century while avoiding anachronism. The paper suggests that the Enlightenment interest in what we would now identify as the subject matter of the human sciences was covered by reference to 'human nature'. The following remarks address the term's usage and three of the major questions that arise immediately: the relation between the term and 'the science of Man'; the term and the signification of gender; and the ambiguous meaning of 'nature'.

It is now widely agreed that modern disciplinary categories, such as psychology, anthropology, or linguistics, are not in any straightforward sense historically meaningful categories before the nineteenth century. What we would identify as psychological or linguistic beliefs existed before then, of course, as they have in most cultures, but historians seek interpretations of belief focussed on the historical actors' own categories and contexts. Elsewhere, I have argued that this is fundamental to the practice of history, and I criticised the practice of projecting modern subjects back into the past (Smith, R. 1988).

This paper attempts more constructive work, suggesting that the subject of human nature should be central to understanding what, in preliminary and expendable terms, we might call the human sciences in the eighteenth century. This is neither an original nor a surprising argument, though I do think the degree to which it enables us to discuss the eighteenth century without anachronistic reference to psychology, sociology, and so forth, has not been appreciated.

Reference to human nature was a commonplace of English- language Enlightenment writing. Whether or not there were equivalents in other European languages is an open question; certainly, reference to *nature humaine* or to *Natur des Menschen* was common enough, but whether these phrases carried exactly the same connotations remains to be assessed. Significantly, the English- language usage provoked little comment: human nature appeared to be one of those taken-for-granted fundamentals in terms of which other phenomena were to be explained. This was the

case, for example, with what is now (but not in the eighteenth century) the most famous study of what nature meant for knowledge and conduct, David Hume's *A Treatise of Human Nature* (1739-40). While the eighteenth-century usage is obvious, it is much less clear when and why the term became common. The poet John Dryden, for example, referred in 1668 to 'a just and lively image of Humane Nature', implying that such an image would portray the shared and inherent qualities of all individual humans (Dryden 1971, p. 15).

Tracing the origins of common word usage is difficult, but it could at least begin with scanning for references to 'human nature' across a wide range of literary, economic, political, and theological writing in the early seventeenth centuries. For example, is it to be found in the eminently practical discipline of rhetoric? At a deeper level of analysis, how are we to relate the origins of the usage to debate about the origin of modern notions of the self? Here, however, I wish merely to note the scope of what is needed to overcome ignorance about the origins of such usages. It is for the future to unravel Samuel Johnson's claim that 'human nature became the fashionable study' in the late seventeenth century (quoted in Fox 1987, p. 1).

When Johnson, Bishop Joseph Butler, or Scottish writers such as Adam Ferguson, Adam Smith or Hume himself referred to human nature, they invoked a ground defining what it was to be human. 'Human nature' was attributed to the individual and made human beings human. Thus a reviewer of Smith's *The Theory of Moral Sentiments* in 1759 referred to 'the principle of Sympathy, on which he founds his system, [which] is an unquestionable principle in human nature...' (quoted by the editors in Smith, A. 1976, p.27). For writers such as Smith, who were concerned that intellectual understanding should start from and return to common experience as a basis for civil society, 'human nature' served as an a priori explanatory category. They discussed the category's content not the legitimacy of the category itself. At the end of his often quoted comparison between the connection of ideas in the mind and attraction in physical nature, Hume wrote: 'Its effects are every where conspicuous; but as to its causes, they are mostly unknown, and must be resolv'd into *original* qualities of human nature, which I pretend not to explain' (Hume 1588, p. 13; emphasis in Hume). When Butler in the early 1720s delivered sermons on human nature, he stressed that the conscience had natural supremacy as a power to guide conduct in each person; thus, he argued, the possibility of moral conduct was grounded in our God-given nature. Butler understood his responsibility as an Anglican clergyman to be the provision of an empirical account of God's design in human nature (Butler 1896).

Many complex issues arise in developing this analysis of the category of human nature as a subject for history. I will mention just three of the most pressing: the relation between the language of 'human nature' and that of 'the science of Man'; the place of what would now be described as gendered language in accounts of human nature; and the ambiguity of the word 'nature'. These are all vast topics and so I can only sketch out preliminary points.

Gladys Bryson, Lester G. Crocker, and Peter Gay are three historians who have described the project of creating 'the science of Man' as a central and perhaps even the central feature of the Enlightenment. Such a science was projected in the seventeenth-century masterpieces by Thomas Hobbes. Hobbes demanded that 'he that is to govern a whole Nation, must read in himself, not this, or that particular man; but Man-kind...', and he then proceeded to ground his idea of a commonwealth in a science 'Of Man' (Hobbes 1991, p. 11, and Part 1). Dividing science ('that is, knowledge of consequences') into natural history and civil history, accepting the Baconian meaning of 'history' as the descriptive route to knowledge, 'Man' became a subheading of natural history (Hobbes 1991, p. 61).

The science of man concerned the consequences of 'the Qualities' of our nature - the senses, the passions, and speech. (It is worth remembering that the disciplinary subjects that Hobbes listed as concerned with this division of knowledge were optics, music, ethics, poetry, rhetoric, logic, and 'the science of just and unjust'.) By the time Hume attempted a comparable normative project, he described it as 'the science of Man' and straightforwardly identified 'human nature' as its subject (Hume 1888, p. xix). My question is therefore whether reference to human nature became commonplace in tandem with a systematic project on the science of man, or whether this reference had multiple roots and those writers undertaking the project adapted and spread an already established vocabulary. Not many moral philosophers, after all, were followers of the Hobbes who had shocked his contemporaries with 'a farrago of Christian Atheism' (quoted in the editor's introduction to Hobbes 1991, p. ix) since he firmly identified material and human qualities in conceiving of a science of man. Some Enlightenment writers, notoriously D'Holbach, took a materialistic route; however, a conviction in transcendent or, at least, non-material qualities of human nature was far more common.

Turning to questions of gender - which was certainly not an eighteenth-century category, we cannot but ask whether 'the science of Man' or 'human nature' referred only to half of humankind. There are no simple answers to this, as a large body of writing by historians now shows. It is at least clear that eighteenth-century authors themselves were as curious about the qualities of woman as of man. As always in eighteenth-century discourse, this attention was at one and the same time directed to their respective 'natures' and to the social worlds that these natures made possible and that, in differing analyses, enhanced or denied these natures. For many writers, 'the science of Man' addressed the qualities of man as the standard of human nature, meaning that the qualities of woman were described by (not necessarily negative) contrast. There was a striking tendency for 'man' to refer to the universal in human nature and for 'woman' to refer to the particular, the latter often in practice leading to talk of children and the family. For example, Montesquieu most distinguished woman from man in the context of discussing such topics as luxury, marriage customs, the family, and population (Montesquieu 1989, Books 7, 16 and 23). When Locke or Hume discussed 'the understanding', they didn't consider the distinction and referred without the slightest self-consciousness to 'Man'.

It has also been claimed, however, that there was a distinctive Enlightenment project to create 'the science of woman' (Tomaselli, 1991). This, it is suggested, was a desire to understand woman as a natural entity, not by setting up a series of contrasts with man, but as she was in herself. The eighteenth century produced a huge literature on women, written by women as well as men, assaying their character, quality of love, fertility, education, friendship, and so on, taking comparative, historical and literary perspectives. Some of this literature, especially after about 1770, attempted to examine the condition of 'women, both in the present and in the past without repeating values subordinating women. Indeed, that subordination itself became a subject of inquiry. J.-J. Rousseau, Denis Diderot and the comte de Buffon contributed elements, Mme Dupin or Mme de Staël attempted complete histories of women and less well-known writers such as the Scotsman William Alexander, who published *The History of Women from the Earliest Antiquity, to the Present Time* (1779), worked in the same vein. It is not obvious that such work constituted a 'science' of woman or that it emancipated women from being simply 'the sex'. But the point I stress is that if 'human nature' was an Enlightenment category and if there were repeated calls for a science of man, this by no means excluded woman from being a systematic, self-conscious, and politically differentiated subject.

The third and, in the space of this paper, final question concerns the many meanings of the word 'nature'. It is undoubtedly one of the richest and most complex words in the English language. I suggest that there was a central ambiguity in eighteenth-century references to human nature that, in part, explains this terminology's appeal and sense of fitness to contemporary writers. On the one hand, some thing's nature was its essential qualities, those attributes that made it what it was and differentiated it from other things. Christian thought generally sustained an everyday familiarity with man's or woman's nature in this sense. The humanistic disciplines of rhetoric or jurisprudence grounded their approach to ordering social or political affairs with systematic references to these qualities or natures. On the other hand, it was common in the eighteenth century to refer to Nature (with a capital 'N') as a state or reality in itself, making it possible to contrast nature with culture, art, or fashion in the manner which was so important to critical political and aesthetic sensibility. Reference to 'human nature' thus became a way of including humankind in the realm of Nature and of arguing about the degree to which individual experience and collective history then turned that nature into something different. By referring to 'human nature' it could appear that Nature herself set the conditions making experience and history possible and setting the terms in which man and woman were to be understood. This usage also made possible powerful moral appeals to 'a state of nature'. Breath-taking developments in natural philosophy gave hope that Nature, and by extension even man's and woman's nature, could be known and in the knowing become the ground of moral action.

These two meanings of the word 'nature' ran into each other and it would miss the whole point of usage to ask for tight definitions. The ambiguity may be historically significant, as evidence of a word mediating a historical shift within a discourse or

connected representations of meaning. This shift is a significant part of what is denoted in references to 'the Enlightenment'. It redescribed an essential Christian nature, originally embodied in Adam and Eve, in terms that owed more to Nature. Increasingly, Enlightenment writers understood the qualities that made men and women what they are, i.e., their natures, to be qualities of Nature transmuted by history and experience. I suggest that it is worth exploring whether 'human nature' became a category in such widespread use in part because the ambiguousness of the word 'nature' meant that belief could change slowly without too great a sense of loss or too great a fear of social disruption. The project of studying human nature was one on which the widest spectrum of opinion could agree. Reading what was written on human nature, different audiences nevertheless distinguished different resonances and listened to those most suited to their purposes. In itself, the call to study human nature attracted little surprise or opposition; studying that nature, in a way, had long been a preoccupation of Christian and humanistic culture. Yet the Enlightenment project to study human nature as a science certainly reconstructed a new view of that nature, one that in the late twentieth century appears intrinsic to modernity.

NOTE

This paper originated in reflections about a conference on 'The nature of the human sciences in the 17th and 18th centuries' held at Lancaster University in September 1990. The conference was held in association with the journal *History of the Human Sciences* which will publish some of the papers in vol. 5, 1992. This paper also relates to my attempt to write on, 'human nature' for a book forthcoming with the University of California Press, *Inventing Human Science*, edited by James Buickerood, Christopher Fox, and Roy Porter. My thoughts and materials owe much to the contributors to this conference and to this book.

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The impact of Skinner in the Journal of Applied Behavior Analysis

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One of the great developments that have taken place in our time is, without a doubt, that of the wide range of intervention techniques derived from Learning Psychology. They were in part developed by behaviorism, from which sprang Behavior Therapy (in its clinical aspect) and, even more generally, the modification of behavior. This shows the great deal of attention being paid nowadays to the currently dominant applied technical Psychology. This happens, precisely, when Psychology is shaken by a number of crises -that of an insufficiently unified theory among others (Westland, 1978)-, leading to a situation that offers, as its principal trait, a high number of theoretical options, not to mention that methodological plurality contributes to reinforce the existing image of diversity (Carpintero, 1989).

This situation, for many, should be carefully contemplated in connection with the loss of the theoretical and academic influence of American Behavioral Psychology, and its substitution by a model that is rather centered on mediational and cognitive processes. Today's Psychology is experimenting such a strong and deep change, that it is tempting to turn to the idea of a revolutionary substitution of a paradigm dominated by another, in order to explain what is happening before our eyes, and many have done so. Nevertheless, we agree with Caparrós (1979, 1980) in that this new paradigm has not totally replaced behaviorism, but has narrowed it to its own scope. In spite of the protagonism of the cognitive approach, an important legacy that we could call the "inherited position" is still conserved. In the first place, this would be represented by Skinner's work, a belligerent author until recently opposed to the changes that were occurring.

The instrument of this radical Skinnerian Behaviorism is, undoubtedly, Behavior Modification, a technology that started in the psychological laboratory and that was based on the therapeutical application of the Learning Theory that, at the same time, was based on animal investigation. In any case, the fact is that Behavior Modification, that can find one of its roots in Skinner, has achieved surprising developments in many ways (Weckowitz & Liebel-Weckowicz, 1990).

The aim of this paper is to analyze the impact of B.F. Skinner in Applied Behavior Analysis. In order to do this, we will analyze the principle means of communication within the behavioral movement, the *Journal of Applied Behavior Analysis*, from its foundation in 1968 to 1988.

OBJETIVES, METHODS AND SOURCES

The methodology used in this study is based on objective and quantitative techniques from a historical, chronological perspective (Carpintero & Peiró, 1981), which regards science as an organization (Carpintero, 1980), as well as other non-quantitative procedures.

The data was obtained from the *Journal of Applied Behavior Analysis* from 1968 to 1988.

Current historiographical trends are based on objective and social criteria founded in the analysis of the references. In general, it has been verified that psychologists receiving the greatest numbers of citations are those considered eminent according to a number of independent criteria (Endler, 1986). The authors and works of greatest impact in a given field of study can be determined by a study of the citations (Price, 1963).

In this journal, we have examined the influence of Skinner in the field of Behavior Analysis by means of the analysis of several indicators of scientific eminence. Furthermore, we have examined the references appearing in all the papers published in this 21-year period, in order to find out the number of citations received by this author as well as his most cited works. In the same manner, we have examined the content of the papers citing Skinner in order to learn their subject matter.

THE MOST CITED AUTHORS IN THE JOURNAL OF APPLIED BEHAVIOR ANALYSIS

The total number of references found in the papers published in this journal from 1968 to 1988 is 19184, with an average of more than 900 references per year (913'5) and a rate of more than 15 (15'66) references per article. This value may be said to be characteristic of experimental sciences (Price, 1965), within the literature of the immediacy of the research front.

If we analyze the distribution of the citations of the most cited authors in the *Journal of Applied Behavior Analysis* (1968- 1988), we see a pronounced dispersion. While the majority of authors are cited once on average, a small group of them are frequently cited, resulting in a very marked pyramidal distribution.

In this study, we concentrate on the 12 most cited authors, those who have most significantly contributed to the development of contemporary Behavior Analysis. The most visible authors, and therefore the "classic performers" in this Journal appear in table 1. These 12 authors are cited in 4118 of the references, which represents more than 20% (21'46%) of the total references, with an average of 343 citations for each of them (Platz Index: 2'53).

Now, we will briefly comment on the 12 most systematically cited authors in different thematic areas: methodological and conceptual aspects of Applied Behavior Analysis, the experimental study of autism, the application of token economy and other techniques used with psychiatric patients and children with behavioral problems in school.

The first group of authors study the methodological and conceptual aspects of Applied Behavior Analysis. D.M. Baer, M.M. Wolf and T. Risley authored the seminal article "Some Current Dimensions of Applied Behavior Analysis", which was published in the first issue of the *Journal*. This article established the norms of the behavioral movement. Its authors describe the methodology, strategies, language, settings and other problems in the area of application, and suggest 7 main dimensions: applied, conceptual, analytical, generalization, technological and effective. Of evident importance in Behavior Analysis are changed behavior, the quantitative characteristics, the experimental manipulations that clearly analyze the cause of the change, detailed descriptions of the procedures which contribute to change, the effectiveness of the techniques in achieving sufficient change, and its generalization (BAER, WOLF & RISLEY, 1968). These three authors were the first three editors of the *Journal of Applied Behavior Analysis*. The Journal was under their direction for its first 7 years. Wolf is also cited in articles dealing with the "Achievement Place", dedicated to the social reintegration of juvenile delinquents.

The second group of authors consists of T. Ayllon and N.H. Azrin, creators of the "Token economy", system which is a complex program involving a reorganization of the hospital environment with the goal of motivating psychiatric patients and modifying their behavior by means of generalized reinforcers (the tokens). The experiment has been influential in the history of Applied Behavior Analysis and Psychology in general. The same technique has been applied in school settings in order to change inappropriate behavior in children. W.C. Becker and K.D. O'Leary applied the operant techniques using token reinforcement and praise. R.V. Hall also used this procedure in the class-room, tokens were withheld when the students' behaviors were inappropriate. Y A.E. Kazdin besides investigating the procedure, has conducted a number of experiments in schools and institutions applying the operant techniques.

A third group of authors have dedicated themselves to the experimental study of autism. O.I. Lovaas, a pioneer in the study of autism in 1961 at the University of California, carried out a program with autistic children which used various procedures such as reinforcement with food, praise, extinction and punishment. R.L. Koegel, a pupil of the aforementioned author, has also experimented with the use of operant techniques with autistic children.

Finally, B.F. Skinner is the founder of operant conditioning and one of the most important psychologists of this century. His most important works will be dealt with in the next section. Among the applications of his theory (furthermore of Behavior Analysis), the most notable are the following: 1. The "Skinner Box" has had enormous repercussions in the field of Experimental Psychology. With this experimental device Skinner demonstrated that if a given behavior in an animal deprived of food is associated with the presentation of food (reinforcer) the frequency of the behavior increases (Bayés, 1980). 2. The "Tender Baby" is a crib with glass walls and ceiling built by Skinner for his second daughter, Deborah. By means of several rattles hung from the ceiling, the baby was able to raise or lower by several degrees the air

temperature within the crib. 3. The "Teaching Machine" was also created for Deborah and, like the two previous devices, was based on the principles of the Experimental Analysis of Behavior. It consists of a device which presents stimuli to the student, a receptor for the student's responses and reinforcement device which indicates whether the answers are right.

SKINNER'S IMPACT IN THE JOURNAL OF APPLIED BEHAVIOR ANALYSIS

From 1968 to 1988, Skinner was cited 179 times (Platz' index: 2'25) in this journal. If we analyze the evolution of these citations in this 21-year period, we may see that in the first 10'5 years (1968-1978) he was cited 113.5 times -which represents 63% (63'41%) of the total-, while in the second 10'5 year period he was only cited 65'5 times, which represents 37% (36'59%) (Figure 1). According to this data it seems that Skinner's influence in the *Journal of Applied Behavior Analysis* has diminished with the passing of time.

1. Skinner's most cited works

The 179 citations this author receives in the *Journal of Applied Behavior Analysis* (1968-1988) are distributed among 33 different works. The 10 most cited works account for 148 or 83% of the citations, and 90 or 50% of the citations were taken from the 3 most cited works. Skinner's 10 most cited works deal with the Experimental Analysis of Behavior (Table 2).

Firstly, the article standing out for its antiquity is the 1935 "The Generic Nature of the Concepts of Stimulus and Response", which analyzes stimulus, response and the conditions modifying the relationship between them. With his 1938 book *The Behavior of Organisms*, the Experimental Analysis of Behavior, which has resulted in so much success for Psychology, was begun. In this book Skinner completed the main experiments carried out up to that point showing a program of contingencies applied to a white rat (Skinner, 1938).

His most widely sold book, *Walden Two*, was published in 1948. It is a novel which had a great impact in the 1960's. In this novel, Skinner proposes a means to solve the daily problems of a community of 1000 people with the help of the Experimental Analysis of Behavior (Skinner, 1948). While in the 1950 article "Are Theories of Learning Necessary?" he holds that the fact of a theory resulting from experimentation does not indicate that it is valid unless the experiment proves to be valuable.

In this sense, the 1953 book *Contingencies of Reinforcement: A Theoretical Analysis*, presents an analysis of the relationship between behavior and its consequences. In the same year, *Science and Human Behavior* explained the author's position in Psychology. In this work, Skinner initiated the possibility of a science of human behavior and the instances exercising control over man: government and the law, religion, and education. Skinner maintained that contingencies of survival and natural

selection are similar to contingencies of reinforcement and operant conditioning. Both explain selection by means of consequences (Skinner, 1953).

Along this same theme, the 1957 book *Schedules of Reinforcement*, written in collaboration with C.B. Ferster, offers the characteristic performances of a large variety of reinforcement schedules and includes data on more than 7000 hours of behavior recorded in the laboratory (Ferster & Skinner, 1957). Also published in 1957 was *Verbal Behavior*, considered by the author himself to be his most important work. In it, linguistic behavior is analyzed; verbal operatives are classified according to the reinforcement contingencies maintained by the verbal community. This work was harshly criticized by N. Chomsky, a critic who enjoyed great influence in Psycholinguistic and Cognitive Psychology. However Skinner himself said that Chomsky had not understood what the book was saying. Real verbal behavior is generated through reinforcement of the verbal perception and all characteristics of verbal behavior can be attributed to reinforcement contingencies (Skinner, 1957).

The 1968 book *The Technology of Teaching* deals with education and includes experiments and research with the use of machines in teaching. In this work, Skinner expounds on the principles of pedagogical technology based on reinforcement contingencies (Skinner, 1968).

Lastly in his 1977 work *Beyond Freedom and Dignity*, its author establishes the determination of human behavior in the environment. An environment responsible for the evaluation of the species and the repertory of acquired behaviors for each one of its members. Human behavior is controlled by the environment. It is a matter of a scientific formula of behavior which according to Skinner can help us to achieve feelings of liberty and dignity (Skinner, 1971).

2. Thematic contents

Skinner is cited in 134 articles published in the *Journal of Applied Behavior Analysis* (1968-1988). The articles are experimental in nature, and we have organized them in Table 3 according to the classification used by Brengelman, Carpintero and Peiro and the authors themselves (Brengelman, 1975; Carpintero & Peiró, 1981; Sos-Peña, 1987).

The largest category is Techniques and Methodology, which includes 96 articles or 72% of the total. Articles in the remaining categories -clinical problems, theoretical studies, specific problems, evaluation and measurement, and problems of schoolchildren- comprise 28% of the total.

In techniques and Methodology we have included works dealing with operant techniques, such as the reinforcement procedure, token economy, punishment and other behavioral techniques such as overcorrection, informational feedback autocontrol, imitation and time-out, as applied to autistic children, mentally and physically handicapped persons, and children with behavioral problems.

CONCLUSION

This paper shows Skinner's impact in Behavior Analysis as verified through an analysis of various scales of eminence, an analysis of the references he receives in the *Journal of Applied Behavior Analysis* (1968-1988) and the opinion of most important authors in Behavior Analysis (Sos-Peña, Tortosa & Carpintero, 1987).

Skinner is one of the most frequently cited authors in the *Journal of Applied Behavior Analysis* (1968-1988), which is the main means of communication within the field of Applied Behavior Analysis. This journal is devoted to the application of behavior to social settings. It began being published in 1968 and aimed at bringing a new dimension to operant research and solving new social problems.

Skinner's most cited works, therefore those having greatest influence in Behavioral Analysis, deal with conceptual aspects of the Experimental Analysis of Behavior. As suggested by data, they have been used in the area of application to problems of social relevance, such as mental deficiency, autism, delinquency, mental illness education...

In conclusion, Experimental Analysis of Behavior is the root of Behavior Analysis, and Skinner, a year after his death, is considered to be one of the most important psychologists in the history of Psychology.

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TABLE 1: THE MOST CITED AUTHORS IN THE JOURNAL OF APPLIED BEHAVIOR ANALYSIS (1968-1988)

AUTHOR	CITATIONS	INDEX OF PLATZ
M.M. WOLF	686	2'84
D.M. BAER	648	2'81
T.R. RISLEY	484	2'68
N.H. AZRIN	364	2'56
K.D. O'LEARY	322	2'51
O.I. LOVAAS	300	2'48
A.E. KAZDIN	258	2'41
R.V. HALL	258	2'41
W.C. BECKER	243	2'38
R.L. KOEGEL	200	2'30
B.F. SKINNER	179	2'25
T. AYLLON	176	2'24

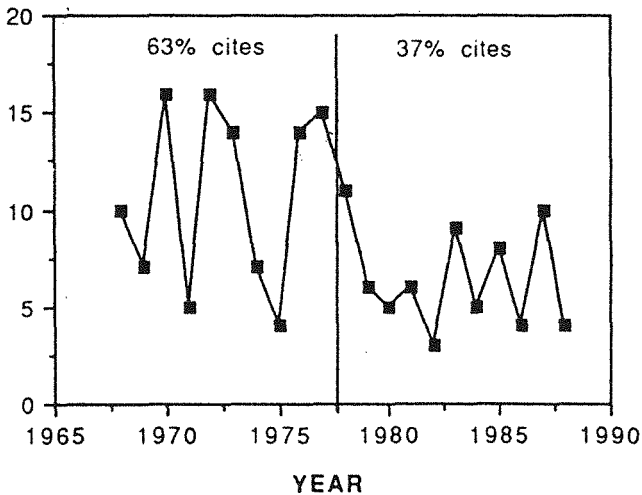


FIGURE 1: EVOLUTION OF NUMBER OF CITATIONS TO SKINNER IN THE JOURNAL OF APPLIED BEHAVIOR ANALYSIS (1968-1988)

TABLE 2: SKINNER'S MOST CITED WORKS IN THE JOURNAL OF APPLIED BEHAVIOR ANALYSIS (1968-1988)

AUTHOR	WORK	CITATIONS
B.Skinner	<i>Science and Human Behavior</i> , 1953	36
B. Skinner	<i>Verbal Behavior</i> , 1957	27
B.Skinner	<i>Schedules of Reinforcement</i> , 1957	27
& Ch.Ferster		
B.Skinner	<i>The Technology of Teaching</i> , 1968	17
B.Skinner	<i>The Behavior of Organisms</i> , 1938	10
B.Skinner	<i>Contingencies of Reinforcement</i> , 1953	10
B.Skinner	<i>Beyond Freedom and Dignity</i> , 1971	7
B.Skinner	<i>Walden Two</i> , 1948	6
B.Skinner	<i>The Generic Nature of the Concepts of Stimulus and Response</i> , 1935	5
B.Skinner	<i>Are Theories of Learning Necessary</i> , 1950	3

TABLE 3: THEMATIC CLASSIFICATION OF THE WORKS CITING SKINNER IN THE JOURNAL OF APPLIED BEHAVIOR ANALYSIS (1968-1988)

CATEGORY	FREQUENCY	%
Clinical Problems	3	2'00
Theoretical Studies	5	4'00
Specific Problems	7	5'00
Evaluation and Measurement	7	5'00
Problems of School Children	16	12'00
Techniques and Methodology	96	72'00
Total	134	100'00

The history of Psychology in Berlin as a “natural” or a “social” science. Steps towards institutionalization at the University of Berlin and academic politics at the end of nineteenth-century Germany -A case study of Hermann Ebbinghaus (1850-1909) or the so-called “Ebbinghaus-Dilthey conflict”

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ABSTRACT

History of psychology in Berlin, that sounds like “local history”. But this is not true. History of psychology in Berlin will be approached in three ways: (1) the psychology which was developed in Berlin; (2) the influence of psychology in Berlin on psychology in other places; (3) the influence of psychology in other places on psychology in Berlin.

In relation with this strategy, history of psychology in Berlin is a “Case Study” and a “Paradigm” for a general process of psychological development.

An important period in the development of Psychology in Berlin was the time between 1850 until 1893, the period of the institutionalization at the university of Berlin and the time of an deep conflict about two different ways to a psychology as science.

In 1850, Hajim Steinthal began his lectures on the “psychology of language” in Berlin. In the same time Moritz Lazarus was living in Berlin. The friendship and cooperation of these two scholars was important for the foundation of the so-called “Berlin School of Ethnological Psychology”. In winter 1893, Carl Stumpf founded the “Department of Psychology” at the University of Berlin.

The main part of the paper includes a description and discussion of some aspects of a personal conflict between Wilhelm Dilthey and Hermann Ebbinghaus and the academic politics at the University of Berlin at the end of the nineteenth century.

The "Ebbinghaus era" in Berlin (1880-1894), with its methodological controversies about the subject, aim and method of psychology, was an important part of the general history of our science. And in this sense, academic politics and personal conflicts around the foundation of psychology at the University of Berlin is a paradigmatical conflict between two different ways of understanding psychology: psychology based on the model of natural or social sciences. Both positions may still be seen today in the controversies of contemporary psychology.

INTRODUCTION

History of psychology in Berlin, that sounds like "local history" or, when seen more critically, "history of science in a specific community or region". But this is not true.

History of psychology in Berlin will be approached in three ways: (1) the psychology which was developed in Berlin; (2) the influence of psychology in Berlin on psychology in other places; (3) the influence of psychology in other places on psychology in Berlin. In relation with this strategy, Berlin is to be seen merely as an example. In Berlin - the bustling middle-class metropolis, the German capital, the crossroad between East and West - science, including psychology, has played an important role in intellectual, cultural and political life. In this relation, history of psychology in Berlin is a "Case Study" and a "Paradigm" for a general process of psychological development (Ash 1980; Woodward & Ash 1982; Lück & Miller 1991; Sprung & Schönpflug 1991; Woodward & Cohn 1991).

An important period in the development of Psychology in Berlin was the time between 1850 and 1893, the period of institutionalization at the university of Berlin. In 1850, Hajim Steinthal (1823-1899) began his lectures on the "psychology of language" in Berlin. This was the beginning of the so-called "Berlin School of Ethnological Psychology" and in 1893, Carl Stumpf (1848-1936) founded the "Department of Psychology" at the University of Berlin. This was the beginning of a longstanding development of psychology as a science and later as a profession.

To begin with, we shall briefly review the period from 1850 till 1893.

Five important steps on the way to institutionalization in Berlin

1850 was the first important year for the history of psychology in Berlin. In this year Hajim Steinthal began his lectures on the "history of language" and the "psychology of language". At the same time Moritz Lazarus (1824-1903) was living in Berlin. The friendship and co-operation of these two scholars was important for the foundation of the so-called "ethnological psychology".

Steinthal was the linguistic genius of the two. His works include "The Roots of Chinese Dialects" which was published in 1854 and the book entitled "An Introduction to Psychology and Linguistics", which was published in 1874.

1860 was the second important year. In Leipzig, Gustav Theodor Fechner (1801-1887) published his book, *Elements of Psychophysics* (Sprung & Sprung 1983). Hermann Ebbinghaus read the *Elements of Psychophysics* in the summer of 1875 in London and he gave him credit for stimulating his research on memory. Hermann Ebbinghaus, who was later to become the first professor of experimental psychology at the University of Berlin (1886), shared many of Fechner's assumptions about the nature of psychology. In 1897 he dedicated his large book entitled *Introduction to Psychology* to Fechner. Ebbinghaus had a personal collection of Fechner's books and writings which can be examined in the library of the German military university in Munich. Fechner's work was for methodological reasons important for Ebbinghaus. Ebbinghaus wrote that all his ideas had come only from Fechner. The Dedication reads in the long version: "Dem Andenken Gustav Theodor Fechners gewidmet" (Dedicated to the memory of Gustav Theodor Fechner) and in the dedicatory poem reads as follows:

"Betrachte ich den Fleiß, den ich verwendet,
Sah ich die Züge meiner Feder an,
So konnt' ich sagen, dieses Buch ist mein.
Doch überdenk' ich's recht, da es vollendet,
Woher mir alles kam, wohin es zielt,
Erkenn' ich wohl, ich hab' es nur von Euch".

A loose translation might read (after Marshall & Rodway 1987):

When I consider the effort I make,
When I look at the strokes of my pen,
Then I could say that this book is mine.
But when I think it over, then I know,
From where it came, to where it leads,
I recognize clearly, it has come only from you.

It was also in 1860 that Lazarus and Steinthal founded the *Journal of Ethnological Psychology and Linguistics*. This was the first journal of scientific psychology in Germany. According to their conception of ethnological psychology, language was the key to the soul of a people or nation (*Volksgeist*). From a modern perspective, we can say that the ethnological psychology of Lazarus and Steinthal was an early version of social and developmental psychology (Eckardt, Bringmann, & Sprung, 1985; Sprung, 1991).

1880 was the third important year. On November 20, 1880, the Prussian Minister of "Spiritual, Medical and Educational Affairs", Hans von Puttkammer, wrote to

inform the Philosophical Faculty of the University of Berlin that Rudolph Hermann Lotze (1817-1881), who was then at the University of Göttingen, would take over the unoccupied second chair in Philosophy. Lotze was an important scholar in nineteenth-century Germany. Like Wilhelm Maximilian Wundt (1832-1920), he had a scientific and philosophical background and was equally at home in both experimental psychology and traditional philosophy. Lotze had a number of outstanding scholars who contributed to the development of psychology as an independent science. Examples of these include Georg Elias Müller (1850-1934) and Carl Stumpf. Lotze began his lectures on April, 1881 but died after he had been in Berlin for only three month. In the same year, on October 25, 1880, Hermann Ebbinghaus presented his habilitation thesis to the Philosophical Faculty of the University of Berlin. This thesis was entitled, *On Memory: An Experimental Study*.

1886 was the fourth important year. On October 1886, the Prussian undersecretary in the "Ministry of Spiritual, Medical and Educational Affairs", von Althoff, appointed Ebbinghaus Associate Professor of the University of Berlin. In the same year, Ebbinghaus gave lectures on subjects such as "psychophysics", "perception" and the "psychology of space images". He also began a program of laboratory exercises in experimental psychology for students of philosophy. The start of lectures on experimental psychology and the beginning of laboratory exercises in 1886 was the "de facto" foundation of Berlin's Department of Psychology.

1893 was the fifth important year. On December, 18 1893, Carl Stumpf was appointed Professor of Philosophy at the University of Berlin. The establishment of a third chair in philosophy and the appointment of Stumpf was the official act of institutionalization of psychology in Berlin; the "de jure" foundation of the Department of Psychology.

And now more details about the difficult way to the foundation of psychology at the Berlin University.

HERMANN EBBINGHAUS AND ACADEMIC POLITICS IN BERLIN AT THE END OF THE NINETEENTH CENTURY

Who was this Hermann Ebbinghaus? Today, the name of Ebbinghaus is firmly associated with the law of human learning and forgetting; as well as the "learning and saving methods". Ebbinghaus was also one of the first psychologists to develop a usable test of intelligence for children: the "combination method". In addition to this, he founded (with Arthur König) the *Journal of Psychology and Physiology of Sense Organs* in 1890. This was later renamed the "Journal of Psychology".

Ebbinghaus was born in Barmen in Rhineland on January 23, 1850. In his short autobiography, Ebbinghaus wrote: "Brought up as a protestant, educated at a gymnasium which had been established in my native town at the time, I entered the University of Bonn in the fall of 1867 and later attended the Universities of Halle and Berlin. Having

begun to study history and philology, I gradually switched to philosophy, ... in the fall of 1871, having left the army which I had joined at the outbreak of the Franco - Prussian war, ..." (Ebbinghaus, 1873)(1).

In 1879, Ebbinghaus began an extensive series of experiments in which he memorized lists of meaningless syllables. Their results provided the basis for his habilitation thesis which he submitted to the Philosophy Faculty of Berlin's Frederic - Williams - University ("Friedrich-Wilhelms-Universität") on April 29, 1880. One can read in the Dean's documents: "Dr. phil. Hermann Ebbinghaus ... now applies for the *venia docendi* in philosophy and submits: 1. His doctor's diploma and doctoral dissertation; 2. His commission as second lieutenant-in-reserve, dated February 12, 1874; 3. A diploma from the University of Bonn, dated Michaelis 1873, and an application, dated Easter 1875, for the diploma from this university where the applicant attended lectures on mathematics for three terms; 4. A treatise for the habilitation, written in German with the title: *Memory: A Contribution to Experimental Psychology* (Huebner, 1880; p. 166).

The experts were the philosopher Edward Zeller (1814-1908) and the physiologist Hermann von Helmholtz (1821-1894). Zeller was an expert in classical philosophy. Zeller writes in his expert opinion (Gutachten): "The habilitation thesis on "memory" follows in general Fechner's psychophysics in that it endeavours to arrive experimentally ... at average measures of psychological processes. The experiments ... seem to be well conceived and correctly executed. However, the factual results are not too enlightening as they merely confirm what could be predicted. Nevertheless, on the whole, the author demonstrates a mastery of the subject and the research approach" (Zeller, 1880; pp. 167-168). In turn, Helmholtz writes: "The experimental investigation is well planned and has been carried out with care and great perseverance. The discussion of results shows good judgement and sound mathematical knowledge. The results are not impressive. However, at the beginning of a series of experiments, one cannot always predict what will be the eventual outcome" (Helmholtz, 1880; p. 168).

On the basis of these expert opinions, and the candidate's performance during the habilitation proceedings, Julius Zupitza, the Dean of the Philosophical Faculty, wrote to von Puttkammer on October 25, 1880: "I take the liberty of informing your Excellency that Dr. Ebbinghaus has qualified as Assistant Professor (*Privatdozent*) of Philosophy in the Faculty of Philosophy at the Friedrich Wilhelms University of Berlin" (ZSTA Merseburg, p.49). Ebbinghaus immediately announced that he would give lectures on "history of psychology" and "sensation and sensory images" in the winter term 1880/81.

Many people think of Ebbinghaus today only in terms of his pioneering experiments on memory. A detailed examination of his work reveals a much greater variety of interests. In the years he spent in Berlin (1880-1894), he was by no means solely concerned with experimental psychology. His lectures had titles such as "On the Philosophy of Schopenhauer", "A History of New Philosophy with an Introduction to

the Development of the Old" and "Philosophical Exercises in Connection with Berkeley's Principles of Human Cognition".

We can learn about Ebbinghaus as a private person from a letter by Lou-Andreas Salome (1861-1937), the friend of Rainer Maria Rilke (1875-1926), Friedrich Nietzsche (1844-1900) and Sigmund Freud (1856-1939), which was written on New Year's Day, 1883: "It is four o'clock in the morning and the old year is buried. In the elegantly furnished apartment on Unter den Linden, where we bachelors have just emptied the last glass of punch in his honor, Ebb(inghaus) ... is still sitting in front of the little Christmas tree, dreaming his New Year's dreams. Today, he is looking into the future and I am looking into the past. He has only favors to ask of the New Year while I give only thanks" (Böttger, 1981; p. 177).

Initially Wilhelm Dilthey's (1833-1911) opinion of the new lecturer, expressed in a letter to Count Paul Yorck von Wartenburg, is extremely enlightening in this respect: "Every week, Ebbinghaus and I take a walk discussing philosophical problems. Locally, he has the best and clearest knowledge of psychology" (Dilthey, 1883; p. 38). Later Dilthey and Ebbinghaus became bitter adversaries.

On January 22, 1886, Dilthey and Zeller proposed that the Faculty of Philosophy appoint Ebbinghaus as "Associate Professor" (außerordentlicher Professor): On June, 1886, the minister finally informed Ebbinghaus of his "... appointment ... with the obligation to lecture on psychology and aesthetics and to offer laboratory exercises in experimental psychology" (Gossler, 1886; p. 1).

Despite his success (or perhaps because of it) as a university lecturer, his relations with some of the faculty's leading members began to deteriorate. When the vacancy for a third full professor in the Faculty of Philosophy was being discussed in the summer of 1893, Ebbinghaus was not even considered. The existing documents suggest that Ebbinghaus had made enemies among the leading faculty members with his focus on experimental and physiological research by students. On December 18, 1893, Carl Stumpf was called from Munich to take over this chair. Disappointed, Ebbinghaus left Berlin to become a full professor at the university of Breslau in Silesia.

From Breslau, he carried out his fight against Dilthey even more vigorously. He replied to Dilthey's essay, *Ideas Concerning a Descriptive and Explanatory Psychology* (Dilthey, 1894) with a paper entitled, "On Explanatory and Descriptive Psychology" (Ebbinghaus, 1896). In this paper, he took a stand against the cultural- historical (geistesgeschichtlich) approach to psychology, as advocated by Dilthey, and defended a model of psychology based on the natural sciences. In his private letters, he was more outspoken. On October 27, 1895, he wrote to Dilthey: "I was, indeed, quite unprepared to find so much unfairness toward contemporary psychology and so little clarity in regard to the fact that what you are recommending, is exactly what people have been doing all along. Neither had I expected such inadequate assessment of experimental endeavours as is shining through the pretended layer of artificial good will" (Ebbinghaus, 1895; p. 46). In a letter to his former colleague in Berlin, Hermann Diels (1848-1922),

he wrote: "Psychology, at it is pursued today, is young and struggling for the recognition it so badly needs. It is not only a question of getting rid of the hostile prejudices of the older generation. Equally important is to obtain adequate apparatus, funds and space. Furthermore, it is necessary to win over opinion so that pressure can be exerted in this direction. Thus, anyone who takes these matters to heart cannot remain indifferent if someone like Dilthey depicts scientific psychology as something that has in principle gone wrong and that he, Dilthey, must straighten out. Many whose judgement counts a great deal lend him their ear and share his opinions. ... Dilthey is not a man of character, ...he used petty means. With these, he forces students into his classes; ... exerts pressure on colleagues to keep them from holding lectures which attract big audiences and, finally, furthers the good reputation of his publications" (Ebbinghaus, 1895a; pp. 1-4).

Dilthey, on his part, wrote to Count Paul Yorck von Wartenburg on March 10, 1896: "Recently I have received a letter from Lipps in Munich asking if I would like to give a paper on my psychological view at the Psychological Congress in Munich in the fall. ... All I can say is that under no circumstances will I sit in the same room with Ebbinghaus, facing a situation where I would have to exchange greetings or a word with him" (Dilthey, 1896; p. 210).

Today, we know "*sine ira et studio*" that Ebbinghaus' scientific point of view was correct. The progress of psychology would come in this time mainly from the analytical experimental methods of the natural sciences and from the use of mathematics. Only in this way could speculative philosophical psychology be overcome. However, we also know that Dilthey's view was correct in that the totality of psychological phenomena, as well as their interrelatedness and individuality, must also be kept in mind. An application of experimental methods and mathematics that is too rigorous can easily lead to so-called "datism" or "modelbound Platonism".

Generally speaking, the move to Breslau had been beneficial for Ebbinghaus since he was able to develop his scientific activities on a broad scale. In 1905, he accepted a call to the University of Halle. Halle was, at that time, a step higher on the academic ladder. On February 26, 1909, he unexpectedly died of pneumonia at the age of 59 (Sprung & Sprung 1987).

The successor to Ebbinghaus at the university of Berlin was Carl Stumpf, who accepted a chair in philosophy on December 18, 1893. He came in 1894 and worked in Berlin until his retirement in 1922. Stumpf turned the Department of Psychology into one of the world's leading centres of teaching and research. He was an important scholar in experimental psychology, musicology and philosophy. His students included some of the major figures within Gestalt psychology: Wolfgang Köhler (1887-1967) (Zimmer 1987), Kurt Koffka (1886-1941) and Kurt Lewin (1890-1947) (Schönpflug 1991). But the "Stumpf era" is another chapter in the history of psychology at the university of Berlin and this "era" was the subject of an other paper (Sprung, Sprung & Kernchen 1984).

All translations from German sources are by the authors of this paper

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Julian Ochorowicz's (1850 -1917) law of reversibility and its relevance to the mind-body problem

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The visitors to the Universal Exposition in Antwerp in 1885 could see, among other things, the four exhibits shown by the Parisian firm "Paul Barbier et Cie". One of them was a hypnoscope (fig. 1). The other three were various versions of a telephone system transmitting sound over a distance. Here we can see one of them a two-diaphragm telephone (fig. 2). The inventor of all these was a versatile Polish psychologist and philosopher, a strong advocate of reformed mesmerism, one of the leading theorists of hypnotism during the second half of the 19th and the beginning of the 20th centuries - Julian Ochorowicz (1850 - 1917).

After graduating from Warsaw University in 1872 with a degree in natural science, Ochorowicz continued studying psychology at the University of Leipzig. During his postgraduate year under Fechner and Drobisch, he wrote (in German) in 1873 his dissertation on human consciousness. On his return to Poland, Ochorowicz received in 1875 his habilitation at the University of Lvov and became the first Dozent of empirical psychology in a Polish university. His first teaching appointment at Lvov was in psychology and philosophy of physics. Otherwise his serious interest was experimental physics (specifically electromagnetism) and problems of hypnotism. In 1882 Ochorowicz went to Paris where, during his ten-years' stay, he was engaged in hypnotic therapy and experiments on the transmission of sound over a distance. After his return to Poland, Ochorowicz concentrated on studying mediumism. But the main occupation in which Ochorowicz was brilliantly successful was his hypnotic treating of patients. Ochorowicz was not only a good hypnotist; he was also belonged one of the best European theoreticians of hypnotism. It was Ochorowicz whom Charles Richet (1850 - 1935), a 1913 Nobel Prize winner in physiology, when preparing the 1905 edition of his *Dictionnaire de Physiologie*, entrusted with the writing of an article on hypnotism and mesmerism. For this paper Ochorowicz was awarded by the French Academy of Sciences. It was the irony of fate that his hypnotic practice met with the fierce opposition of Polish medical circles.

Although Ochorowicz is known mainly for his efforts as a psychologist, he worked throughout his career as a scientist in several fields outside the usual confines

of psychology. But in my view, a feature that was common to the whole of Ochorowicz's very versatile activity was that he worked along the borderline between the mental and the physical. As I see it, it is in this sense, that his contribution to the mind-body problem should be appreciated. Let us proceed to consider the evidence. We must first come back to the point from which we started.

The hypnoscope exhibited at the Universal Exposition in Antwerp (fig. 1) consisted of a steely slit tube approximately 5 cm long and 4 cm in diameter. After the magnetisation the tube served as a magnet with the edges of a slit as the North and South poles. This magnet was so strong as to lift weights twenty five times as heavy as itself. A hypnoscope served as a simple device for assessing what Ochorowicz named hypnotic susceptibility, or the subjects' susceptibility to hypnotisation.

A telephone system reproducing loud human speech was the discovery for which Ochoi-owicz was awarded a gold medal at the Univei-sal Exposition in Antwerp in 1885 and on which he took out a patent. As a matter of fact, it was a substantial improvement on the Bell electromagnetic telephone of 1875 in which a second diaphragm has been employed and an electromagnet has been replaced by a cylinder-type permanent magnet. It was thanks to the latter innovation that the new Ochorowicz's telephone made possible to transmit sound over a distance without aid of reinforcing batteries. Ochorowicz's telephones had been manufactured on a large scale in Europe till the end of the 19th century.

The question that immediately arises is what do the hypnoscope and the new Ochorowicz's telephone have in common? Fist of all, note that a cylinder-type permanent magnet in the two-diaphragm telephone is nothing but the hypnoscope. This seems to imply that some universal law of nature must underlie the functions of both of them. And it is to this claim that we now proceed.

Two factors contributed to Ochorowicz's invention of the hypnoscope. First, his courageous defense of animal magnetism, or mesmerism. For our present purposes we need not enter too far into this question. Suffice it to say that he stood up, at least theoretically, for the existence of animal magnetism independently of hypnosis, and strove for a rescue of the former from obscurity and disrepute. Second, his early investigations into the influence of a magnet on the human body with the aid of a magnetoelectric machine magnet.

As was said earlier, the hypnoscope was a device assessing the existence and degree of hypnotic susceptibility which, according to Ochorowicz and contrary to Jean-Martin Charcot (1825 - 1893) and in general, to the "Paris school", had nothing in common with hysteria.

The hypnoscope was put on the subject's forefinger (fig. 3) for two minutes. If after this time there occurred in his finger such peculiar bodily sensations as tingling, pricking, swelling, stiffening and so on, a subject was put down as easily hypnotisable. Between 1880 and 1882 Ochorowicz examined for hypnotic susceptibility some 700

normal and neurotic subjects. Of this group 235 or about 33 per cent were susceptible. (In 1884, Charcot invited Ochorowicz to his neurological clinic at La Salpêtrière hospital in Paris to verify his claim that the hypnoscope could be used as a diagnostic tool in mental illness.

Ochorowicz passed the exam with excellence. Among 12 women hysterics presented to him he rightly identified 8 as non-susceptible and 4 as susceptible in a greater or lesser degree).

According to Ochorowicz, those who are susceptible to the hypnoscope are all susceptible to *ideoplasmy*. The term "ideoplasmy" (from the Greek *idea* for "idea" and *plasso* for "to form in the mind", "form a notion of a thing") was taken over by Ochorowicz from Durand de Cros (1825-1900), a French physiologist who advocated the idea of a psychical influence of thought on the human body.

The short time at my disposal does not permit me to discuss Ochorowicz's theory of ideoplasmy in detail. In what follows, I will give only a short account of it.

On the whole, ideoplasmy is a psychophysiological phenomenon consisting in the materialisation, or physical realisation of an idea of a certain organic state. Ochorowicz distinguishes three categories of the phenomenon in question: sensory, motor, and material or trophic. It is to this latter category that he attaches the greatest importance since it contains "the most astounding facts".

Normally, having been scalded we have a feeling of scalding, or have an idea of being scalded. Is the reverse possible, i.e., is there any possibility that the very idea, for instance, of scalding could produce a local inflammation and swelling in the body? - Ochorowicz goes on to ask. And he replies "Improbable as it may seem, that's the case". However, just because a material ideoplasmy is something real, it does not follow that every person in every state is subject to its effects. In order that this could be realised, two conditions, one psychological and the other physiological, must be fulfilled on the part of a subject, i.e., a tendency to *monoideism* and the *presence* of ideo-organic association respectively.

First, in order to produce physiological changes an idea must be separated from other ideas and become the dominant one.

This can be accomplished by suggesting such an idea overtly in the state of hypnosis. In *monoideism*, thus, there occurs a certain "narrowing of consciousness" upon the hypnotically suggested elements.

Second, as a matter of fact, what is operative in the production of organic changes is ideo-organic associations rather than a suggested dominant idea. According to Ochorowicz, apart from purely psychological -or, as he names them, "*ideo-ideic*" associations, there are the so-called ideo-organic associations, or associations of ideas with certain organic states mediated by vasomotor nerves. For instance, in the case of ideo-ideic associations a feeling of hunger (in the brain) is associated with an idea of the object (in the brain) which aroused that feeling.

Likewise, as is natural, an idea of hunger (in the brain) must be associated (through the mediation of vasomotor nerves) with a certain organic state of the stomach (out of the brain) which aroused that idea. That such a state arouses the corresponding idea, feeling, need, etc., is common knowledge. Who would have thought, however, that the reverse is the case, that is, that an idea might call to mind an organic state with which it had been associated? This has been revealed only by hypnotism. It turns out that a dominant idea suggested in the course of hypnosis evokes a given organic state with which it had been associated. As a matter of fact, this is the mechanism of material ideoplasia. On the one hand, an idea of change; on the other, an organic complex bringing about this change.

According to Ochorowicz, ideoplasia is a particular case of a universal law of nature, namely the *law of reversibility* (or reciprocity) as he calls it. He derived this law from his all-energetic theory of nature which he had advanced in an 1879 treatise on the 'philosophy of physics' *Sila jako ruch* (Force as motion). In general, the law of reversibility runs as follows: "If under given conditions a force A can produce a force B, then inversely, under other conditions force B can produce force A". The idea of this law occurred to Ochorowicz when he studied dynamoelectric machines and observed that if the electric current produced mechanical movements, then inversely, the latter could produce the former.

Nevertheless, Ochorowicz gave the reversibility law a very general expression that, as he was convinced, held true not only for the physical but also the mental. Without going into detail, let us make the meaning of Ochorowicz's law clearer by giving related examples. We begin with the mental sphere.

If a sensation produces a mental image, then inversely, a mental image can produce a sensation (as in hallucination).

If a disease can cause an emotion, then inversely, an emotion can cause a disease (e.g., psychosomatic disorder).

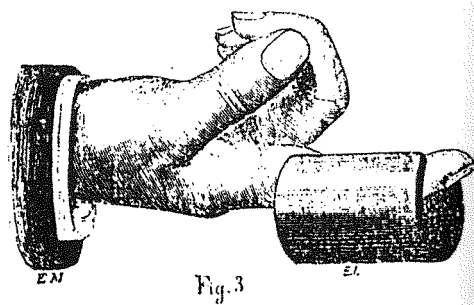
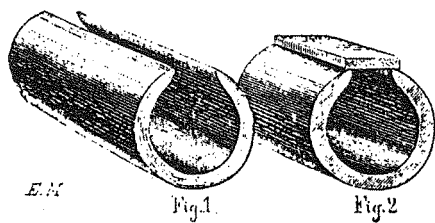
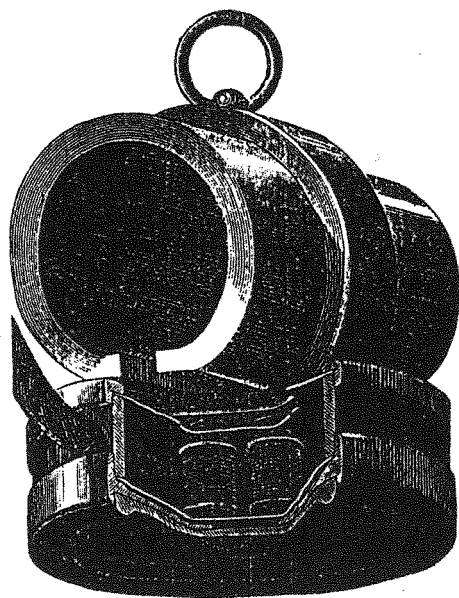
If paralysis of an arm produces an idea of paralysis, then inversely, the latter can cause paralysis (e.g., a hysterical paralysis).

The real importance Ochorowicz attaches to his law lies in that it makes it possible to form a priori judgments about probable results of mutual conversion of different kinds of energy.

Specifically, it enables us to foresee inverse transformation when only the unilateral one is known. In this manner Ochorowicz tried even to explain paranormal phenomena, like mediumism, to the effect that, provided under normal perception conditions sound produces an idea of sound, in abnormal mediumic conditions an idea of sound can produce sound, say, the sound of knocking at the door. What is more, during the last years of his life Ochorowicz went so far as to foresee by virtue of his reversibility principle the possibility of photographing not only a thought process but also... phantoms!

Ochorowicz finds also his law of great heuristic value in the domain of technological inventions. That a fire, and thereby heat, could be made by the mechanical movement, i.e., friction, was already known to primitive men. But that, inversely, heat could produce motion, became clear only in the 19th century when the steam engine was invented. In 1857, the French inventor Leon Scott, constructed the phonautograph, a device which transformed the spoken voice into the mechanical movements of a diaphragm (recorded as a wavy line on the smoked surface of a rotating cylinder). But it was only twenty years later that Thomas Alva Edison, having invented the phonograph, realised the converse change of mechanical movements into spoken voice. In a sense, the invention of a telephone should be considered as the realisation of the law of reversibility in that electric impulses into which sound waves were converted (in a transmitter, or a microphone) are converted back to the incident sounds (in a receiver). Now it is important to note that by virtue of this law Ochorowicz, as early as 1979, laid down the principle of the construction of a device for transmitting visual images over a distance (which he called the telephotoscope), namely: to convert light into electrical impulses and vice versa. Thus, one should rightly credit him with being a precursor of TV.

Ochorowicz's contributions to psychology lies in his research on hypnotism and mediumism. Besides, both of them belong, according to him, to the province of psychology. What is more, in carrying out research in this direction psychology has a great future ahead of it in that it may throw a new light on the problems of the processes of association, the ego (or self), and personality. In carrying out research into such related to hypnotism and mediumism phenomena as ideoplasmy and ideo-organic associations, being ruled in turn by the law of reversibility, "we are touching a line marking a border between mind and body".



In need of a new utopia? Considerations on the past and future of critical social science

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In 1967 students of my generation started the "Critical University", a self-organized counter-curriculum addressed to students, workers and pupils. With this project of a theoretical and practical critique of social science, the university, and society at large, we aimed at a radical social transformation toward individual and social autonomy. The scope of entries in the 1968 guide to courses and workshops comprised the "democratization of schools and universities" and a "Springer-press tribunal", "the future of Latin America" and "the task of sociology in a technocratic society". Though the "Critical University" did not last, the project of critique remained on the agenda of the New Left, in Berlin like elsewhere. A quarter of a century later, however, processes of social change speeded up dramatically, without our doing, and in utterly unforeseen directions: after the collapse of most communist régimes, the capitalist market economy is about to move East triumphantly. Is this the end of projects for a better society, of critical social theory, and a radical critique of social science? Or is there a need for a new utopia?

Surely, the current social changes raise more questions than I can hope to tackle in this paper. A (self)critical review of New Left radicalism, in terms of its impact upon social imagination as well as its limitations, remains a task for the future. Trying to cope with disappointment, I would like to at least touch upon some problems related to a self-reflected historiography of the discourse of radical critique in social science. I will outline the paradigm shifts of critical social theory - from production to power, from power to language -for the sake of approaching two questions: First, how did the vision of a societal alternative and of potential actors of a social transformation change in the course of changing paradigms? Second, what became of the role once assigned to critique in the project of a critical social science?

FASCINATION WITH SOCIAL REVOLUTION: APPROACHES FROM THE PARADIGM OF PRODUCTION

For West German students of the 1960s, the rediscovery of Marx and Marxist social theory, a theoretical tradition largely extinguished during Fascism contained the prospect of a practical alternative to what we perceived as a reemerging Fascist threat. Reading Marx in the context of the emigré left's linkage of capitalism and Fascism

made a social alternative imaginable and seemed to provide us with a key to social reality at large. Marx' notion of material production as the main territory of social struggle for a better future and core domain for the understanding of social phenomena opened up a new view of history and society.

Our naive reading aside, the historical-materialist paradigm of production provides a theoretical model for understanding the everyday world of human experience as a sociohistorical result of material human activities. The dual notion of production, as a technical labour process and at the same time reproduction of social relations, represents the given social objectivity as a product of earlier generations' activities. The dichotomic conceptual framework of Marx' social theory (from the dual character of labour and commodity to the antagonism of productive forces and social relations of production) allows us to distinguish conceptually between the objectifications of practical human relations with nature, on the one hand, and objectified intersubjective relations as the realm of possible critique and transformation, on the other. It thus provides historically immanent criteria for the rationality of critique and its practical aim of transformation. The possibility of such transformation is anchored in radical needs of the producers; if these historically formed needs cannot be satisfied under the existing relations of production, critique which makes the needs conscious and demands their fulfilment is rational. As was lucidly demonstrated by Markus (1986), the paradigm of production can be - and perhaps still ought to be - reformulated in such a way that obvious shortcomings of the Marxist model are avoided.

1. Visions of autonomy and solidarity

In the student movement's struggle for emancipation from authoritarian constraints in private and public life, alienating working conditions as well as the narrow confines of provincialism we sensed around us, visions of what we might achieve grew like magic. Individual and collective autonomy was the enthusiastic goal of theory and practice, uniting counter-curricula and happenings, attempts at the democratization of departments and city agitation. In those days, Socialism meant the promise of solidarity, cosmopolitan openness and a world to be shaped according to needs. As to the actors of a Socialist transformation, an alliance between students and workers in the metropolises and liberation movements in the Third World seemed to suggest itself so obviously that not much thought was given to the problematic concept of "revolutionary subject".

2. Critical Social Science

The theoretical aim as envisaged in the late 1960s was a critical social science which would provide an orientation in the process of a radical transformation of society. Taking over the model of Marx' Critique of Political Economy - a critique of classical economy and at the same time critical account of capitalist reality in the form of a revolutionary theory - we viewed the social sciences as a comprehensive body of systematically distorted social knowledge. Its radical critique was to uncover this distortion as well as its roots in the knowledge interests which had led to the fragmentation of the social sciences, and it was to restore the "true" body of societal knowledge. Attempts at approaching this task took on several forms depending on

which aspect of Marx' theory of culture was accentuated (Markus 1990).

In the early days of disgust with the prospect of joining the ranks of the "servants of power" (Baritz 1960), the concern of radical critique was with the social function of science. The polemical notion of ideology inspired critiques of social science which focused on the interests that were being served by particular theories, and thus demasked the assumed neutrality or impartial rationality of the knowledges in question (Blackburn 1972). Combining ideology critique with Marx' polemical notion of culture as a "superstructure", i.e. the notion of a necessary dependence of all political, legal, religious and cultural institutions and practices upon the economic structure and processes of economic change, attempts were made to establish a comprehensive link between the development of capitalism and the history of science and the social sciences. Historical critiques of sociology (Lefèvre 1971), social psychology (Nolte & Staeuble 1972; Strickland, Aboud & Gergen 1976), psychology (Jaeger & Staeuble 1978) and political science (Blanke, Jürgens & Kastendiek 1975) abounded and are here only randomly referred to. Studies in the sociology and political economy of science established an empirical link between capitalist and science development (Stölting 1974; Rose & Rose 1976) which ranked top radical at the time, even though it may nowadays be considered to be a mere empirical fact (Markus 1990).

For the sake of transforming social science it was, however, not sufficient to demonstrate its economic dependence and social function. Departing from the elaborate concept of ideology in Marx' critique of political economy, the social sciences could be viewed as a comprehensive body of systematically distorted, "reified" social knowledge. This reified knowledge imposed severe limits on social imagination, thus preventing visions of a possible better future. In order to regain the systematically repressed power of imagination, critique was to penetrate and reopen the systematic closures of bourgeois thought. The "positivist dispute" (Adorno et al. 1976) had been a prelude to unearthing dialectical modes of thinking from a knowledge interest in human emancipation (Habermas 1968/1977). An updating of critical theory would require a closer analysis of the nature of monopoly capitalism and the welfare state as well as attempts to recover the radical knowledge and visions that had been excluded from the trail of social science. In the long run, however, the fiercely contested question of a proper perspective of socialist transformation and the project of a critical social science went out of tune. In spite of being designed as groundwork for a practical social transformation, social class and consciousness analysis, historical critiques of social science and "marxist" or "critical" remakings of sociology and psychology were largely to remain ends in themselves. Increasingly detached from social movements, the critics of reifying specialization in social science became themselves specialists.

REJECTING THE TYRANNY OF TOTALIZATION: APPROACHES FROM THE PARADIGM OF POWER

After the failure of the 1968 strategy of mass mobilization, sectarian party Marxisms and an increasingly stale marxology slowed down the quest for totalizing

theories. Also, the "long march through the institutions" showed up the lack of a Marxist political theory. Foucault's theory of power suggested itself as a valid response to the integrative potential of the welfare state. The model of class struggle was supplemented with and sometimes replaced by a strategic notion of power being divided among various parties and social agencies, permanently changing hands between social actors. The program of a microphysics of power inspired the analysis of particular institutional practices and techniques which rendered humans productive as well as docile, and at the same time provided an antiscientistic rhetoric with regard to the human science knowledges generated by such power practices.

1. The riddle of subversion

Insofar as one can speak of a practical perspective involved in Foucault's social theory, it is not the mobilization of energies toward a desirable societal alternative, but subversive intervention in "currently intolerable" affairs. With regard to the rhetoric of subversion, however, the prison as an elementary institutional model for an understanding of the disciplinary processes which have extended from the subjection of individual bodies to the societal body of the population, entailed an ambiguous message. Subversive intervention might take the form of lending a voice to the silenced prisoners, and thus encourage their self-acting. But once society as a whole is regarded as a prison, nobody would be left to intervene. On a systematic level, Foucault's theory of power amounted to the same problem. His grounding of the strategic notion of power in a totalized and anonymized notion made it difficult to understand why one should act subversively at all.

2. Archeology and genealogy of the human sciences

Foucault did not use the term "critique" when he outlined the program of an archeology of knowledge and a complementary genealogical historiography of the human sciences. This program aimed at both, an unmasking of the narrowing of reason by the discursive rules of exclusion, and an analysis of the power practices which brought about the particular discourses. Still, a practical motive for the quest for knowledge was hardly discernible.

For a historiography of the human and social sciences, the genealogical approach offered nevertheless imaginative ways of linking the knowledge-generating institutional practices with the contexts in which the knowledge operates. For instance, it could be demonstrated how administrative, regulatory and curative problems in schools, factories, prisons, psychiatric hospitals gave rise to psychological knowledge and techniques which made individuals classifiable and calculable (Rose 1985). Psychological techniques and languages have in turn shaped the ways of describing, evaluating and managing the conduct of others and oneself, in public and private. A wide field is thus provided for an analysis of the relationship between power and subjectivity which is characteristic of liberal democracies (Rose 1991).

COMMUNICATIVE RATIONALITY AND THE IRREDUCIBLE PLURALITY OF LANGUAGE GAMES: APPROACHES FROM THE PARADIGM OF LANGUAGE

The "linguistic turn" in social theory has generated as diverse consequences as Habermas' fundamental reformulation of critical theory and the postmodernist deconstruction of modernity's metanarratives. In contrast to the paradigm of production, the paradigm of language focuses on a view of the social world as a plurality of language games/forms of life. As Markus (1986) has at length analyzed the implications of this view, I mention but the most relevant aspects of the project of a critical theory: Understood as codified linguistic rules, social relations cannot be changed consciously and as a totality (Markus 1986, 39). Insofar as the social model implied in the paradigm of language is one of direct participation, it leaves no place for critical theory. The social scientist functions as an interpreter at the interface of language games, in which role he might at best contribute to the mutual understanding of communal traditions. Nevertheless, at least the Habermasian use of the paradigm of language extends to the claim of a foundation of critical theory.

1. The ideal speech community

For Habermas, the very possibility of critical theory was at stake when he began to supplement the paradigm of production with the paradigm of language. Due to his initially reductive misreading of the notion of production as instrumental labour, the phenomena of interaction needed a conceptualization of their own. In the course of this conceptualization, however, he arrived at a more radical reworking of critical theory. According to his diagnosis of late capitalism, social class antagonism was largely pacified and a "revolutionary subject" thus absent. At the same time, the productive forces could no longer be viewed as a critical potential, because technology and science themselves provided a legitimizing basis (Habermas 1968a). Marx' paradigm of production could, therefore, no longer fulfil the function of explaining and interpreting the present from the standpoint of radical demands for and the viability of a better future. How else could the link between emancipatory interests and transformative goals be justified?

In his attempt at a social epistemology, Habermas (1968 b/1977) outlined the anthropological basis of the knowledge interests in technical control and communicative interaction, to which he added an emancipatory interest in overcoming overt and internalized social domination. From a Freudian model of nonconstrained dialogue which did not provide a satisfactory answer he moved on to a transcendental analysis of the norms which constitute an ideal speech community. Thus he tried to establish a notion of "communicative rationality" which would provide a philosophical foundation for the radical idea of a participatory democracy as well as a criterion of social progress.

Among the objections raised against Habermas solution of grounding an emancipatory potential in the communicative faculties of humankind, Markus' (1986)

is especially convincing in pointing out that the privileging of the Western form of rationality is irreconcilable with the notion of a plurality of human cultures and the principle of their autonomy, and in acknowledging a plurality of "critical subjects" who would have to engage in a process of working out desirable and viable goals of radical change.

2. Irreducible plurality?

For postmodern philosophy and social theory, there seems to be no need for a project of conscious social transformation; social conflicts and the misery in the world are not ascribed to objective causes, but to the fact that humans still hold on to wrong meanings and values. Lyotard's (1984) dismissal of the modern meta-narratives, the idea of social emancipation, solidarity and self-determined activity suggests that there is nothing lost and nothing to deplore. The anti-universalistic attitude against the traditional as well as marxist view of social reality may be seen as a mimetic response to what appears to mark an end of the modern process of actual and projected universalization. Still, Baudrillard's (1975, 1978) parodies on symbolic reproduction, the terror of the code, the revolt of the signs, and the implosion of power use the rhetoric of radicalism, and thus seem to suggest radical revolt or refusal. A heterogeneous group of potential actors of revolt - the young, ethnic minorities, the old, the *déclassés* - is posited vis-à-vis the despised "masses", and declared to be exempt of the tyranny of the code if only they want to be. Rightist and leftist revolt become undistinguishable.

For postmodern social theorists it seems to matter little that the features of postmodernity apply only to part of the world. Their emphasis on irreducible differences can and has been read as a claim on the privileged position (Bauman 1989). Once, in case of conflict, the differences turn out to be irreconcilable, postmodernism shows its pre-Hobbesian face, leaving civil war as ultimate arbiter.

IN NEED OF A NEW UTOPIA?

Among recent theories of the postmodern condition, Harvey's (1989) offers the most coherent attempt at explaining the cultural and psychohistorical phenomena of space and time relations in terms of fundamental changes in the mode of capitalist accumulation. For the purpose of answering my initial question, I prefer Bauman's suggestion of a radical review of our conceptualizations of the global social space. There seem to be sufficient reasons to take the postmodern dismissal of universal norms and the strong emphasis on irreducible plurality as a symptom rather than as a fact to be accepted. So far, traditional as well as Marxist and Postmarxist notions of international relations have been modelled according to the social realities of the nation-states. The plurality of non-Western cultures has thus been reduced to a mere "environment, a territory for action but not a source of action" (Bauman 1989, 57). This is, however, no longer in unison with the multiplicity of relatively autonomous and yet mutually interdependent agents. The social space populated by these agents cannot be conceptualized in terms of the classic inner-societal space, with its universally binding authority, unified codes of moral and behavioural norms etc. Its

conceptualization requires full acknowledgement of the differences brought about by both, local traditions/visions and the constraints imposed by the dynamics of the world market. The practical task of reconciling the heterogeneous plurality of agents on a global scale would presuppose a minimum of reducible differences concerning the conditions of coexistence. For this minimum to be worked out among the agents, mutually acceptable conceptualizations of the differences could be helpful. It would seem no small contribution if social science cleared the ground for such conceptualizations by working through its own constraints in obsolete views.

There is, I think, no need for substituting the social utopia of modernity, i.e. the project of individual and collective autonomy. But there seems to be an intense need for reconsidering the forms which this project could take on a global scale. First steps in this direction could well consist in a critique of the categories in which we conceive of social reality, including a reflection on their sociohistorical embeddedness.

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The generation of 1886 in contemporary psychology

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As the Spanish philosopher Ortega y Gasset once pointed out, the main category of history is "eminence", all varieties of History always revolve around this subject. Some people's actions and creations serve to channel, positively or negatively, the developments which others may carry out. There is a full historical dialectic, and in it what is "important" is that which is efficient, functional, within the interaction and social communication matrix which makes up scientific organization. In such a radical search, we only become fully conscious of the problems involved in establishing the eminence of previous works and authors when we observe the low level of convergence between different estimation scales or surveys attempting to determine the "great undertakings" of a period or country. In our search for adequate measuring techniques we reach bibliometric methodology, a procedure applied to written manifestations characterizing any science. This technique may be integrated in a more comprehensive approach to explain historical evolution.

The range of useful indicators for measuring the impact of scientific production is very wide. However, in comparison with the traditional way of facing the topic of scientific quality -based on a retrospective judge of the past- present historiographic tendencies point towards more objective and social criteria based on the analysis of references. There is plenty of evidence that psychologists judged to be eminent are those with greater number of references in scientific literature (Carpintero & Tortosa, 1990). This is a social conception of "eminence" defined, within what Carpintero calls the "context of communication", as the degree of definition and attention given to an author's contribution by the scientific community.

The purpose of this paper is to determine the impact on contemporary scientific psychology of "eminent authors" belonging to the "generation of 1886". The study population were researchers included in the Scale proposed by Annin-Boring-Watson (1968); that is, scientists of acknowledged eminence by expert judges in history. Their impact will be evaluated, in the first place, in terms of the number of members of this generation situated amongst those with greater impact for the period 1887-1945 in four American psychology journals (Carpintero, Peiró & Tortosa, 1988); a period coinciding to a large extent with the authors' productive stage. And, in second place, their impact on a multidisciplinary repertoire of references will be measured, in this case, the Social Sciences Citation Index (SSCI) for the period 1966-1985.

Generations and psychology

Our study's working model is the variable "generation", understood according to the historical method of generations (Ortega, 1958; Marías, 1967). Generation refers to a group of individuals differentially characterized according to certain features, acquired through social interaction, which are relevant for understanding those individual behaviours and historical and social phenomena in which they intervene (Carpintero, 1978). Generational groups do not simply succeed one another, but overlap and connect with each other, thus coinciding in the historical and social task at a different level. The movement of history is explained from the simultaneous coincidence of several generations, for each generation has a vital sensibility supporting those attractions and rejections which drive some to innovate and others to be conservative.

We accept, together with Marías (1967), that every generation covers a 15 year period and is called by its middle year, with 7 years before and 7 after. In order to find out which is the "decisive generation" of a given historical field, its eponym is located -the person best representing the essential characters of the period- and the year when this person was thirty years old, or was born, is taken as the center of that generation; allowing for a generational ladder which acts as a net through which historical reality may be seen. Marías considers 1856 to be the central date from which this generational series may be fixed, which would logically continue forward with 1871, 1886..., and backward with 1841, 1826... (Marías, 1970). This date is acceptable to psychology, for in the fifties Helmholtz, Wundt, Dilthey, Galton, Spencer, or Sechenov, are either approaching or have just turned thirty; and, in addition, Freud, Binet, Kraepelin, Ebbinghaus, Husserl, or Dewey, are born around this time. All generations appearing in a historical period are considered to be current generations, in the sense that they co-exist, but only two of them are strictly active; the others are either not yet active or have stopped being so.

The two generations immediately prior to Lewin's constitute the most immediate context for the generation being considered. To the 1856 generation (those born between 1849 and 1863) belong the representatives of the second generation of American psychologists -Dewey, Cattell, Baldwin-; physiologists such as Loeb and Sherrington; the reflexologists Pavlov and Bechterev; phenomenologists such as Husserl; histologists such as Ramón y Cajal; psychoanalists and psychiatrists of the stature of Freud, Janet, Kraepelin, or Bleuler; representatives of experimental psychology of higher processes such as Ebbinghaus, Külpe, G.E. Müller or Binet, together with Ch. Spearman, the initiator of correlational tradition.

Authors belonging to the generation of 1871 (those born between 1864 and 1878) largely represent the continuation of the channels opened by Wundt, James, Freud and Pavlov, with the exception of J.B. Watson, born at the very end of the generation (1878). They carry the mark of American psychology, and they include the initiators of the first psychological Schools: structuralism -Titchener-, functionalism -Claparède, Angell and Carr-, dynamic psychology -Woodworth-, hormic psychology -McDougall-

, connectionism -Thorndike-, personology -Stern-; individual psychology -Adler-, and depth psychology -Jung-; together with them, comparative psychologists -Yerkes-, industrial psychologists -Dill Scott-, educational psychologists -Goddard and Terman-, and psychophysicologists -Cannon-.

The generation of 1886 (born between 1879 and 1893) is the last of the glorious century of science. As evidenced by their scientific contributions, members of this generation will make their presence in research felt from the 1910s up to the 50s. It is a generation of eminent men for psychology, with a predominance of psychologists, nearly 75% of these are identified as such by Annin-Boring-Watson (1968). Next to them in decreasing order there are: psychoanalysts, psychiatrists, physiologists and biologists, social scientists and natural scientists. Their average level of "eminence" is high, two fifths are placed above "level 20", and 15 of them are in the two highest ranks. As to their country of origin, three fifths come from Anglo-Saxon countries (51 from the USA and 13 from Great Britain), the remainder come from continental Europe: Germany standing out with 25 eminent authors. Central-European scientists, in particular German speaking scientists, are marked by an unavoidable socio-political phenomenon: the rise of Nazism to power. In many cases scientists were forced to seek exile in Anglo-Saxon countries, thus, practically destroying a brilliant research reality (Mandler & Mandler, 1969; Coser, 1983; Geuter, 1987).

Here we find the greatest representatives of the Gestalt School -Wertheimer, Koffka and Köhler, together with the field psychologist Lewin-; behaviorists -Hunter and Weiss-; the main representatives of neobehaviorism -Guthrie, Tolman and Hull-; Marxist psychologists -Kornilov and Rubinstein-; representatives of new tendencies bringing light to the psychoanalytic perspective and psychiatrists -Klein, Rank, Horney, Sachs, Alexander, Sullivan-, among them the representative of existential analysis -Binswanger-. It is also the generation of some of the representatives of the typological outlook on personality -Jaensch, Kretschmer and Spranger-, developmental psychologists such as Bühler, Goodenough, Werner, Wallon and Gesell; educational psychologists -L. Hollingworth, English, Valentine, Wheeler-; industrial psychologists -Bingham, H. Hollingworth, Moede, Paterson, Strong-; representatives of the factorial analysis of intelligence -Brown, Thompson, Kelley-; psychobiologists such as Piéron and Lashley; physicians such as the operationalist Bridgman; statisticians -Fisher-, and representatives of psychology of tests, both projective -Rorschach- and psychometric -Thurstone-. Together with them, linguists such as Sapir, or cultural anthropologists such as Benedict, Linton and Malinowski, who studied the relationship between personality and cultural forms.

Impact of authors of the generation of 1886 in Four American Psychology Journals

References appearing in four of the most important and oldest psychology journals in the USA were analyzed. That is, *American Journal of Psychology* (1887-), founded with the object of offering laboratory-based experimental psychology;

Psychological Review (1894-) -with a strong theoretical orientation, but with experimental, review, and conceptual papers, together with others on methodological and historical issues, and discussions on basic psychological problems; *Psychological Bulletin* (1904-), offering critical reviews in certain fields (especially in comparative and social psychology, tests, educational and genetic psychology, psychopathology, sensory psychology and psychophysiology), together with detailed information on bibliographic material and professional issues; and *Journal of Experimental Psychology* (1916-), centered on "hard" experimental research together with research on instrumentation and apparatus, methodology, and measurement. The analysis therefore covers nearly half of the history of the "new" scientific psychology in the USA.

Members of this generation were beyond all doubt very influential. However, our analysis of the psychological information published, aims at offering a more specific and detailed knowledge of this influence. Starting with the 20 most cited authors in each journal, a final group of 57 authors -out of the 80 possible authors if none was repeated- was established (Table 1). Some authors appear frequently cited in two or more journals. However, the impact of the majority is restricted to one or two journals -a usual phenomenon in contemporary science (Zalbidea & al., 1989). It seems as though in psychology there is no well established group of names that may be taken as the "core" of the discipline; there is instead a variety of sources of reference not totally independent of each other, which coexist according to the topic or national tradition on which work is carried out.

Names obtained from the analysis of references of papers contained in the four journal publications, certainly include the leaders of the main psychological schools in the way these are usually presented in the historical textbooks. There is, of course, a clear over-representation of American psychology. However, the great lines of influence exerted by European psychologists also stand out: Wundt's experimental or individual psychology (but not social or ethnic), and its American variety, Titchener's structuralism, with clear differences between them (Tortosa, 1989); experimental psychology of the senses, as well as some of the authors responsible for experimental or empirical developments in the study of higher processes: the forefront of the Gestalt School and its field psychology variety; classic authors in the measurement of mental skills in France and Great Britain; "orthodox" psychoanalysis; the Russian School of conditioning; and the more purpose oriented hormic psychology of McDougall's. Among American authors, the great theoretical backbone of its psychology: pragmatism, functionalism, connectionism, behaviorism, dynamic psychology and neobehaviorism in its cognitive, deductive and radical variants. In addition, a large presence of learning theoreticians and experimentalists with an interest in education; developmental psychologists, in some cases interested in the measurement of mental activity and psychometrics; experimentalists centered in sensory-perceptual processes; psychophysiolgists and neurophysiolgists close to topics on learning and the application of their research to the industrial field. The generation of 1886 remains reflected in the tension between representatives of the European Gestalt tradition and

the neobehaviorist tradition centered on the theoretical and experimental study of learning, and the European and American psychometric tradition centered in the field of intelligence tests and psychodiagnosis; in general, between “pure” and “applied” psychologists.

Focusing on their most cited works, the presence of papers on the following stands out: psychophysics, the effect of the intake of stimulants on task performance, the applicability of the principles of conditioning, and the factorial focus on intelligence. Among the books, an important group of manuals representative of the Gestalt School stands out: *The Growth of the Mind* and *Principles of Gestalt Psychology* by Koffka; *Gestalt Psychology* by Köhler; and with them Lewin's *Dynamic Theory of Personality* (1935) which is actually but a collection of 8 papers; only 2 of them had not previously been published and another 2 had already been published in English (“The Conflict between Aristotelian and Galileian modes of thought in contemporary psychology” and “Environmental forces in child behavior and development”). On the other hand, there is another block of works linked to the behaviorist orientation: the chapter on “learning” written by Hunter for *Foundations of Experimental Psychology* (1929), edited by Murchison, and works representative of Tolman's Gestaltic, purposive and molar behaviorism (*Purposive Behavior in Animals and Men*) as well as Hull's deductive and formal behaviorism (*Principles of Behavior*). Lashley is always difficult to place, since he begins very near one end - Watson's behaviorism- and finishes near the other -Gestalt psychology-; however, Lashley's most cited work covers the most elaborate product of his research on the cerebral location of functions, a study which diverted interest towards non-localized attitudes in concluding that ultimately cerebral dynamics is not a question of links or mere paths, but a question of “rhythms and sequences of activities, of excitation guidelines” -in other words, of configuration or global structuring of action. The remaining works cover more applied aspects: childhood developmental and educational issues (Gesell and Gates), and a number of works on psychological measurement: Pintner's *Intelligence Testing* and Well's *Mental Tests in Clinical Practice*. Finally, attention must be drawn to Boring's *History*, a widely cited manual then and now in spite of the criticism it receives (Table 2).

Impact of authors of the generation of 1886 in the SSCI (1966-1985)

Overall, members of the generation of 1886 during this period have received a total of 59.123 references, with an average of 563 references per author, much above the average of the list and even above the average obtained in the SSCI by authors integrating the Annin-Boring-Watson scale (Tortosa & al., 1983, 1989); this shows these authors' strong influence in the social sciences at present. However, references are not randomly distributed, there is a pyramid shaped distribution with a small central group of authors very frequently cited (e.g., Lewin, 4.332 references), and a wide base of authors receiving few references (e.g., Klemm, 7 references, or Jaensch, 8) (Tables 3 and 4).

Five authors on their own receive 29, 15% of references, with K. Lewin clearly standing out, and cited mainly in the fields of social psychology and personality; R. Fisher, essential to contemporary analytical statistics, coined terms now very much in use (e.g., null hypothesis, degree of liberty...) and developed the analysis of variance technique together with several parametric and non-parametric techniques; Thurstone, connected to the development of factor analysis techniques and psychometrics in the USA, together with the construction of the first attitude scales and with theories on primary mental skills; M. Klein, probably the best-known child psychoanalyst, modified some Freudian ideas on infancy and development including the conception of the "Electra complex" and the development of female sexuality; Hull offered a formalized system of behavior and a psychological model of great impact resulting from an extensive and rigorous experimental programme which had great repercussion. This authors' motivational approach has without doubt been of fundamental importance to this discipline.

Another group of five authors follows, explaining another 19% of references, with an average near 2200 references: F. Alexander, a neofreudian representative of the Chicago School; H. Werner, a developmental psychologist who in spite of proposing an important field theory on perception, incorporating the motor aspect of perception, has been remembered primarily for the 1926 English version of his classical work; Harry Stack Sullivan, a psychiatrist who based an original approach to psychiatric problems, especially concerning schizophrenia, on a socio-psychological focus of personality; Malinowski, the cultural anthropologist who considered culture to be a system of collective habits serving human needs, and carried out important field work in the Trobriand Islands; and K. Lashley, a psychobiologist especially remembered for his studies on the cerebral localization of functions.

Another heterogeneous group of 10 scientists explains a further 20%, thus approaching the 70% of total references received by the group of "eminent" authors belonging to the generation of 1886. A. Gesell, developmental psychologist who offered, together with a relevant theory of development, some interesting observational scales for children of preschool age. K. Horney, a neofreudian who moved near Adler's approach and turned to the concept of basic anxiety in order to explain the development of personality. The three co-founders of the Gestalt School: Köhler, Wertheimer and Koffka. One of the main representatives of neobehaviorism: E. Ch. Tolman, precisely the author most in tune with the Gestaltists' approach. Edward Sapir, an anthropologist and linguist who, advocating a close relation between anthropology, sociology and psychoanalysis, exerted a strong influence over Sullivan. Linton, a cultural anthropologist who, together with Sapir, considers culture to be the total sum of patterns of behavior, attitudes and values shared by members of a specific society. Rubin, a Danish phenomenologist very close to the Gestaltist approach with his phenomenological analysis of figure-background relations, and his demonstration of the role of attention to perception. Finally, one industrial psychologist, Strong, especially interested in the measurement of vocational interests.

A final point to consider refers to the sources from which references were extracted, with the purpose of determining their main areas of impact in psychology, avoiding the masking effect which would result from the inclusion of a vast set of journals belonging to several social sciences. The individualized analysis of journals in which these authors have greatest impact shows important differences between them. For example, while psychoanalysts are mainly cited in clinical and psychotherapy journals, especially with a psychoanalytical orientation, Fisher or Thurstone are cited in publications of a general-methodological focus, mathematical psychology and psychological measurement; Hull and Tolman are cited in animal and human learning journals; Lashley and Gelb in psychobiology and/or clinical journals; Wertheimer in journals dealing with perception; Sapir in periodical publications centered on psycholinguistics; Lewin in social psychology and organizational journals; and Werner in developmental psychology. The study of the sources from which references were extracted may therefore allow us to detect the main areas of influence of authors, thus complementing the different views on their impact (Tortosa & al., 1989).

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TABLE 1: Most cited authors in four American psychology journals (1887-1945)

AJP		PR		PB		JEP	
Author	Refs.	Author	Refs.	Authors	Refs.	Author	Refs.
Wundt	593	Thorndike	191	Thorndike	228	*Hull	197
Titchener	547	Titchener	186	Watson	157	Dodge	175
Washburn	261	*Tolman	180	Gates	137	Hilgard	172
James	156	James	168	*Thurstone	137	Thorndike	142
*Boring	153	Watson	161	*Pitner	136	McGeoch	131
Bentley	137	*Hull	151	Yerkes	133	Hovland	127
Hall	129	McDougall	150	McGeoch	126	Freeman	126
*Köhler	118	Wundt	125	*Lashley	124	Travis	113
*Fernberger	110	Baldwin	125	Peterson	124	*Cason	101
*Dallenbach	108	*Lashley	120	*Hollingworth	123	Razran	95
*Koffka	107	*Köhler	106	*Hunter	120	Ferree	93
Ebbinghaus	105	*Lewin	106	*Hull	116	Pavlov	84
Stumpf	97	Woodworth	102	Dunlap	113	Woodworth	80
Binet	94	*Koffka	101	*Wells	110	Darrow	75
Thorndike	91	*Boring	96	Lehman	106	Jasper	74
Helmholtz	90	Dodge	90	Titchener	106	Bills	74
Calkins	82	Spearman	84	*Gessell	104	*Tolman	72
Ward	81	Carr	75	*Boring	102	*Lashley	71
Freud	78	Dewey	75	Terman	100	*Fernberger	63
Külpe	78	Dunlap	75	Pavlov	99	Skinner	63

Scientists with an asterisk (*) belong to the Generation of 1886

TABLE 2: Most cited works by authors of the generation of 1886

Boring, E.G.	A History of Experimental Psychology
Cason, H.	The Conditioned Eyelid Reaction
Dallenbach, K.	Attribute vs. Cognitive Clearness
Fernberger, S.	An Introspective Analysis of the Process of Comparing
Gates	Psychology for Students of Education
Gessell, A.	The Mental Growth of the Pre-School Child
Hollingworth, H.	The Influence of Caffeine on Mental and Motor Efficiency
Hull, C.L.	Principles of Behavior
Hunter, W. S.	Learning: II. Experimental Studies of Learning
Koffka, K.	The Growth of the Mind
Koffka, K.	Principles of Gestalt Psychology
Köhler, W.	Gestalt Psychology
Lashley, K.S.	Brain Mechanisms and intelligence
Lewin, K.	A Dynamic Theory of Personality
Pintner, R.	Intelligence Testing
Thurstone, L.L.	The Vectors of Mind
Tolman, E.C.	Purposive Behavior in Animals and Men
Wells, F. L.	Mental Tests in Clinical Practice

TABLE 3: Authors of the generation of 1886 most frequently cited in SSCI

Name	Refs.	Rank.	Prof.	Country	Name	Refs.	Rank.	Prof.	Country
K. Lewin	4332	26	Ps	Ge	R. Fisher	3692	20	CN	GB
L. Thurstone	3297	27	Ps	USA	M. Klein	3220	17	Pc	Ge
C. Hull	2695	27	Ps	USA	H. Werner	2295	24	Ps	Ge
H.S. Sullivan	2270	23	MP	USA	B. Malinowski	2256	22	CS	GB
F. Alexander	224	18	Pc	Ge	K. Lashley	2064	27	Ps	USA
A. Gesell	1839	25	Ps	USA	K. Horney	1604	23	Pc	Ge
W. Köhler	1588	27	Ps	Ge	E. Tolman	1535	27	Ps	USA
E. Sapir	1396	19	CS	USA	M. Wertheimer	1193	27	Ps	USA
R. Linton	948	19	CS	USA	K. Koffka	831	27	Ps	Ge
E. Rubin	802	27	Ps	Den	F. Fromm	767	12	Pc	Ge

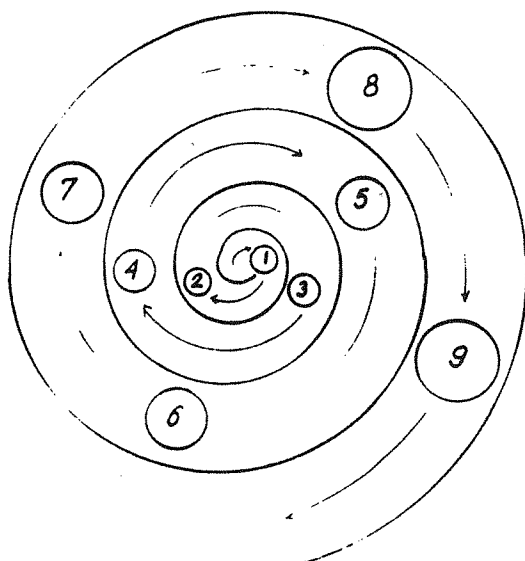
TABLE 4: *Works most frequently cited in the SSCI (1966-1985) by authors of the generation of 1886*

Lewin	Field Theory in Social Science (1951)
Fisher	Statistical Methods and Scientific Inference (1959)
Thurstone	Multiple-factor Analysis (1947)
Klein	The Psychoanalysis of Children (1932)
Hull	Principles of Behavior (1943)
Alexander	Psychosomatic Medicine: Its Principles and Applications (1950)
Werner	Comparative Psychology of Mental Development (1940)
Sullivan	The Interpersonal Theory of Psychiatry (1953)
Malinowski	Magic, Science, and Religion (1939)
Lashley	Brain Mechanisms and Intelligence (1929)
Gesell	Developmental Diagnoses: Normal and Abnormal Child Development (1941)
Horney	Neurosis and Human Growth: The Struggle toward Self-Realization (1950)
Köhler	Gestalt Psychology (1929)
Tolman	Purposive Behavior in Animals and Men (1932)
Sapir	Language, an Introduction to the Study of Speech (1921)
Wertheimer	Productive Thinking (1945)
Linton	The Study of Man (1936)
Koffka	Principles of Gestalt Psychology (1935)
Rubin	Synsoplevede figurer. In D. Beardslee y M. Wertheimer, Readings in Perception (1915/1958)

Claude Bernard's "expériment invoqué" in the behavioural sciences

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ABSTRACT



The most important steps in Claude Bernard's "Expériment invoqué" in the history of behavioural sciences are, in our opinion, the following:

- 1. Incidental records of wild-man cases - which Claude Bernard was also concerned with.*
- 2. Scientific concern with the social influence upon these cases.*
- 3. Observing them for years.*
- 4. Notifying and presenting them as an "expériment invoqué" situation (in Claude Bernard's view).*
- 5. The enlarging of the concept of "expériment invoqué" when studying cases of monozygotic twins.*
- 6. The assessment of one independent variable "expériment invoqué" value in scientific research.*
- 7. The development of the "expériment invoqué" method in modern psychology.*
- 8. The prevailing of "expériment invoqué" as the proper method in social sciences.*
- 9. The requirements of the "expériment invoqué" method for analysing complex social phenomena. The possibility to pattern catastrophic social phenomena in order to prepare psychological assistance in preserving human beings.*

1. We can find a description of method, especially of those methods that cannot be induced by the researcher for deontological or practical reasons, as well as the concept of "expériment invoqué", by referring to the works of the French scholar Claude Bernard (1813-1878). He was the first among 19th century anthropologists to draw a clear distinction between two methods which he termed as "expériment provoqué" and "expériment invoqué".

Both in his *Introduction à l'étude de la médecine expérimentale* (Paris, 1865) and his *Discours de réception à l'Académie Française* (delivered in May, 1869), Claude Bernard offered several details concerning his method which allow the researcher to study uncommon behaviours without having to induce them himself.

In support of the method called "expériment invoqué", Bernard resorted to natural phenomena or, to use his own words, to "naturally prepared" facts, "happening accidentally in people's lives", without any interference on the part of the researcher. They include congenital diseases, cranial wounds, monozygotic twins, mentally and morally retarded subjects, all quoted from doctor J. Itard's therapeutical practice, which was widely known at that time.

2. Let us examine the evolution of Claude Bernard's methodological conception towards the idea of exposing human facts by "expérience invoqué". We cannot proceed on this path without a thorough knowledge and understanding of the historical context and the level reached by science in the middle of the 19th century.

As is well known, all teaching, therapeutical and research activities carried out by this famous physician and physiologist were oriented towards the creation of an experimental medicine. Cl. Bernard established all the basic principles of an experimental scientific research in the field of pathology, human psychology and therapeutics of those times. The experimental spirit, he said, should preside over any knowledge that pretends to be scientific. As a consequence "the fundamental research method is produced in the laboratory, being checked and controlled all the time, by revealing the causes that produced it and orienting it towards the aim in view"(pp. 33-34).

Using strong logical arguments, Bernard prescribed all the methodical and procedural requirements that have to be met by experiments performed on human beings.

But it should be observed that he was formulating these requirements in a context where researchers opposed even experiments performed on animals, while vivisection, along with physiological tests done in the laboratories were the subjects of violent dispute. The church protested against the experiments included in the curing of the ill, and scientists were also confronting with a series of prejudices and "moral interdictions". In his *Discours à l'Académie...*, Bernard remarked that there was still an old-fashioned tendency to classify sciences into such fussy categories as: natural or "purely observing" sciences, such as astronomy; experimental sciences, namely chemistry and physics; and anthropological sciences, there being no communication or mutual support among them" (p. 78).

In Claude Bernard's methodological conception, experiments performed on human beings were meant to establish the causes of normal and/or pathological

behaviour, special attention being paid to the immediate causes generating the phenomenon. This restriction was imposed on scientific investigation by the religious beliefs of the time, which forbade scientists to question the *primary cause* of the phenomena specific to human behaviour.

Scientists had to compromise and adopt strategies to cope with these restrictions: 1) Operating a clear distinction between *primary determinism*, or the spiritual cause of human behaviour "accumulated along the times", as he put it, and *close determinism* resulting from inducing the subject's immediate reactions during the experiment. It is only this *immediate cause* that should become the object of research; 2) Claude Bernard systematically reduced and attenuated the shock produced by "experimental medicine" on the medical opinions of his time. His arguments were generally valid and consequential, and he underlined the differences and the similarities between *the method of observation*, generally accepted in medicine, and *the method of the experiment* organised in the laboratory. "Scientific observation, he said, always implies experimental judgement, provocative argument.

Claude Bernard often insisted on the complementarity of the two methods and on the need to combine them in any scientific analysis of human behaviour; he integrated the concept of *experiment* in the wider notion of *human experience*. These concepts occur interchangeably in his works, but this distinction stems from the linguistic peculiarities of French. There is an intentional *mediation*, an effort to smooth things over whenever Bernard refers to the medical experiences involved in his therapeutics. Finally, he described the specific method used in dealing with *unusual cases*, called "expériment invoqué". This method can be applied to facts "caused by nature", that appear accidentally, without being influenced by researchers. But these real facts, if studied systematically, can contribute to the scientific understanding of the human being. This was one of his strongest "scientific arguments" for calming critical spirits. It revealed that "nature" itself, *fate* or *god* perform experiments on human beings that are worth studying. Bernard mentions that one can use as an invoked experience an induced experiment carried out by another researcher.

The author concludes that, at the most general and abstract level, "it is the researcher who *invoked* and *provoked* the facts, in specified conditions, in order to reveal information, that is 'experience'"(p.36).

Bernard's works contain few details concerning the effective use of his method. His methodological conception naturally refers to the medical disciplines he was interested in. He mentions some fundamental requirements in using invoked experiments: 1) to compare unusual with "normal", everyday facts; 2) to check, by means of a "rational scheme", the experimental idea with which the researcher approached the case; 3) to explore as thoroughly as possible, resorting to the help offered by specialists in related fields, which amounts to examining phenomena from many points of view. Bernard quotes an example taken from his own practice, describing how he checked the idea of moral and intellectual retardation in human beings by resorting to J. Itard's experiences (1842). The progress of human knowledge

seems to include, in Bernard's view, two main stages: at first *historical*, and then *scientific*, both existing in his time; each stage includes in its turn three steps (feeling, provoked or invoked observation and scientific knowledge and intervention) (1865)

3. The human beings who developed in extreme environments and evinced abnormal reactions and asocial behaviour had long been considered an open seri by physicians, biologists, anthropologists and even philosophers. Such individuals were a natural challenge to Bernard. In what follows, we shall describe some of these cases, mainly those that have been constantly appearing in the works of psychologists. Cases of individuals belonging to the human species who turned "wild", living outside society, have been dscribed by the biologist C.Linne (1758) who labelled them as "homo ferus"(Fisher lex., 1969). Michael Traugott Fronius (1761-1812) quotes an earlier case in a paper published in 1794 at Sibiu (Romania).

In the study *Description of the wild man found in Transylvania, near Brasov in 1781* (which was published in Hungarian -1770 -, and German -1794- in Sibiu and Viena), M.T.Fronius remarked: "such representatives of human species found in the wilderness (....) can become a point of interest to biologists, philosophers or historiographers. Biologists can discover a sample of the degeneration suffered by human species, philosophers can penetrate into the secret depth of pyschology, histographers can find highlights for further research". For the author himself the case meant an occasion for feeling "the real value of social life" in human evolution. The wild man was found by a peasant in the woods around Brasov. He was about 23-25 years old. He had difficulties walking on two legs and could not stand upright. He could not talk and did not evince any propensity. He was scared, had no positive emotions, a.s.o. Fronius' description is much more detailed, but we cannot reproduce all the details. The author had the opportunity to observe this "wild man" three years later, but the creature underwent but few changes; he could not sleep in a bed, and would make his own primitive shelter for the night. His "sense of propety" seemed to have become stronger, as he would gather around himself all kinds of worthless knick-knacks.

T.Bogdan makes an interesting remark on Fronius' method of investigation, which implied a series of tests performed on the subject. "The social reactions" of the individual were also put to test, by comparing his response to people he had come to know with his attitude towards people who were total strangers to him.

We can find other similar examples in psychological literature: the case of Kaspar Hauser in Germany described by A.Feuerbach (1928), the "wolf-children" found in Indian and studied by K.M.Zing (1940), a.s.o. Another cathegory of cases that may function as an "expériment invoqué" are monozygotic twins (A. Gesell, H.Thompson, 1925; J.Hilgard, 1933, etc).

4. Although Cl.Bernard elaborated his *Introduction to the study of experimental medicine* within the strict boundaries of the medical sciences, his works had a strong methodological impact on all the behavioural sciences at the end of the 19th century and during must of the 20th century.

He anticipated in almost two decades the creation of experimental psychology (W. Wundt, 1879). The Roumanian psychologist C. Radulescu-Motru (1868-1957), who was one of Wundt's disciples, wrote about the importance of Bernard's work: "...in the famous book written in 1865 there was no direct reference to psychology, yet all its problems were considered. In the psychological literature of our times (he wrote in 1898 and later on in 1923), there are only two books whose influence can be compared to that of Bernard's writings. They are William James' *Principles of Psychology* (1890), and Wundt's *Vorlesungen über Menschen und Tierseele* (1863), both reprinted several times.

C. Radulescu-Motru (1923) also tried to integrate the "expériment invoqué" into psychological studies. And, as to underline the importance of the "provoking idea" in Claude Bernard's methodological conception, the Roumanian psychologist comes to the following conclusion: "Those who cannot interpret the experiences provided by nature will sooner or later resort to nature itself in order to justify their hypotheses" (pp. 21-22).

The "expériment invoqués" have often been a point of interest not only for biologists, but also for a number of physicians, philosophers, psychologists, jurists, historians, philologists, a.s.o. Obeying the principle according to which extreme cases were to be compared to those belonging to the area of normality, the model of analysis and study described in behavioural sciences generally follows the same steps (fig. 1.).

<i>Observation and description of the behaviour evinced when the subject is integrated in the social context</i>	<i>Educational and medical influences suffered by the subject</i>	<i>Modifications obtained in the subject's behaviour case</i>	<i>Final interpretation of the</i>
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Figure 1. Model of analysis and study described for behavioural sciences follows four steps.

The conclusions drawn by the researchers who study and interpret the behaviour evinced by these wretched human beings generally end with a suspicion that their diseased condition is congenital. There are some defining aspects of such descriptions which we may detect both in those anticipating and those coming after Bernard: first, such topics have often been reanalysed at various stages in the development of behavioural sciences; second, each new insight contained a cluster of theoretical ideas governing the scientific interpretation; third, all researchers persisted in following their subjects' evolution over the years; fourth, they all revealed the possibilities of acquiring information in various domains through the study of human beings formed in a "wild" condition, or of other exceptional cases.

5. Claude Bernard's "expériment invoqué" has been constantly and concentrically developed ever since in the field of psychology - more specifically in general, genetical and experimental psychology, being extended to other psychological group phenomena.

A century later, the 1st volume of the *Traité de psychologie expérimentale* edited by P. Fraisse and J. Piaget (1963) contains the following remark made by Paul Fraisse: "the distinction between 'expérience provoqué' and 'expérience invoqué' introduced by Cl. Bernard is extremely useful in psychology.(...) In the case of the former, the researcher influences *the independent variable* and observes the consequences of his action.(...) L'expérience est dite invoqué quand *la manipulation d'une variable indépendante a été réalisée sans qu'intervienne l'expérimentateur*" It is the case of cerebral injuries caused by severe shock disease, or of biological identity found in homozygotic twins. But it is also the case of differences in cultural level, differences in education due to unequal chances in people's lives, including educational phenomena that cannot be artificially produced.

Along with Claude Bernard, Paul Fraisse also considers the possibilities of using psychological experiments organised by other researchers as "expériences invoqués". He illustrates the use of "expérience invoqué" in psychology by the study of human perception in which case the researcher can make a comparative study of *the natural variations* present in the given process (by establishing the differences in age, sex, aptitudes), and *the accidental ones* (like deafness, blindness, a.s.o.).

The so-called method of monozygotic twins - included in the concept of "expérience invoqué", is often used both in European and North-American psychological research, including R.Zazzo (France) and A.Luria (USSR) among the best known in the field.

Some authors consider that Claude Bernard's "expériment invoqué" is one of the most widely used in social sciences; the reason for this is, according to R.Pinto and M.Grawits (1969), that the researcher is very seldom in a position to trigger off certain social phenomena.

The analysis of correlations existing between cases of lung cancer and smoking habits, which can be done only by using an "expériment invoqué", is the example they provide in this respect (*Methods in Social Sciences*, p.313).

In North-American social psychology, the concept of "expériment invoqué" is replaced by an equivalent term, "correlational research". It stands for "a method aimed at revealing the associations or correlations existing between two factors without however determining whether they bring about changes in each other" (R.Feldman, 1985).

According to R.Feldman, the correlational approach is used in those instances where the researcher cannot produce or change the investigated variable. It is the case of human behaviour evinced during earthquakes or any other instance pertaining to the socio-psychology of disaster.

In Roumanian academic psychology, the method of "expériment invoqué" has been referred to by several authors in textbooks of general psychology, educational psychology, social psychology, psychology of the child (T. Bogdan, 1968, 1971; A.Chircev, 1971, 1983; Ana Tucicov-Bogdan, 1973; a.s.c.).

It is a method that can help us reveal certain behavioural patterns and the relationship existing between man and nature, society and individuals, ethnical groups and culture, a.s.o. While the early experiments were meant to contribute to the theory of human development by establishing -through comparision-, the part played by social background in process, contemporary experiments are mostly directed towards revealing the contribution of socio-cultural elements to the behaviour of individuals or groups. The "expériment invoqué" may prove useful in the humanization of macro-social relationships, in the theory of communications between large group of people, between different ethnical groups and cultures.

We shall now provide, in a summary form, the advantages offered, in our opinion, by this method in the field of behavioural and social sciences.

1) The social and psychosocial situations occurring in real life or produced accidentally are highly significant. 2) The realities to which the method applies are authentic. 3) The recorded behaviours may acquire a "post-dictis" value. 4) The completeness of behaviours has a great illustrating force. 5) It is open to multidisciplinary study. 6) It explains the connections existing between the conditions and the effects they engender. 7) It generates useful analyses for science. 8) It persists in social memory. This may also result in certain imaginary distortions, reductions or amplifications of the events implied, which do not affect nevertheless their very essence.

The impossibility to check and measure *more geometrico* the social facts constituting the subject of the experiment is an important limitation to the method.

6. The methodology of "expériment provoqué", that is the laboratory experimentation is ineffective when psychological research focuses upon complex natural psychological phenomena. First of all, the efficiency is challenged by the systemic nature of that kind of phenomena. Secondly, the complexity of intercorrelated action of all the social processes, structures and factors involved, as well as the genuinely implacable nature of the very phenomenologic development does not allow us to generalize some incomplete (partial) data obtained by isolating experimentation of some sequences of the overall phenomenon.

Such phenomena, which could be approached by a "post-factum" model-analysis, are natural catastrophes (fires, floods, eruptions of volcanos, earthquakes a.s.o.), and social catastrophes (the effects of totalitarian régimes upon the spiritual and socio-economical life of some nations- civilization, the sociopsychical effects of economical crises, the sequels of huge mass-action as wars, revolutions, nuclear catastrophes, a.s.o.)

Nowdays, we are able to attempt a more veracious psychosocial diagnosis of such natural or social displays of huge energies by using the systemic-mathematical model-analysis and computers' ability to intercorrelate an enormous quantity of data.

In social psychology there are only a few systematic approaches to such phenomena in agreement with Claude Bernard's "expériment invoqué", but in the 80s, research of this kind has been carried out upon the volcano at Atmer (Columbia), 1985 (M.Vorweg, 1987).

7. Data from experiments or case analyses have been obtained by connected sciences as medical and individual psychology, psychiatry, sociology together with data of studies of the military or civilians during the war, or of the effects of nuclear catastrophe of Hiroshima and Nagasaki, or the characteristics of "psychological war", etc. The analyses have been performed mainly upon individuals and also by statistical generalisation of group behaviour (as panic pattern). They mainly isolate some variables, and study them without any concern for their intercorrelation with other variables over which they have no control (F. Prax, Pélicier, 1979; P.Lefevre, 1979).

8. Complex psychosocial research could not operate a strict choice between the "*expériment provoqué*" and the natural, "*invoqué*" experiment as a unique study method. They could develop unnatural (artificial) devices such as the isolation of some sequences of general development of natural events, or the study of the way some variables function in different situational patterns, then they compare the data obtained thereby.

Concrete research upon social phenomena also uses methods such as: systematic examination of facts, interviews with people having participated in those events, questionnaires (with closed or open answers).

This kind of research is meant to evaluate in psychosociological terms the main characteristics of social group structures and dynamics such as: interpersonal relationship, level of cohesion, attitude structures, competence structures, efficiency of acting, tendencies in the development of interpersonal behaviours, in order to obtain an optimum in decision making regarding organization, influence and management.

9. From the data at our disposal, the systematic study of the earthquake in Mexico and Colombia revealed first of all a phase of behavioural blocking both in individuals and professional staff (apud M. Vorweg, in Salazar and Tatiano, 1986), because of humans' restricted ability to act due to anxiety, to the disorganisation of everyday life, and to a lack of authentic information. Emotional destabilisation due to socioeconomic disorganisation developed into a collapse of collective security of the population as a whole, correlated with the annulment of the threshold of personal security which caused some behaviour of personal sacrifice.

Then extremist behaviours appear, after the stupor, and attempts to deny reality -especially its multidimensionality, behaviours such as: aggressive responses as personal defence against the loss of one's family and one's customary everyday life, or the opposite, resignation, apathy.

They have differentiated "five behavioural phases which called for specific kinds of crisis intervention: elementary fear of death (people kept apart from one another, clinging to their possessions and unaware of opportunities for social and organizing behaviour); flight behaviour (people overcome by panic ran away at random exposing themselves to uncontrollable dangers...); activation, in the form of concern about one's relatives and friends (activities first centered on finding out whether or not relatives or friends were affected); participation in organized rescue operations to save

neighbours, often leading to an emotional crisis". This crisis could last for hours, days and even weeks. The length of time of the various phases is quite different in different people (M. Vorwerf).

Organised interventions (management and/or medical care) led to minimizing actual disruptions of the ability of individuals, groups and organisations to act (Stacherbak, 1986, research concerning Chernobyl). They distinguished three different kinds of interventions: the willingness to act, social competence to act, and the aim for action in organizations.

The aim of those model-analyses is to demonstrate the fact that survival in catastrophic situations means enlarging the possibilities of action, that is, they help people to increase their ability to act, to "fit ourselves and others for action" (M. Vorwerf, o.q.).

10. Roumanian social psychology faces the challenge of systematic study of a socio-human catastrophe, that is the effect of the totalitarian régime which has enslaved that nation. The study asks for a model-analysis, inevitably complex, because simply isolating some of the variables (such as economic oppression, the controlling of individual a.s.o.) does not operate widely enough in such a historical case and could not explain the tragedy of a people which had already reached a high standard of civilisation, but was brutally forced to an abrupt return to behaviours characteristic of a level of civilisation historically overcome. And this in both individual and macro-social levels.

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The psychological sources of modern rearing

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ABSTRACT

In our animated century, also child-rearing has changed. In the 1980s, the restriction and pressure characterizing child-rearing in previous centuries were placed among the least accepted methods.

The question here dealt with is the role played by rearing-theory in these changes; particularly, the role played by psychoanalysis, which, according to some authors, is directly connected with the modification and crisis in modern rearing.

However, psychoanalysis allows for a number of approaches to rearing that lead to very different practical consequences. From the 60s on, one of them has been widely accepted, as it agrees best with current life style and society.

Among the significant changes characterizing our century, some of the most important ones are those which have taken place in the field of child-rearing. If we were to study the tradition of modern rearing at our disposal according to different sources, we would create an impression that in the field of child-rearing just as in the field of technical development or in demographic expansion, in the 20th century's custom-system after stagnating for a long time and then altering slowly there has been a sudden change.

There have always been excellent people who thought that the most important means of rearing are understanding and sympathy for the child. There is no need to prove that until the beginning of the Twentieth century severe restraint and deterrence were not only accepted but suggested methods in the child-rearing. Katharina Rutschky in her work *Schwarze Pädagogik (Black Pedagogy)* (1) described long series of warning examples from the middle of the 18th until the end of 19th century. In the pedagogic works cited by her parents there were admonishments to compel obedience from their child and to break down his will at the right time, if possible at that age when the adult's physical superiority seems to be absolute.

Recently, popular editions on this issue have insisted on avoiding pressure and cruelty by all means. In certain cases it is pardonable by Benjamin Spock if a parent

beats his child (2). But many popular books for parents dating from the 80's regard physical abuse as an irreparable fault in rearing that must be avoided by the parent. There are countries where physical abuse of the child is prohibited by law. But hostility to abuse does not relate only to physical abuse. In the book written by Alice Miller about hidden cruelty in child-rearing there is a chapter under the title "Tender Cruelty" (3).

The question I would like to speak about in this report is the following: to what extent the changes influencing the masses emerging in rearing are the consequences of the diffusion of principles and ideas regarding rearing, or whether we can say that all this has no influence, if they are not linked to the social processes?

Philippe Aries (4) thinks that the relation with the child and the characteristics of child-rearing are the complex parts of the social process and the history of civilization: by his historical recent analysis discriminating treatment of the child has come from the gradual separation between the world of the children and adults because of decreasing infant mortality and the middle-class way of life. On the contrary, the excellent representatives of psychoanalysis think that treatment of the child seems to

be a vicious circle: a good illustration for this statement is Alice Miller's book mentioned above. Miller believes, together with a number of psychoanalysts, that the parent who was compelled in his childhood to suppress his own feelings, necessarily adopts the same means in his child's rearing. On the other hand Alice Bálint, who is also an psychoanalyst, thought that if parents understand their children better it will alter the conditions and required degree of the suppression of the child (5). Elsewhere she states that "reforms in child-rearing necessarily modify the culture, even if there is not a conscious purpose...So pedagogy is one of the most revolutionary sciences" (6).

The influence of child-rearing on the social processes was recognized just by considering psychoanalysis in the last decades. Some writers dealing with childhood believe that we can recognize psychoanalytical influence in the recent failure of child-rearing. In her book (7) on the basis of reports made with children and adults, Marie Winn has held an alarming hypothesis concerning the whole of American rearing-culture of the 80s: there is no longer a protective world for children; they, just as in Middle Ages, melt into the adult community without any transition. Parental prestige, which was once an essential condition for child-rearing, and which had a significant role yet in the indulgent rearing of the 60's, has a negative value today. Adults refuse to make important decisions and they often throw responsibility upon the child, even during the years when he is not able to ponder things. They don't make an effort to select the facts of reality he could get to know, although if they wished to do this, they could hardly do so because of the commercial influence of modern telecommunication. They don't try to hide their more and more difficult fluctuation of privacy, their intimacy from the child. So the child as an equal person takes part in his own rearing; but the outcome is doubtful. Children don't become adults who are more mature,

balanced, and can understand the world better, but they are anxious, uncertain and prematurely old persons who cannot undertake their adulthood.

According to Marie Winn the cause of this position is in psychopedagogical theory represented by Selma Fraiberg and other psychoanalysts. She states that the parent often acts as his own child's psychoanalyst: instead of telling him directly what to do, he continuously analyses both him and himself, and tries to get the child to understand the motivations of his decisions. Winn brings parental doubtfulness in connection with the neurosis-theory emphasizing the importance of early effects, and the earlier and earlier beginning of sexuality with the child sexual theory.

But is it really the influence of psychoanalysis? Along the telecommunication and muddling family life, were Freud's concepts about child development and sexuality what "caused childhood come to an end as an independent stage"? (Winn, p. 130). In certain respects psychoanalysis seems to realize what is criticized by Marie Winn. According to this both children and adults are characterized by passions and ambivalences; this point of view played an important role in the children's inadequate emancipation relative to their ages. She refers to Freud in connection with the increasing influence of the earlier and earlier start of sexuality, sexual perversion and incest in the world of child, and the acceptance of all this.

Most of us would probably think that psychoanalytical rearing is one kind of pedagogical adaptation that actually questions the need for rearing and, what is more, the adult's presence. On the one hand this standpoint comes from the above mentioned vicious circle -from the idea that the adult's world is irremediably corrupted. On the other hand it relates to the tendency in which development is a spontaneous, self-creating process, and if it is not disturbed, will produce harmony. According to Neill, who established the Sommerhill team, the child who does not have any restrictions develops within his natural capacity, if he is only kept away from adults' encroachment. According to this tendency of psychoanalytical rearing, a coherent self-scheme is produced by libido. According to Neill, we must choose from prestige, freedom, order and self-determination (8).

We find a less extreme, but similar standpoint in Alice Miller's book. She has directed parents' attention to the fact that recent rearing hasn't gratified the child's but the adult's needs.

It is a paradox that the starting-point of originally conservative as well as psychoanalytical liberal rearing are equivalent, and their basis is force, although of very different kinds: the world of the child contrasts with the world of the adult and the child contrasts with society. The crucial difference between the two rearing-types is that in conservative rearing social requirement is the positive pole and the child's nature is the antisocial one, whereas according psychoanalysis, which stands on the basis of the self-creating principle, the child is ideal and the world of adults is inhuman.

Freud also thought that children ought to be confronted with the hostile world of adults (9). But his followers in Budapest differed in opinion from him, and on the basis of their work quite a different pedagogical view seems to have been outlined.

Alexander Ferenczi, the Bálint couple and Imre Hermann proved with their observations that the child has an innate need for the outside world. So instincts may not be essentially opposed to the outside world. Alice Bálint attaches a crucial role in the rearing-process to the environment, whose first representative is the mother. By her standpoint the limitation of instinct goes together with all sorts of life, and one of the most important aims in rearing is that the child learns to tolerate the tension relating to this limitation. In 1937 a conference was held in Budapest with the participation of Hungarian, Czechoslovakian, Italian and Austrian psychoanalysts and also Anna Freud and Dorothy Burlingham. The Bálints introduced their view (10), in which they tried to place psychoanalytical rearing on a new standing.

For unknown reasons Alice Bálint's views about rearing were not extended in psychoanalytical nor in pedagogical literature. Her family and friends were forced to emigrate. She died in 1939 when she was only 41. But most of her work was written both in English and French. Her husband Mihály Bálint became an outstanding figure in world-wide psychoanalysis, and lived until 1964.

It seems that the antipedagogical tendency in psychoanalysis was rather in harmony with the Western European sentiment of life which was called identity-crisis in civilization by John Sommerville (11), who studied historical changes in childhood. In the 20th century "the culture does not believe in itself anymore". One proof of this is dadaism, according to which the only way for human being of remaining faithful to himself is to remain infantile. Young people revolt not only against the older generation, but against the whole epoch and civilization -such a revolt was reflected in the hippy and punk movements; the former was milder, while the latter was aggressive. To remain for ever youthful -this style is reflected in the way adults dress, the idea of eternal youth.

All this makes children and rearing an especially hard task. When adults behave like children, then children do not overexert themselves to become adults -Sommerville affirms. But without aims and perspectives rearing cannot be imaginable.

Christopher Lasch in his book *The Culture of Narcissism* (12) puts the adult being unable to grow up in a closer light, and calls this self-adoring. According to Lasch, this widespread attitude leads to "seeking after happiness in an extreme way, longing for prompt gratification, and one lives his life restlessly, in the attraction of one's ungratifiable wishes" (p.9); that is, from a psychoanalytical point of view he remains a child. Lasch thinks that the distortion of our time and the self-adoring is a consequence of bourgeois individualism, and he directs our attention to the fact that rearing depends only partly on parents. "The deterioration of the Superego is reflected in the deterioration of parental prestige in American society", he writes.

But one would think that self-adoring and the infantilization of the masses are not only in ideological connection with bourgeois individualism. If we agree with Alice Bálint's opinion about frustration-tolerance and learning the capacity for despair it would be necessary to analyze the characteristics of modern consumers' life style. In Western societies both for children and adults states of deficiency are very rare, and thus the need for despair is more and more rare in everyday' life.

In order to know what kind of role was actually played by psychoanalysis in the changes in rearing there remains an important fact to be considered. By the 1930s psychoanalysis became well-known also in the United States. Nevertheless, as Mary Winn stated too, it has noticeable influenced rearing only since the 60s.

As a summary, we would say that psychoanalysis definitely plays a part in the modern rearing and also in its distortions. But there were more interpretations of psychoanalysis which led to different practical conclusions. That psychoanalytical tendency according to which it is important to eliminate pressures and limitations from rearing and believes in the child's self-creating capacity is only one of the possible adaptations. As Marie Winn writes, the child protected world seems to have come to an end; instead, the world of adults has fused together with that of children. But this is just the opposite of what happened in the Middle Ages: then children had to behave like little adults, and now adults would like to remain children. It appears that now it is harder to imagine a compromise between the individual and society, just as during the time of psychoanalysts' work in Budapest, who still spoke proudly about west civilization in the 30s.

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Dutch youth and American mass culture, in the 1920s and 1950s A cultural studies approach

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INTRODUCTION

An evident but significant fact is that Holland is a small country and has always been under the cultural influence of other countries. The awareness of smallness, and of being surrounded by other, larger and more powerful nations has been essential to Dutch national consciousness since the seventeenth century (Goudsblom 1967). Fears of foreign cultural domination referred in particular to France, Germany, and - in this century - the USA (Wilterdink 1990). 'Americanization' was feared and resisted particularly in the interwar period by the cultural and intellectual élites, though there were significant exceptions. After the Second World War, when American influences were stronger than ever before, these fears were to some extent expressed again, although the prevailing attitude became more ambivalent. In both periods, but particularly after 1945, variants of pro-Americanism were at stake, some of which were certainly not opposed to specific forms of American mass culture.

In this essay my central focus will be on the reception of American mass culture in the Netherlands, particularly among youths, respectively in the 1920s and in 1950s; two periods in which the 'dialectics of Americanization' were intensely experienced by contemporaries. The central questions are: how were the specific manifestations of American mass culture concerning youths received in the Netherlands among various groups? What groups regarded these American mass cultural forms as threatening? What groups regarded these as liberating? And for what reasons? I will deal with relevant similarities and differences between the two periods. And I will also consider the ways in which American mass cultural forms were partly mediated by other countries, especially England and Germany. But first some preliminary remarks have to be made on conceptual problems of cultural transmission and exchange which present themselves here.

The process of borrowing mass cultural forms from America

In this context theoretical notions on cultural transmission are highly relevant. In general I underline the following five theses formulated by the cultural historian Robert Darnton:

1) The study of cultural transmission requires close attention to the vehicles of communication and their mode of operation; 2) The semiotic conventions of organizing messages need to be studied just as much as the content of the messages; 3) Far from being a matter of passive reception, cultural "consumption" is an activity, the appropriation of a message or the making of meaning at the receiving end of the transmission process. This activity itself varies according to time, place, and social location. The study of cultural transmission therefore involves the study of cognitive styles - of reading, viewing, listening as historical phenomena; 4) The problem of intertextuality: people are incessantly making and taking messages, often in their own way and on their own terms, even under the conditions of modern, mass culture, in which professional groups manipulate the media and a mass audience allegedly "consumes" their "product". In other words the fashioning and refashioning of meaning cannot be reduced to the two models that so far have dominated the study of the transmission of culture: the downward-percolation model and the elite- versus popular culture model; 5) The attempt to construct a new frame of reference will not draw heavily on communication theory in sociology - at least not from the old Lasswell-Lazarsfeld way of putting questions: who said what to whom under what circumstances and with what effect? Literary theory and discourse-analysis may be more helpful. One certainly would be foolish to ignore arguments about the way rhetoric can undercut the ostensible message of a text.

More specific problems and points of attention which should be covered in a fully elaborated reception history of American mass culture, are the following.

- What accounted for the rise, prosperity, and decline of American youth cultural forms in the Netherlands? In this connection the relevant economic, social, cultural-mental and political context have to taken into account. For instance, what power relationships in cultural exchange were at issue, especially with regard to 'youth cultures'? Also the relationship between modernization (or civilization) processes and mass cultural forms need our attention, since - as will become clear - 'America' symbolized and embodied various kinds of modernity.

- What institutions and other vehicles in America and Europe, more specifically the Netherlands, were responsible for transmitting American youth cultural forms to Dutch audiences? What roles did youth specific culture and recreation industries (fashion and design, film, popular literature, music and dance, amusement, sports), mass media and youth organizations, youth clubs, dance halls, jazz- or rock music clubs play? In this connection the influences of pedagogics and other psychosocial practices modeled upon American exemplars are relevant too.

- What exactly did American mass culture - actually or supposedly - influence, due to what real or putative causes, and for what reasons?

- What meanings and connotations were attached to the forms and contents of American mass culture concerned? What images, fantasies and/or myths of America did each of the groups involved hold (e.g. specific images about America in rock

music)? How were these images represented? With what material and immaterial cultural elements were these associated? What own contributions did Dutch youths make, and what re-interpretations did they give?

In addition much attention to the aesthetics of youth cultures should be paid. Then we must try to 'read' and 'decipher' these with the help of some heuristic or analytical scheme. Besides we should ask what psychological and social functions these aesthetics fulfilled as to the needs, wishes and preferences of the youths involved.

- What role models and behaviour codes were implied in the information and images of American mass cultural forms as offered by culture industries and mass media? How were these received by young people of various classes?

- To what extent did a process of 'decontextualization' occur during the cultural transmission and dispersal of the mass cultural forms concerned? How did these evolve in the European, respectively Dutch context? Did specific variants possibly assume a high-brow status (e.g. modern jazz)? Or was a development towards a relatively autonomous, mass-mediated culture at stake which had only weak, or almost no links with American mass culture in a stricter sense?

Also a comparison with similar reception processes in other societies might be made. Comparative - and per definition interdisciplinary - culture studies offer opportunities for obtaining clear insights in processes of cultural transmission and cultural impact which can hardly or not at all be achieved in other ways (Blair 1988).

Crucial for countries like Holland which are at the nexus of various cultural streams was (and still is) the import of American popular culture as mediated by other, especially neighbouring countries, in this case England and in the secondary instance (West) Germany. Before the large expansion of modern mass media - and their creation of a 'global village', not in the least concerning youth cultures -, and also before the higher rates of geographic mobility of young people such intermediating processes were very probably more important than nowadays. We now arrive at the crucial topic of 'superculture', to use a term coined by Bigsby: "Although American popular culture necessarily carries the imprint of the society which produced it, its movement beyond the confines of America changes both meaning and structure. It becomes plastic, a superculture, detached from its roots, and widely available for adaptation, absorption and mediation." (Bigsby 1975: xii-xiii). This implies that no complete cultural homogenization (or cultural levelling) is at hand. "Rather, American popular culture - Hollywood films, advertizing images, packaging, clothes and music - offers a rich iconography, a set of symbols, objects and artefacts which can be assembled and re-assembled by different groups in a literally limitless number of combinations." (Hebdige 1982: 216).

Nevertheless, in this connection we can often discern phenomena of 'Americanicity' (or 'Americanité', a neologism borrowed from Roland Barthes, 1977), which are concentrated round the theme of some youthful and dynamic life, filled with excitement and adventure, and dominated by an imaginary 'American'

surrounding (Chambers 1985: 38). Advertisements and tv-commercials played a significant role in this world of images - they do even more so in our contemporary situation of world-wide, mass-mediated culture -, e.g. advertisements for American brands of cigarettes or soft drinks.

Case study I: 'Roaring twenties' in dutch society?

When studying the reception of American mass culture in Europe the 'roaring twenties' in the Netherlands constitute an interesting case. During the interwar period this small country, undergoing the radiations of various cultural formations (German, French, British, American), was a conservative society par excellence, qua economical structure, qua ideological anchoring, qua attitudes to the developments which took place elsewhere in the world. (Though there were countervailing trends which I will articulate later.) So one may expect that the various kinds of modernism and modernity that were at stake in American high and popular culture of the twenties were at right angles to the cultural life that prevailed in the Netherlands at the time. This leads us to the obvious questions: to what extent, and how, were the Anglo-American mass cultural contents and forms received among the Dutch? And what were the outcomes of the socio-cultural clashes that took place in the Netherlands?

Very characteristic of the twenties were the many contrasts in everyday cultural life, strengthened by the 'simultaneity of the unsimultaneous' (that is various socio-cultural formations next to each other at the same time). In the Netherlands these contrasts were articulate, due to the general cultural climate with its conservative overtones, class-distinctions and denominational divisions on the one hand and the relatively rapid dissemination of material and cultural objects conveying messages of modernity, through new and expanding media of communication and modern technology on the other. Various forms and contents of modernity and modernism became highly visible in the public sphere. Next to material aspects of American popular culture, this especially concerned the symbolic level of images and sounds offered by mass media and culture industries, with regard to film, popular music, fashion and design, amusement and leisure.

The reception of American mass culture among Dutch youths concerned at least three domains of culture: a) the leisure culture of the rank and file which built on a mixture of working class culture and folk culture, with traditional riotous manifestations of the youth, like 'eel-pulling', and the 'lazy-bones'-feast, a rudiment of a Dutch variant of the older charivari; b) the modernist movement among small groups of artists, and c) the conspicuous consumption culture of the well-to-do, following the latest trends. In the first instance the mass cultural impact from America mainly pertained to the cinema films: westerns, melodramas, comics, love stories; and vicarious participation in sport by watching 'Americanized' sporting games. Only partly was there an active participation of working class youths in the new body culture (modern dances and sports like tennis, rowing, hockey) and jazz music. This was mostly confined to dancing in public dancing-places; for the rest the latter domains

remained almost fully reserved to middle- and upper-class youths. Especially for those youngsters who belonged to liberal circles in trade and industry, and first of all the *nouveaux riches* among them, the 'roaring twenties'-culture from the American better off became the primary frame of reference for the ways they spent their leisure. In this context one has to discern that the reception of American culture was partly mediated through the socio-cultural contexts of the metropolises of France and Germany and, only secondary, also of England (Wiser 1982, Laqueur 1974, Jenkins 1974). A significant element was the escapism of young Anglo-Saxon intellectuals and artists to specific cities of Europe (Paris, Berlin, Munich, Budapest, Vienna), but also less obvious places like Bucharest, Rapallo, Bandol, for instance. For the majority of these youngsters a main reason lay in the much lower costs of living - as a result of a very favourable exchange rate for Americans and British - than in their native countries and home cities. Another reason for leaving one's homeland, especially for the British, was unemployment. Also refugees from prohibition in the USA came over ostensibly to taste culture, but also to get drunk.

In the 1920s the American lost generation's presence in Europe was manifest mainly in France, and this mostly concerned a new impetus to modernism in the arts. The radiance of its culture as well as the ready reception it received in Paris at the time, may have been more limited for that very reason. Anyhow, its influence was only lightly felt in the Netherlands, also due to the fact that Germany remained the primary reference-society for the Dutch. Moreover, in Europe since the turn of the century, modernism and the modern experience had pressed on the most in the urban, industrial parts of Germany, and above all its capital Berlin (Eksteins 1989).

While in the postwar period there was everywhere a natural scepticism about 'Americanization', in the end it was in Germany that the least resistance was shown. "There self-doubt was most profound, and America capitalized on this doubt, both figuratively and literally." (Eksteins 1989: 271). In the Weimar Republic each of the groups involved borrowed specific elements from the large and heterogeneous constellation 'America'. From around 1923/24 for many of the owners and managers of big companies the USA became the model country par excellence concerning modern technology, work methods and management practices (Taylorism and Fordism). This even partly applies to the trade unions and labour movement, especially their social-democratic variants. But also within the arts (literature, music, architecture) the USA offered ideal examples for particular groups of intellectuals and artists, spread across the political spectrum. Within the domain of leisure and recreation several elements from American popular culture - fashion, popular music, modern dances, musicals, revues, horror films, melodramas, 'trivial literature', 'American' sports - were imported and incorporated into the German culture and amusement industries. In this connection also elements from 'high' and 'low' culture were intermingled (Hermand & Trommeler 1988: 49-58, 69-89, 313-322, 401-407).

In the early 1920s even a USA-cult developed in élitist circles in Berlin. Especially among young male artists, like for instance Brecht, an 'American look' (beardless with sharp profile, 'steel body') was in vogue; overt sexuality and rough kinds of sport like boxing were understood as an indication of self-assurance. Against

the 'bloodless abstractness of expressionism' one now discovered modern civilization as 'second nature' and celebrated its material objects, with American skyscrapers as the prototype. This America-anarchism of intellectuals changed into a critique of American capitalism in the mid-twenties, when American investors penetrated the German economy, following the example of the Dawes loan in 1924.

Besides, among middle- and upper-class youth of the big city (Berlin), 'Americanism' in the form of 'roaring twenties'-fashion, dance-crazes, jazz music and songs, and possibly also driving in cars or on motor-bikes, was very popular. This trend was criticized through parodies in many musicals, however, while also specific students' journals opposed the 'naturalistic-bestial moment' of jazz music (Schäfer 1986; Herman & Trommeler 1988: 149-151).

Particularly Germany's popular culture had a strong impact on the public at large in the Netherlands. Many 'American' influences entered Dutch society from Germany, that is, mostly from Berlin, Europe's 'New York' at the time. German popular culture in its turn was influenced by American mass cultural forms and trends, putting their stamp on part of the numerous German 'schlagers' (hits) - with a peculiar mixture of German and English words in their texts -, on musicals and on the UFA-entertainment films in some of which also two Dutch film actresses, Truus van Aalten and Lien Deyers, played the leading lady. The UFA (Universumfilm Aktiengesellschaft)-studio's were to a high extent financed by American investors; there were close links between the film industries of both countries too; the Hollywood-Berlin connection flourished.

But also Germany's modernist trends in the arts exerted their influence on the Dutch. After 1918 Dutch artists (writers, painters, musicians) and intellectuals regularly visited the city of Berlin or went to live and work there for some time. Like other members of the 'generation of 1914' they were fascinated by the image of the traveler. They were 'wanderers between two worlds', wanderers between the dead world of the war and the still indeterminate new world. The urge to leave one's own country and travel elsewhere, preferably to some big metropolis, ran parallel to the urge for spiritual and societal innovation, towards living dangerously and vitalism (Wohl 1979). Several of them actively took part in Berlin's night life, with its mixture of all kinds of high- and low-brow culture with strong 'American' overtones. Also Dutch tourists who spent a long weekend or a vacation there, underwent such metropolitan experiences.

Those Dutch well-to-do youths who were involved in the culture of the roaring twenties, were also attuned to the Berlin-scene, next to Paris and London. Their largest geographical concentration was to be found in the cities and adjoining villages and small towns in the western and central part of the Netherlands.

The spectre of Americanization; three central issues

As to 'Americanization' in the Netherlands, three problems were high on the agenda of the cultural élites: 1) advertizing, 2) cinema and American films for a mass audience, and 3) the new body culture concentrated around 'modern dances' (and

associated popular music), Americanized sports and fashion. It must be emphasized that particularly in the first two instances and to some extent in the third one too, visual images which were at right angles to 'Calvinist' Dutch culture, constituted the essentials of the cultural form. Following similar developments in America, in all three cases also an 'American' cult of youthfulness was at issue. The symbolic role attributed to youth and youthfulness in relation to modernization and modernity was crucial here. Especially the second and third issue led to moral panic and intervention strategies with regard to the leisure of youth.

In the light of the prevailing cultural-mental climate it is not astonishing that the popular culture of the 'roaring twenties' was generally not welcomed affectionately by the Dutch élites. The large majority of them opposed the advance of 'Americanism' and the associated mass cultural forms. This response-pattern was manifest in the moral panics concerned and the social-pedagogical interventions which were practised, from the Right and the Centre as well as the Left. However, there were significant 'liberal' minorities, both among the economical and cultural élites, which held a more open-minded, if not positive attitude towards various kinds of modernity, among which were those conveyed by mass cultural forms as borrowed from America. The same applies to allied segments of Dutch youth from the middle- and upper-class. The reception of the 'roaring twenties'-culture among these youths was liable to domestication processes, e.g. modern dances as mediated through English dancing-schools and women's fashion styled after neat British exemplars. Furthermore, the conspicuous consumption patterns and leisure activities concerned were often confined to settings, constrained by domestic life at parental homes, and - only secondarily - by denominational segregations.

The participation of the rank and file in the culture of the 'roaring twenties' was mostly at the symbolic level of films: the world of images of film-stars and the vicissitudes of their 'private' lives; the watching of Americanized sporting games, and also dancing in public dancing-halls. For the rest these youths kept to their working class and folk culture traditions, subject to the prevailing divisions and distinctions due to pillarization. So, as a whole, the 1920s in the Netherlands were not so much 'roaring' but rather constituted a tamed variant of wilder and more spectacular tendencies abroad.

Case study II: American popular culture in the 1950s and early 1960s

Crucial with regard to the impact of American mass culture was the further modernization of every-day life under American hegemony in the Netherlands during the first decades after the Second World War. The American economic, social, cultural and political interferences in this country within the framework of the Marshall-Aid program influenced production and consumption levels to such an extent that a mass consumption culture gradually developed in Dutch society. This development really carried over into the second half of the fifties.

America was the model country par excellence for new consumption patterns in Holland. Via direct import or the adoption of ideas and novelties from the United States, various commodities became popular. This list included not only all kinds of household articles, but also self-service shops, television sets, specific brand automobiles and so forth. The styling of industrial and many other material objects was also heavily influenced by American trends. Especially the streamlining of the equipment and the environment in modern cafés, snack bars, coffeeshops, icecream-parlours, jukeboxes, recordplayers and wireless sets had its impact on the material culture of the youth. However, not only influences from America onto the Netherlands brought about these changes. Also relatively autonomous developments due to further industrialization and modernization occurred within the country itself. This was like in other West European countries; in other words, to some extent an autonomous convergence took place.

In this reception history other significant elements are various psychosocial practices in a transatlantic setting: management practices with America as the model country par excellence; marketing and advertizing dominated by American agencies; educational and cultural exchange programmes: study tours of Dutch managers, representants of trade unions, scholars in the natural and social sciences to America, and on the other hand sabbaticals and visits of American scholars and advisers - especially on policy-making within various domains - to the Netherlands (Van Elteren 1987); import of (applied) behavioural and social sciences. One must recognize, however, that various ideas had originally been developed in Europe and brought to the United States through immigrants (many of whom were Jewish refugees). There these ideas were mixed with those 'hard-boiled' empiricist and pragmatic traditions which were characteristic features of higher learning in America.

Especially practical pedagogics as borrowed from America had a big relevance. With regard to changes in the pedagogical relations between young people and their elders, the further influence of the mental health movement - which had already been introduced in the Netherlands during the inter-war years - was of great significance. Through changes in education and research, it contributed to a further modernization and professionalization of the psychosocial intervention practices with respect to youth, especially within the areas of child guidance clinics and youth work. But on the other hand, directly or indirectly - through the popular press, columns in women's magazines or broadcast speeches - this also resulted in a psychologizing of the societal roots of "youth problems" and a further control of pedagogical relations by professionals.

Besides positive feelings about America, there were also negative sentiments. Most members of the traditional cultural élite in the Netherlands strongly opposed the advance of 'Americanism'. In the postwar years this cultural concern, however, was not seldom related to an admiration for the American political tradition. These policymakers and politicians experienced, to their dismay, that with the orientation on America as a guiding country and the import of all kinds of ideas, products and consumption goods, the Trojan horse of 'vulgar mass culture' was also brought in. This was in line with similar developments in other West European countries, especially England and France.

The American popular culture which invaded the Netherlands after the war was certainly not enthusiastically greeted by older professional pedagogues and those parents and other elders who were still strongly under the influence of the prewar cultural pattern they had internalized. An essential part of their life history dated from the period before the war, an era which was under the hegemony of the Humboldtian ideal of *Bildung* of German 'higher' culture. The élites of the various religious and ideological 'pillars' - characteristic of Dutch society at that time - were very concerned about the appeal which American films and dances and American mass culture as a whole had on youth. They tried to contain this movement through strategies to spread "Culture" among a broader range of society. With regard to this policy, hardly any differences existed between the various pillars.

It should be emphasized, however, that even the anti-mass culture critiques of Dutch opinion leaders (a.o. well-known social scientists) were heavily influenced by anti-mass culture thoughts of American writers and scholars - of whom a considerable number were former Jewish refugees from Europe (Rosenberg & White 1957; 1971; Jacobs 1961).

After the Second World War the daily life of Dutch youth was increasingly influenced by developments abroad, resulting from the growth of communication and information networks, increasing geographic mobility and the further internationalization of mass consumption and mass media markets. These developments accelerated at the end of the fifties when a profound modernization of Dutch society in. Already during the liberation of the Netherlands many members of the younger generation had eagerly embraced the popular culture-elements which the American, English and Canadian soldiers brought with them: chewing gum, jeans, colourful checked shirts, dixieland, jazz, dances like the jitterbug, and so on (Kroes 1981: 11-12). In the middle of the fifties Dutch youths began to give more articulate expression of their feelings, in relation to popular music (rock 'n roll and bebop) and other elements of American popular culture. In September 1954 young people acted very enthusiastically during a jazz concert of Lionel Hampton when playing his hit 'Hey Baba Ree Bop' in Amsterdam. In 1956 Holland was flooded by rock 'n roll songs and films. In September of that year the film *Rock around the clock* (with Bill Haley and his Comets) was shown in cinemas in several Dutch towns and soon teenage riots took place. The behaviour of these youngsters drew the attention of the mass-media and on the other hand the latter played an important agenda-setting function with regard to the life-style of other young people. At the same time moral panic-reactions of the broader public and of local authorities were stimulated by a tendentious news service about these rock 'n roll concerts and films and the deviant behaviour of teddy boys. In this period two conspicuous youth cultures were proliferating in the Netherlands, one of the 'nozems' (teddy boys), the other of the 'artistiekelingen' (existentialists/beatniks).

In the second half of the fifties the spendable income of working class youngsters steadily increased, mainly because of a lesser need or obligation to pay a part of their wages to their parents. Unskilled and semi-skilled workers especially tended to spend

their money on new consumption goods and commercialized leisure activities. Because of a biographical background which differed significantly from their parents, a generation which grew up in a period overshadowed by the Great Depression, unemployment, Nazism and war events, these youngsters dealt with the changing societal circumstances in a very different way. While they took the relative economic stability and prosperity more or less for granted, the elder generation saw the situation as one which was obtained through hard work and sobriety. The owners of cinemas, cafés and dancing halls were very well aware that this youth category was an important new commercial target-group. Products of the Anglo-American culture industry: Tarzan movies, westerns and sex films, and also rock 'n roll and jive music were soon the main components of this commercial recreation supply. In their longing for 'freedom' these working-class boys also occupied the streets where they tried to demonstrate their masculine identity. Their distinctions and status symbols were the moped, checked shirts, leather jackets and black working man's trousers, leather boots and pomade in the hair to give shape to a greasy forelock. Most of these attributes and aesthetics were inspired by Anglo-American models. These 'working class dandies' generally drove a streamlined moped (Italian type) with a buddy seat; preferably to carry a sexy female partner. This vehicle offered them the possibility of modern mobility (second best after an automobile, which most of them could not afford or were not allowed to drive). The girls in this social milieu had an inferior position; they were a kind of 'appendage' of the boys. They wore wide frocks and broad belts and had a so-called 'candy floss' hairdo.

'Teddy boy behaviour', however, was also a trend which was followed by a rather big group of youngsters. The 'original' wilder teddy boy style was commercialized and partly domesticated as a result. This process went hand in hand with the 'creation' of the neat teenager type in those years, about which later. The teddy boy style contained elements of traditional working class culture (informal solidary relationships, territory-boundaries and bodily masculinity) and had borrowed new elements from the Americanized mass culture: tight jeans, petticoat, leather jacket, pointed shoes, a duck's bottom, and behaviour patterns which were portrayed in American films like *The Wild One* (1953) with Marlon Brando, and *East of Eden* and *Rebel without a cause* (both released in 1955) with James Dean as their main actor. In short, items that were a contradictory mixture of authentic and manufactured style-elements. The most significant model of masculinity for Dutch teddyboys, too, was Elvis Presley, 'the working-class Southern Boy from the wrong side of town with sexy black movements' (Brake 1985: 73).

These young men did not ground their actions on higher ideals or anti-bourgeois norms, but they tried to enjoy life as much as possible in the rather unrestrained years after their school period, aware that soon the possibilities for this would vanish. They wanted to enjoy their 'freedom' for as long as possible and did not care much for the preoccupations voiced by adults who tried to make them feel more responsible for every-day business at the shop floor and for their future family. One might be tempted

to see these youngsters as precursors of a new mentality. Although these youngsters freed themselves of parental authority, in general they were to continue the older generation's way of life (Krantz & Vercrujsse: 1959: 95-97; Peet 1987: 221-222).

At the end of the fifties managers in many Dutch companies became more worried about the problem of how to motivate and integrate young unskilled and semi-skilled working men in the work situation. From the contemporary perspective of 'modern sociology' with its optimistic view on the coming integral emancipation and integration of workers in the welfare state, many of these young men were considered as "dawdingly emancipating workers". Because of the technological and organizational changes that took place (further mechanization and a beginning of 'real automatization') big categories of production workers hardly needed higher educational qualifications. Emphasis was primarily put on required behaviour qualities like responsibility, identification with the company, care, devotion, 'autonomy' (within specific industrial constraints of course), resoluteness, changeability and adaptability. The educational and industrial-pedagogical policies regarding these young people were based on American models and primarily aimed at the strengthening of the aforementioned qualities and at cultivating an adequate work mentality. The more general background of these policies were critiques of 'mass culture' which involved fears of 'rootless' and 'alienated' mass people who were supposed to be extremely susceptible to the pseudo-*Gemeinschaft* offered by totalitarian movements like Communism and Fascism (Banning 1953, Bednarik 1955, Feitsma 1958). Especially those young workers who were strongly oriented to the aforementioned mass and consumption culture were supposed to incur this risk. Paradoxally, most of these ideas were borrowed from American authors. More than once these latter were former refugees-intellectuals from Germany who had accommodated European ideas to the American setting (e.g. the neo-Freudian Erich Fromm and his American pupil David Riesman whose publications - many of which were translated into Dutch - were very popular in the Netherlands).

Industrial managers also had to find a new balance with regard to the changing social and psychological relationships between bosses and workers. A more adequate leadership style had to be created. This issue was closely related to the problem of 'industrial disciplining' which then arose due to the social security system and the labour market relations, both of which had improved greatly in the meantime. Due to the fact that the workers involved had almost no fear of becoming unemployed, their bosses and chiefs no longer had recourse to traditional disciplinary tactics. This was a more general tendency that was also signaled in various other West-European countries at the time. Their leadership style had to incorporate a more 'psychological' approach than they had previously been used to employing. Even the human relations-approach did no longer suffice. Essentially a tendency developed towards a further pedagogization of the work relationships between bosses and younger workers. Many of the young workers concerned were hardly receptive to this; frequently they offered resistance to these measures (Brown 1954: 18, Mulder 1961: 66-79).

A deviant life-style also developed among young members of the middle class. It became most distinctly manifest around the Leidseplein, a well-known picturesque square in the downtown of Amsterdam. After the Second World War, J.P. Sartre's

interpretation of existentialism had a strong impact on the mentality and life style of a group of intellectual bohemians in Paris. Around this intellectual-artistic avant garde, meeting in Quartier Latin at the Left Bank of the Seine, soon a bigger circle of trend followers developed. Dutch youngsters, who felt themselves attracted to the 'true life' with all of the violent contrasts of the French capital, were also members of this expressive movement. Most importantly, this group included the poet Simon Vinkenoog (who frequently travelled between Amsterdam and Paris) and several of his friends and followers. The existentialist philosophy of life, however, was often reduced to a few superficial elements: a praxis of 'authenticity' which turned into egocentrism and narcissistic behaviour. Moreover the culture industries soon succeeded in commercially exploiting this life-style, particularly in fashion, films and popular books. This kind of 'existentialism' easily found its way to America; his most fervent supporters over there were known as 'boheys'. In other European countries the more popular variant of existentialism was also disseminated.

In the United States the more authentic kind of existentialism became the vogue within the bohemian culture of the beats, a kind of American existentialism 'at the guts level'. This was a counter-cultural group which was strongly opposed to the American way of life in the fifties. Against the streamlined, commercialized society they promoted anarchistic individualism, 'voluntary poverty' and a liberating flight from the dullness of the moral and social system through drugs, hitchhiking and long chaotic trips by cars across the country. The ideas of Zen-Buddhism also had a big influence on their world view and way of life. At the end of the fifties, this initially small literary movement - annex subculture - grew into a broad expressive movement whose members were called 'beatniks'. A process of commercialization with regard to fashion, film and music also took place. In the United States an inextricable mixture of the existentialist and beatnik life-styles resulted which had a strong impact on young people in West-Europa; not in the least in the Netherlands. Temporary or permanent American expatriates in Holland and young Dutch American travellers functioned as intermediaries.

The culture of the Dutch beatniks, the 'Pleiners', was, in essence, a romantic revolt against the common, the petit bourgeois and the oppressive elements in the Dutch social and cultural climate in the fifties and the early sixties. It was primarily a situationally determined escape from everyday life at home and school. During their leisure time it occurred within the subculture of jazz-cellars, ice-cream parlours and art studios, while during holidays they took hitchhiking trips. Resistance was also directed against one's own (possible) tendency to conform to the welfare state and the consumption society. If there was any rebellious attitude, this was hardly in relation to parents. These young people were more opposed to contemporaries who participated in the old-fashioned youth organizations, like scouts and students' corps. They understood the Pleiner-subculture as an appeal to autonomy, experimentation with life, improvisation and risk-taking. An appeal, however, which was only very modestly acknowledged in practice, as it concerned a rather cosy domestic culture within the sheltered enclaves of cafés and jazz-cellars.

At the transition of the fifties to the sixties, role models implied in the information and images offered by the culture industries and the mass media began to play a more important role in the development and containment of self-images, social identities and life-styles among Dutch youth. My central thesis is that during this period the influence of the more traditional pedagogical discourse declined and gave way to modern pedagogical strategies of control which were - to a great extent - borrowed from American practices. Closely linked to this development were culture industries directed towards youth which began to mold young people through introducing the ideal types of 'teenager' and 'tween'. On the other hand, however, popular Anglo-American culture appealed greatly to Dutch youths and offered them some freedom, at least in the symbolic-expressive domain. Even the life-style of French existentialism as it was picked up by groups of Dutch middle-class youth, was closely intertwined with American elements, that is the subculture of the beatniks. These developments which ran partly parallel and partly contrary to each other, created tensions that dominated the social relations between young people and their significant counterparts among the older generations.

Although the former ideal types were commercialized and domesticated variants of the 'original' teddy boys and existentialists/beatniks, at the time this nevertheless meant a breakthrough in the societal relations of the Netherlands. These young people deviated from the prevailing ideology of soberness and the calvinistic ethos, which considered enjoyment and having pleasure to be very objectionable. Furthermore, these processes did not just have a one-sided influence like is supposed in critical theory of the Frankfurt School-tradition and in kindred left perspectives (Williamson 1978, Lazere 1987). It should be emphasized that the socialization of young people by the culture industry was less imperative than that of the traditional pedagogues. In the commercial market - otherwise than in the case of publicly organized services - power is based on money and not on moral authority or state power. Those who have the necessary money at their disposal can - given a certain commercially organized supply - take less notice of the prohibitions and prescriptions of a public morality. Also one did not have to buy a product and one could still turn off the radio of gramophone for instance. Besides, youth might give its own interpretation to the 'message' which was hidden in the interferences and consumption supply of the culture industry (Webster 1988: 24-26, 174-208). This also offered certain degrees of freedom and possibilities to youth. The products which were marketed enabled them to distinguish themselves from adults and peers. Also, by meeting the needs and preferences of youth, the culture industries legitimized specific elements of youth cultures: for instance, hedonism and consumerism by youth. Besides, by means of the mass media, a wider distribution of youth cultures took place. This increasingly offered young people common identification and reference points.

'To be modern' in the fifties, when the modernization of everyday life in the Netherlands pressed on forcefully, did not mean so much the liberty to choose one's own way of life, but first of all the flexibility of conforming to the constraints of the

existing social system. A fundamental supposition was that people could be allocated in multifarious ways within a functionally ordered society. This was also the dominant stance taken by 'modern sociologists' at the time (Gastelaars 1987: 155). Right at this point the more 'authentic' (not domesticated) 'artistiekelingen' and 'nozems' deviated from the rest of the population and - deliberately or not - constituted countermovements to the 'civility' of the prevailing cultural climate. The 'alternative' life-styles which were imported from abroad offered the bohemian groups and their youthful epigones the opportunities to escape temporarily and situationally from the societal order. But it was a rather small group which identified with the subcultural milieu around the Leidseplein and similar enclaves elsewhere in the Netherlands. They were labeled as 'dropouts' by the general public and the hard kernel of these young people experienced a rift between the generations. The majority, however, had rather good relations with understanding elders, not seldom also their own parents (especially in the new middle class). To the latter category also belonged the younger generation of 'progressive' educators who were aiming at a dialogical relationship between professional pedagogues and youth, within the framework of 'youth service', instead of the one-sided pedagogical steering of youth as was still practiced within traditional youth organizations. They were oriented to the more informal codes of behaviour which were implied in the 'bargaining household' within the domain of interpersonal relations which was becoming influential in this social milieu. This occurred in the more general context of the over-arching civilization process which pressed further on in these years (Wouters 1986).

To a certain extent a similar process took place with regard to the teddyboys from the working-class, be it that this did not involve intellectually articulated protests but rather an attitude against the grain which brought new forms of bodily expressions in the public sphere. This also partly involved 'oppositional' elements of a more traditional working-class culture. The relationship of these young people with their parents was, as a whole, not bad, but somewhat loose. In this social milieu, the traditional command household was also gradually dropped as a result of changing power balances between young and old (de Swaan 1982, Zeegers 1988: 156-224, du Bois-Reymond 1990).

That American symbols and role models could play such an important role, was not in the last instance a result of the decreasing influence of the more traditional pedagogical policy, in favour of more 'modern' pedagogical beliefs and practices which were mainly modeled upon American exemplars. Insofar certainly *no* depedagogization of the sphere of leisure of youths took place, but another pedagogical steering than previously. This applies especially to the middle of the road teenager culture. Another part of the explanation can be found in the circumstance that the American mass culture which became much more influential satisfied existing needs and desires of big groups of Dutch youth. The images, fantasies and myths which lay hidden in this popular culture - about a more exciting reality and a life filled with kicks; the specific content of which might be very different for various categories of youth - offered them possibilities to escape from trivial everyday life, at least in their

imagination at certain times and in certain places, in order to enjoy life intensively. Paradoxally, in this respect too, America functioned as guiding country, in these countermovements against the "disenchantment of the world" as a result of the modernization processes that took place.

CONCLUSIONS

If we compare case studies I and II, we may conclude that in both cases 'America' embodied and symbolized modernity in its broadest sense. It was a common denominator in the debates on, and responses to various kinds of modernization which were labeled as 'Americanization' of the indigenous culture. 'America' as such played a crucial role in the action patterns of various groups (differentiated according to class, generation and sex) with regard to the broader civilization process that pressed further on in the Netherlands. In this the 'simultaneity of the unsimultaneous' - particularly comprising very different social attitudes towards modernity - led to increased tensions between the groups involved, both in the 1920s and 1950s. It was only in the late fifties and early sixties, however, that generally the power balance between the younger and elder generation really began to change into a more equalitarian relationship, with the new middle class as vanguard. Among parts of the working-class also gradually a bargaining household evolved, be it that this process was certainly not free from social-psychological frictions.

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Metaphor: From a figure of speech to the force of discourse. A synopsis of the historical development of philosophical theories of metaphor

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ABSTRACT

In the methodology of the social and behavioral sciences attention is increasingly focussed on the way in which linguistic categories and rhetorical strategies mediate our experience of the world. Foremost in this regard is the "discovery" of the crucial function of metaphor, not so much as a decorative figure of speech (as it was regarded for ages) but as a vital heuristic device of language which enables us to disclose those aspects of our experience of reality which cannot be described in a literal way. In this paper the history of this shift from a philosophical understanding of metaphor as a figure of speech to metaphor as the force of discourse is traced.

It is shown that the set of presuppositions -pertaining to the philosophical understanding of language and meaning in general- which governed the traditional understanding of metaphor, had to be overcome and replaced by a different set of presuppositions before the crucial cognitive function of metaphor could be discovered. The traditional set of presuppositions, called the rhetoric of the word, and the definitions of metaphor to which it lead, are first explained with reference to the so-called substitution-, comparison- and iconic-signification theories. In each case metaphor is defined as a rhetorical trick or decorative device - exploiting some resemblance or analogy between the customary meanings of terms - with no unique cognitive impact. The opposite set of presuppositions, called the semantic of the sentence, underlying the recent understanding of metaphor as the force of discourse, is then discussed with reference to three theories of metaphor in which it found expression, namely the interaction-, controversion-, and tension theories. In each case metaphor is defined as a process of semantic interaction between terms - often on the basis of disparity rather than resemblance

or analogy - through which creative interpretation of language and the disclosure of unconceived dimensions of the world is brought about. This growing consensus within philosophy about the cognitive force of metaphor, its power to render new interpretation of our experience and disclose dimensions of reality which are not always describable by the literal use of language, is of far reaching consequence for the methodology of the human sciences. It exemplifies the way in which our experience of the human world is always mediated and structured by the means and ends of our discourse about it.

INTRODUCTION

One of the remarkable recent developments in the methodology of science in general, and more specifically in the methodology of the social and behavioral sciences, is a growing concern with and appreciation for the decisive role conceptual frameworks (like paradigms) and conceptual tools (like models) play in the mediation of our experience of reality, as well as in our theoretical understanding and explanation of that experience. Attention is increasingly focused on the interface of *language, thought and experience*, i.e. on the ways in which our conceptualisation of our experience in general are determined by the linguistic practices and/or systems of discourse to which we adhere. As a result there is a growing realisation of the epistemological and methodological relevance of previously disregarded linguistic categories and rhetorical strategies. Foremost in this regard is the "discovery" of the crucial function of metaphor, not so much as a decorative figure of speech (as it was regarded for ages) but as a vital heuristic device of language which enables us to disclose those aspects of our experience of reality which cannot be described in a literal way¹. For instance, analyses are made of different prevailing paradigms of scientific investigation, showing that they can be reduced to (and are consequently extensions of) different "root-metaphors" in terms of which a specific aspect of reality or field of research is fundamentally conceived. Similar analyses are made of the way in which metaphors are developed into extensive conceptual models in terms of which experiential data are structured and theoretical explanations inferred. Hence we have witnessed in recent years a staggering upsurge of research, especially within philosophy and the methodology of science, on the cognitive function of metaphor², attesting to the fact that metaphor should not be regarded as a mere figure of speech, a trope amongst tropes, but as the creative force of discourse through which previously unreflected dimensions of reality are disclosed within language, and whereby reality is described anew.

Against this background the aim of this paper is to trace the history of this shift from a philosophical understanding of metaphor as a stylistic *figure of speech* (fulfilling only the decorative function of saying figuratively that which can be said more accurately in literal language) to metaphor as the *force of discourse* (fulfilling

the vital cognitive function of disclosing reality on a more fundamental level than could be achieved by the description of reality in literal language.) Towards this end I will distinguish between two different, in fact quite opposite, sets of presuppositions to which most of the philosophical theories of metaphor which have been developed traditionally, as well as more recently, can be reduced, showing thereby that the presuppositions which governed the traditional understanding of metaphor as a figure of speech, had to be overcome before the crucial cognitive function of metaphor could be discovered³. Before this switch from a trivial to an epistemologically important understanding of metaphor can be demonstrated, the nature of the distinction, and the two sets of presuppositions demarcated by it, calls for a brief explanation.

The opposite sets of presuppositions underlying the traditional and the recent understanding of metaphor respectively, are namely uncovered when the following question is asked: is the basic meaningful unit of language the *word* or the *sentence*? And consequently, should metaphor be explained in terms of something that happens to (a) word(s), or as something that a sentence does? When the history of philosophical thought is examined in the light of this question, the following polarity is unveiled. Firstly, it becomes clear that the traditional approach to metaphor was determined by the assumption - pertaining not only to metaphor but to language and meaning in general - that the *word* is the basic semantic unit of language, and that metaphor should therefore be explained in terms of something peculiar - the "result" of a rhetorical trick! - that happens to words when used in metaphors. Accordingly, this traditional approach to metaphor can be called *the rhetoric of the word*. Secondly, it becomes clear that the recent appreciation of the creative force of metaphor rests on the assumption - again not only pertaining to metaphor but to language and meaning in general - that the *sentence* is the basic semantical unit of language, and that metaphor should therefore be explained in terms of a unique semantical function fulfilled by sentences when construed metaphorically. In contrast, this approach can accordingly be called *the semantic of the sentence*. A synopsis of the development and recent shift in the philosophical understanding of metaphor can thus be structured in terms of this polarity between the *rhetoric of the word* and the *semantic of the sentence*. The rhetoric of the word, governing philosophical thought on metaphor from Aristotle up until late in the twentieth century (excluding one or two exceptions), will be discussed first by referring to three theories of metaphor in which it found expression, namely the *substitution-*, *comparison-*, and *iconic-signification theories*. The semantic of the sentence, underlying the recent understanding of metaphor as the force of discourse, will then be discussed, again with reference to three theories of metaphor in which it found expression, namely the *interaction-*, *controversion-*, and *tension theories*.

Metaphor and the rhetoric of the word

The assumption that the word is the basic unit of meaning in language has dominated philosophical thinking about language and meaning from pre-Socratic days until the early twentieth century. Different philosophers, for instance Plato,

Aristotle, Augustine and Kant, proposed different theories on the ontological and epistemological status of concepts (whether they should be regarded as reflections in human consciousness of apriori "universals" in the world, or whether they should be regarded as constructions of human consciousness projected onto the world). But all of them assumed that words are *names* which refer to the *concepts* which are their fixed *meanings*, and thereby - by means of these concepts - to entities (including qualities, actions and events) in the world. The relation between language and the world was thus conceived as a relation of fixed *correspondence* between words as names and the entities named by them. Accordingly, it was assumed, as Richards (1936: 11) put it: "... that a word has a meaning of its own (ideally, only one) independent of and controlling its use and the purpose for which it should be uttered." The meaning of a sentence, and of a larger whole of language like a speech or a text, was thus understood to be the sum of the proper meanings of the names of which it was made up.

In accordance with this general assumption that language is a collection of names with proper meanings through which we name entities in the world, metaphor was explained as some peculiar way of naming in order to achieve some or other rhetorical purpose or decorative effect. Thus, Aristotle - the first philosopher to theorise about the matter - defines metaphor in his *Poetics* (1954: 1457b/5-10) as follows:

"Metaphor consists in giving a thing a name that belongs to something else; the transference being either from genus to species, or from species to genus, or from species to species, or on grounds of analogy."

Laying the basis for what later became known as the substitution theory, this definition explains metaphor as the *substitution* of one word for the proper meaning of another, a transfer made possible - as Aristotle subsequently elucidates - by some perceived *resemblance* or *analogy* between the proper meanings of the two words in case. For instance, to quote one of his examples (1954: 1457b/25-30):

"As old age (D) is to life (C), so is evening (B) to day (A). One will accordingly describe evening (B) as the 'old age of the day' (D + A)... and old age (D) as the 'evening' or 'sunset of life' (B + C)".

Important for our purpose is to note that, according to this definition, it is always possible to paraphrase metaphors back into literal statements by reinstating the originally substituted words without the literal paraphrase suffering any loss of meaning or cognitive content. In other words metaphors do not create or constitute new meaning within language, i.e. meaning which cannot be achieved by the customary, literal use of language, and therefore do not disclose and/or refer to aspects of reality which cannot be described adequately in literal statements. Consequently, metaphor was confined by Aristotle (cf. 1954: 1404b - 1405a/15) to the rhetorical category of figures of speech - amongst other tropes like synecdoche and metonymy - which serve no other purposes than that of deception (in argument) or decorum (in literature).

This underestimation of the function of metaphor as being a mere stylistic device was further entrenched by Aristotle in his *Rhetorics* (1954: 1406b/20) when he equates *metaphor* and *simile*, leading us towards the comparison theory, when he states:

"The simile.... is a metaphor, differing from it only in the way it is put, and just because it is longer it is less attractive.... A simile succeeds best when it is a converted metaphor."

It is important to note that a simile is a literal statement of comparison between the meaning of two entities, the comparison being made explicit by terms like "X *can be compared to* Y", "X *is/seems to be like* Y", "X *resembles* Y", etc. The equation of metaphor and simile thus implies that metaphor is only a syntactically abridged form of simile, i.e. a simile in which a literal statement of comparison is made albeit without stating it explicitly (cf. 1954: 1410b/15-20). This comparison theory of metaphor differs from the substitution theory in as far as it does not understand metaphor as the improper substitution of one name for another, but rather as the proper naming of a comparison. It is akin to the substitution theory however because of its reduction of metaphor to some perceived *resemblance* or *analogy* which makes the comparison possible. As in the case of the substitution theory, the comparison theory furthermore implies that it is always possible to paraphrase metaphors back into literal statements without loss of meaning or cognitive content, not by reinstating the originally substituted words, but by completing the abridged simile syntactically. Again the verdict is that metaphors do not fulfill a unique cognitive function, but only the trivial stylistic function of presenting a simile in disguise.

Up until the twentieth century the substitution and comparison theories were merely echoed in textbook definitions of metaphor⁴. It was generally accepted that metaphor should be regarded as a figure of speech, belonging therefore to the subject matter of rhetoric, and not to that of philosophy. Consequently, metaphor became a forgotten remainder, excluded from philosophical questioning, with the result that the substitution and comparison theories, and the assumptions underlying it, were never challenged within philosophical thought⁵. Even Nietzsche (cf. 1922: 287-319) - always the exception to the rule! - who rediscovered the fundamental importance of metaphor for, as well as within philosophical thinking, still adhered uncritically to the assumptions of the rhetoric of the word.

The only meaningful modification of this traditional understanding of metaphor that I could find, is the iconic-signification theory proposed by Paul Henle (1959: 173-195). Aiming to update the original Aristotelian definition in terms of a general theory of symbolism he interprets "thing" and "name" in the definition (i.e. that metaphor consists in giving a *thing* a *name* that belongs to something else) very broadly as referring respectively to *sense* (= thing) and *sign* (= name). The original "substitution" definition is thus transformed into a much broader semiotic definition in terms of which metaphor entails that a certain sense is signified not by its customary sign, but by a sign which customarily signifies another sense. This in turn is linked to a general distinction within his theory of symbolism between *signs as symbols* and *signs as icons* which entails, as Henle (1959: 177) explains, that "[a] sign is a symbol insofar as it signifies according to an arbitrary rule, insofar as it is a conventional sign... [but]

a sign is an icon to the extent that it signifies in virtue of similarity." Any sign is thus a *symbol* of its customary sense, but can also be used as an *icon*, signifying another sense (similar to its customary sense) figuratively. Thus the metaphorical sign signifies two senses, the one as a symbol and the other as an icon, namely its customary, literal sense as well as a new sense which is nevertheless the customary, literal sense of another sign. As Henle (1959: 175) puts it: "A word is an immediate sign of its literal sense and a mediate sign of its figurative sense." The new, metaphorical relation between the sign and its iconically signified figurative sense is then *mediated* by the literal sense, so that "... it is only through the literal sense that one arrives at the figurative." And again, as in the case of the substitution and comparison theories, the possibility of such a mediation is attributed to some perceived *resemblance* or *analogy* between the customary, literal sense and the mediated, figurative sense for which the metaphorical sign serves as an icon. It is further deemed possible to state this resemblance or analogy in the form of a literal comparison or simile in which the iconic relation between the metaphoric sign and its figurative sense is made explicit, thereby paraphrasing the metaphor into an ordinary literal statement without any loss of meaning or cognitive content. Consequently, in spite of its renewal of the traditional understanding of metaphor, the iconic-signification theory does not depart from the major assumption of the rhetoric of the word, namely that metaphor is merely a rhetorical trick or decorative device with no unique cognitive impact.

The recent appreciation of the cognitive force of metaphor should thus be related to a radical shift away from the traditional approach to metaphor. It could even be argued that the discovery of the unique function of metaphor was postponed for centuries because of the superficial explanation it had to receive on the basis of the inadequate assumption that language is a collection of names (or signs) with fixed proper meanings (or senses).

Metaphor and the semantic of the sentence

The identification of the sentence, rather than the word, as the basic semantic unit of language, is the result of a much broader shift which occurred during our century within the philosophy of language in general, and philosophical semantics in particular. The major contributing factors were without doubt the application (mainly within French philosophy) of the *structural linguistics* of Saussure, the reduction of philosophy (in the English-speaking world) to what became known as *linguistic analysis* by philosophers such as Wittgenstein, and the growing emphasis (in the remainder of continental thought) on the *hermeneutical nature* - being mediated by language - of human existence (i.e. "Dasein") brought about by Heidegger. Without discussing the distinct, and not at all mutually inclusive, contributions of these three developments, it can nevertheless be stated that all three, in their own way, challenged the traditional understanding of language as a collection of names, each name having its supposed "proper meaning", through which we name entities in the world. Despite respective differences, these three developments agreed that the relation between

language and the world should not be conceived as a static relation of *correspondence*, but rather as a multifarious and dynamic relation of *interaction*, caused by the multiple ways in which we use language for different communicative purposes in the world.

Consequently attention now became focused on the way(s) in which meaning is constituted in the *actual use of language*, not as an inventory of names, but as an instrument of communication and interpretation of the world. This was accompanied by the discovery of the crucial role of *context* with regard to the (interpretation of the) meaning of linguistic utterances, because it is only within the wider linguistic and experiential context that the specific use (and therefore meaning) of a linguistic utterance can and should be determined. Correlative to this dual emphasis on the *use of language* and the *contextuality of meaning*, the semantic difference between the word and the sentence was discovered, because it is only through the *act of predication*, the so-called "speech act" of a sentence, that linguistic utterances are used to perform a specific function within a context. The meaning of a sentence is therefore not the sum of the supposed "proper meanings" of the words it consists of - but rather the converse: words receive a relative semantical value according to the contributive role they play in the actual use made of a sentence within a specific context. This basic assumption of the semantics of the sentence can be summarised by quoting Benveniste's (1971: 104, 110) conclusion that: "A sentence constitutes a whole which is not reducible to the sum of its parts; the meaning inherent in this whole is distributed over the ensemble of the constituents.... The sentence, an undefined creation of limitless variety, is the very life of human speech in action... with [it] we leave language as a system of signs and enter into another universe, that of language as an instrument of communication, whose expression is discourse."

The interaction theory, formulated by Richards (1936: 89-137), is to my mind the first incidence where these insights into the contextuality of meaning and the semantic importance of the sentence were applied to a philosophical interpretation of metaphor. Assuming what he calls (1936: 32, 36) "a context theorem of meaning" which implies that "what a word means, is the missing parts of the contexts from which it draws its delegated efficacy", Richards states (1936: 96) that words could never have "proper meanings", because the meaning of linguistic utterances will always be "... resultants which we arrive at only through the interplay of the interpretative possibilities of the whole utterance." In terms of this understanding of the way in which meaning is constituted - and always constituted anew - by the interanimation of linguistic utterances in specific contexts of language-use, metaphor is defined (1936: 93) as follows:

"[W]hen we use a metaphor we have two thoughts of different things active together and supported by a single word or phrase, whose meaning is the resultant of their interaction."

The first of these "two thoughts of different things active together", namely that which the metaphor in actual fact speaks about - in Richards' words "the underlying idea or principle subject" of the metaphor - he calls the *tenor*. This tenor is however

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approached in the metaphor by means of "a thought of [a] different thing" which Richards appropriately calls the *vehicle*, because it *transfers* some of its intent onto the tenor by interacting with it to constitute the full meaning of the whole utterance. For instance, to quote one of Richards' examples (1936: 102), in the poetic mixture of metaphors:

"A stubborn and unconquerable flame
Creeps in his veins and drinks the streams of life."

The *tenor* (unmentioned in the utterance itself) is a *fever*, whereas the vehicle(s) through which it is approached is that of a *flame*, which in its turn becomes the *tenor* for a second metaphor of which the vehicle is a *drinking animal*. The point Richards wants to stress by this analysis of metaphor in terms of the irreducible interaction of tenor and vehicle, is that the meaning of any metaphor is more than just the meaning of the tenor or the vehicle taken separately, and even more than the sum of their meanings taken together, because it is a *unique meaning* which is constituted anew through their *interanimation*. Furthermore, this juxtapositioning of the tenor and the vehicle, and their resulting interanimation on a semantical level, need not be precipitated by some perceived resemblance or analogy between them. In many cases an element of resemblance between the semantical value of the two terms is rather the outcome of the new meaning created through the metaphor, than its precondition. As Richards points out (1936: 104): "In general, there are very few metaphors in which *disparities* between tenor and vehicle are not so much operative as the similarities." Accordingly, for the first time in the history of philosophical thinking about metaphor, the important claim is made that the meaning of metaphor is not reducible to an ordinary literal expression, because it achieves the creation of a unique meaning and cognitive content, which will be lost if the juxtapositioning and interaction of the disparate tenor and vehicle is paraphrased away.

This far-reaching breakthrough in the understanding and appreciation of the potential cognitive force of metaphor, was subsequently consolidated by two further modifications and refinements of Richards' interaction theory⁶. In his tension theory Black (1963: 25-47, 1980: 20-43) further emphasises the absence of any given resemblance or analogy between the "tenor" and the "vehicle" in most original metaphors, stressing thereby the creative semantic role fulfilled by the tension of the disparity between the terms of the metaphor. In this regard he introduces a further distinction, complimentary to that of "tenor" and "vehicle", namely that between the "focus" (i.e. the particular metaphoric terms) and the "frame" (i.e. the larger context in which it is embedded). Within a certain frame the semantical focus is shifted towards specific terms in the sentence which act as a filter through which that which the sentence is actually about, the disparate principle subject, is viewed. Furthermore, it is not so much the customary meanings of the terms in the metaphorical focus of the sentence which act as a filter, but rather their implied connotations or what he calls (1963: 40) "system of associated commonplaces." Metaphors are thus constituted by two distinct semantic subjects, the principle subject or "tenor" (often not mentioned

explicitly) and a subsidiary subject (the metaphorical focus of the expression) which acts within a certain frame as a filter through which the principle subject is viewed. Through this process the connotations or "associated commonplaces" of the subsidiary subject are activated and brought to bear on the "tenor", thereby forcing a link or disclosing a relation between aspects of or entities in reality which were previously inconceivable. Consequently, metaphor acts as an indispensable heuristic device in our endeavours to discover and conceptualise more of the hidden structures of our world.

Lastly, in what he calls a controversion theory, Beardsley (1958: 114-164, 1962: 293-307) introduces a further distinction between the primary and the secondary levels of meaning of most sentences: the primary level being what the sentence purports to state and the secondary level being what is implied - through the intentional use of the primary level! - between the lines by means of suggestion, irony, innuendo, etc. As he explains (1958: 122): "The suggestion [i.e. secondary meaning] is part of the full meaning of the sentence, but its presence is not felt to be as central or as basic as the primary meaning, on which it nevertheless depends." The particular nature of the primary, as well as of the secondary meaning of a specific sentence is further determined by the context which - acting as a filter - selects some possible explicit as well as implicit semantical values of the words in question, while repressing others. In the case of metaphor, according to Beardsley, these two levels of meaning interact in a unique way to enforce what he calls a "metaphorical twist" through which an inversion of the two levels is brought about. What most metaphors purportedly state on the primary level of meaning - the combination of a subject and a predicate (called by Richards the "modifier" of the metaphor) - amounts to a contradiction or an absurdity if taken literally. Terms which belong to mutually exclusive, often logically contradictory conceptual categories, are deliberately combined. The result of this "calculated category-mistake" is a collapse of meaning on the primary level, leaving a semantical vacuum so to speak, which can and must then be filled by new meaning, else the sentence will remain senseless or absurd - which is not the case with metaphors. Thus, the actual meaning of metaphors are realised through a metaphorical twist, a creative interpretation focused on the secondary level of meaning - i.e. by relating some of the *implied connotations* of the "modifier" to the subject - thereby constituting a new meaning-content for a seemingly contradictory or absurd expression. Good metaphors have no sense, but can and must always be made sense of. It is precisely in this capacity to enforce creative interpretation that metaphor's cognitive force to disclose unconceived dimensions of reality (rather than describe already familiarised aspects of reality) is revealed.

CONCLUSION

A synopsis of the history of philosophical thought on metaphor clearly shows a shift, occurring on the basis of a new understanding in the twentieth century of the function of language and the nature of meaning in general; a shift from a trivial understanding of metaphor as a decorative figure of speech to an appreciation of metaphor as a unique cognitive force of discourse. This growing consensus within philosophy about the power of metaphor to render new interpretation of our experience

and disclose dimensions of reality which are not always describable or conceivable by an unambiguous, literal use of language, is of far reaching consequences for the methodology of science in general, and more specifically for the interpretative practices that characterise the discursive structures of the social and behaviour sciences. At the very least it exemplifies the way in which our experience of the world is always mediated and structured, if not constituted and determined, by the means and ends of our discourse about it. It is therefore not inapt to conclude this paper on a truly poetic note, by quoting Shelley (quoted by Richards, 1971: 90), one of the distinguished masters of metaphor, when he points to the vital metaphorical nature of all discourse:

“Language is vitally metaphorical; that is, it marks the before unapprehended relations of things and perpetuates their apprehension, until words, which represent them, become through time, signs for portions of classes of thoughts instead of pictures of integral thoughts: and then, if no new poets would arise to create afresh the associations which have been thus disorganised, language will be dead to all the nobler purposes of human intercourse.”

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NOTES

- ¹ The following selection from the vast number of material on the subject will suffice for introductory purposes: Hester (1967), Lakoff & Johnson (1980), Knights & Cottle (1960), Leatherdale (1974), MacConnac (1976), Mooij (1976), Ortony (1980), Sacks (1978) and Wheelwright (1962, 1968)
- ² Cf. the three consecutive bibliographies on metaphor, compiled by Shibles (1971), Van Noppen (1985) and Van Noppen & Hols (1990)
- ³ This analysis of mine, and more specifically of the presuppositions underlying the history of philosophical thought on metaphor, was mainly inspired by Ricoeur's (1978) treatment of the matter
- ⁴ For an exposition of the unqualified repetition of the two Aristotelian definitions throughout history, for instance (to mention only the most influential) By Cicero, Quintillian, Hegel, Max Müller, Gustav Stern, Brinkmann and Whateley, cf. Bedell-Stanford (1936), McCall (1969) and Stutterheim (1941).
- ⁵ As Berggren (1962: 237) justly points out: "For centuries ... metaphor was considered to either nothing more than a stylistic ornament, superimposed on cognitive discourse for emotive purposes, or else a mere illustrative comparison whose possible meaning and truth could emerge only when the metaphor was reduced to literal statements. In either case the use of metaphor was traditionally considered a hindrance rather than a help to any serious cognitive pursuit..."
- ⁶ For the sake of brevity I emphasise only what is to my mind the two most important further developments. For a fuller account of other contributing authors and related theories of metaphor, cf. Berggren (1962: 238-258, 1963: 450-572), Scheffler (1979: 97-118) and Mooij (1967: 71-89).

Contributions to connection of psychoanalysis with Christian ideology

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ABSTRACT

Till now the reception of Freudism by theology was an entirely restricted area of study in Hungary. In order to perceive what kind of intellectual milieu received the theory of Freud and his followers on the side of Christian theology, in this presentation we are going to cite some conclusions from works of two representative writers, namely Mihály Marczell and László Noszlopi. Marczell draws one's attention to the ideological danger of Freudian psychoanalysis (excessively materialistic, biologically founded epistemology, neglecting spirituality as efficacy, ideological 'trash'). But the theoretical role of Freudian sublimation ('superior transposition of sexuality') and the recognition of the 'tempting' role of the unconscious are estimated positively by him. He thinks that emphasizing the 'linear achievement' of instinct drive is a mistake in psychoanalysis leading to grave theoretical and practical outcomes. He thinks that in choosing and using the notions (e.g. libido) Freud's theoretical expansion seems to be realized. Psychoanalysis by the liberation of sexual instinct, so to say, comes into antagonistic conflict with Christian ethics. According to his conviction, in psychoanalytical practice (therapy) a total revaluation in the notion of purity is taking place, which he considers as a dangerous experimentation with morality. Marczell thinks that ethics could be grateful to psychoanalysis first of all for expanding the field of pathological conditions and phenomena, and on the other hand for urging their treatment, even if it is done incorrectly. In his opinion the technique of mind-counsellor using a Catholic method was a precursor of the psychoanalytic method. Psychoanalysis seemed to be justified in the property of his own treatment.

In his work László Noszlopi makes an effort to prove that psychoanalysis belongs directly to Christian psychology. According to Noszlopi, on the basis of Freudian theory one could state that only those wishes not really

given up by us come back as symptoms.

He thinks that if all phenomena are explained by sexual instinct by some followers of psychoanalysis, then this interpretation can be valuable only regarding the character of the interpreter.

Till now the reception of Freudism by theology was an entirely restricted area of study in Hungary¹. From the extremely rich material² we can only cite some conclusions from works by two representative writers, Mihály Marczell (on the Catholics' side) and László Noszlopi (on the Protestants' side) in order to somewhat perceive what kind of intellectual milieu received the theory of Freud and his followers on the side of Christian theology, which vehemently took note, reacted, criticized and in some aspects accepted the results of psychoanalysis³.

A summary and a high estimation of psychoanalytical results from a religious point of view is given by Mihály Marczell. In his three studies published in the periodical review *Theology* ⁴ he summarizes his objections from the standpoint Christian ethics. He interprets psychoanalysis as a considerably materialistic, biologically founded epistemology. According to Marczell, although Freud⁵, as compared with G. Lombroso⁶, O. Weininger⁷, P. Janet⁸, Krafft-Ebing⁹, who are representatives of extreme biological determinism, professes a subjective judgement, in spite of all only in his (and in Weininger's) works can one discover that view in which the physiologically founded system retracts the value of mind, in which personality as an active component fades away and only the primitive drive remains influencing the formation of life; and this drive is **sexuality** and its dynamics. As understood by Freud the question of sexual motivation cannot be examined within the frameworks of guilt and virtue, because this is a nature-determined need, bursting into overwhelming force ¹⁰

The theoretical role of Freudian sublimation ('superior transposition of sexuality')¹¹ is estimated positively by Marczell, but in connection with gender certain distinctions must be made. According to this, the compass of anatomical, physiological and biochemical determinations come from God, one cannot change them. But the way one treats, uses (or uses up) one's own physical realities - well, this is not particularly determined in real life. The spiritual component in decisions of will can change the manifestation of instinctive drives. The arousal of sexual motivation may be 'planned', one is not totally at the mercy of unconscious will. Thought reaching the conscious can be expelled, transformed by another thought. If the will exerts prompts to gratification this may happen rather with a pathological personality background.

In ordinary cases with the quiver of physical will nearly simultaneously also come ethical, moral contents in the mind which control the act. These inhibitions are at least so inherently part of spiritual contents preceding human acts as in the case of physical processes the signs of instinct drives. In the acts of sexual life, like other types of acts, understanding, consideration, contribution, decision, maybe opposition, refusal become crucial between will and reason¹².

Arguing for Catholic ethics¹³ Marczell cannot think possible that a merely anatomically physiologically and materially founded research is able to reach valid consequences without metaphysical facts.¹⁴ According to his opinion the truth is represented by Saint Thomas, who says that the spirit acts both with and without the body. Also on the basis of St. Thomas' ethics, the writer states in his study published in 1938¹⁵ that human acts may become valuable if they are able to rise from the influence of complex determination, and free from pressure manifest themselves on the basis of free-will.

According to Marczell's interpretation Freudian psychoanalysis takes **experience**, the effect of the past as a dynamic drive of the present act. The unconscious, memories 'being prevented to break through from the deep of life', determine the present, but at least play the most important role in the actual act.¹⁶ In his opinion the question is not whether the unconscious exists, but whether the act goes beyond the human freedom, the subjective free-will by its effect. Although on the basis of a mechanistic or physical view the drives exerting themselves towards certain aims can be received as drives becoming effective without being prevented, a serious fault in psychoanalysis is, according to Marczell, that the 'linear achievement' of instinct drives is emphasized. It cannot be accepted that sexual instinct-drives are necessarily the causes of psychosis because of suppression. In his view of natural science, Freud logically makes a mistake: from a single, real, but pathological case he creates his theory in an inductive way without any cause or experience. According to Marczell, this expansion seems to be reflected in the choosing of notions. The meaning of libido (a word of Latin origin) is the feeling of instinctive gratification, 'sensual instinct-murmuring', but Freud gives a wider sense to it: all drives which are useful for life. Also the deceiving use of notion contributes to the belief that the symptoms observed in persons with sexual disorders are universal, the primitive cause of bodily and spiritual phenomena.

In his 1939 study¹⁷, Marczell calls attention to the fact that the ethical standpoint in Freudism is a consequence of ideological 'trash', that with the notion of 'drive-flood' he wants to give a final interpretation for all human phenomena. He touches upon the connection between psychoanalysis and sexual instruction. He thinks this is especially dangerous 'if the analyst insists in drawing practical outcomes when treating a person'.¹⁸ The psychoanalytical science for liberation in instinctive life intends to eliminate the pathological effects of self-restraint, but in so doing, so to speak, it comes into antagonistic conflict with Christian ethics, e.g. purity before and in marriage, about sin, especially the mortal-sin.¹⁹

Biological materialism and the notion of instinct in psychoanalysis establish the ideology by which material (and the natural life-instincts being like derivation) is the directing principle in life and the finding of human substance is the natural (instinctive) life. Although according to Marczell's knowledge the field of psychoanalysis is divided, and there are people who admit the metaphysical connections and the spirit's substantial reality, Freud and his followers deal only with 'perceptible' phenomena. On the basis of this, the aim is the achievement of happiness in the earthly life, the

balance in bodily and spiritual processes. The prevention of illness also become an institutional aim, this could be insisted on by coeducation and sexual instruction.

According to the Catholic standpoint, the body and physical life are holy (holy manner of living), but this is not holy materialization. From the Christian point of view life is a double burden: Christian achievement in personal life and life in the future, that is reproduction. But the latter is rather a secondary task while the former is a hard one only people with a strong spirit are able to do by the grace of God. In Marczell's opinion less psychoanalysts would be needed if people were prepared from their childhood for inner discipline, for 'living in freedom like God's sons'. But modern social customs (e.g. the style of dressing), the arts and literature contribute to the spreading of neurosis, because trying to wake instincts harm young people growing up.

According to his convictions, in psychoanalytical practice (therapy) there is a total reassessment of the notion of purity, which he considers as a dangerous experimentation with morality. Marczell thinks that a revision of purity may be occasionally admitted (in a concrete pathological case), but only if this is useful for being distant from sin, not for inducing it.

Arguing against psychoanalytical therapy he remarks that while the discovery of the past is going on, e.g. in the treatment of sexual neurasthenia, the content in the unconscious brought up has elements which are sexually non-neutral for either the patient or the therapist (or maybe both of them)²⁰. From Christian point of view it would be better if the process of discovery was seen only as a basic work and purification was expected from grace by forgiveness. But the discovering process remains effective psychologically only when the patient perceives that his secret is in the hands of a God-substitute. According to Marczell the other important component of successful therapy is that the patient, the psychically crushed person, acknowledges his own fault; he has to realize his prodigal-son nature and must want to recover what he has lost -only in this way can he wait for God's manifestation through the act of supporting professionals. If the patient sees that he has to face the purpose of God in his history then he can conclude that after closing the past he must resurrect. His life and emotional functions are restructured by this, and this what the psychoanalyst cannot give the suffering person.²¹

From the 30s Marczell seems to discover within psychoanalysis that some tendencies emphasize the role of spirituality as being substantial in comparison with instinctive drives, and also that sublimation has a greater scope as in Freudian theory.²² Beyond all this Marczell thinks that ethics could be grateful to psychoanalysis, first of all, for expanding the field of pathological conditions and phenomena and, furthermore, for urging their treatment, even if it is done incorrectly. The study could contribute to a more exact ethical estimation of the person and to the development of ethical rearing in Christian pedagogy. With the help of the Freudian thesis Catholic ethics realized that 'the obtained experience', the present past and also instinct might be the sources of temptation. For the sake of release from sin the theologian is ready

to admit that the misuse of suppression ('repressed drive') might result in a pathological condition. So on the basis of results in modern sciences psychological support and treatment can and must be changed also in Christian relations.²³ The theologian has to follow the past with attention as a force waiting for interpretation, this is also the centre of the attention of some tendencies in psychoanalysis, and this could raise points of view which are adequate for people with transcendental thinking.²⁴ Marzcell thinks that, from the point of view of treatment and the elimination of sin, psychoanalysis and the Christian tendency have common elements. Both adopt guiding and exemption (forgiveness and confession are Catholic methods) and the basis of these is discussion. In his opinion the technique of the mind-counsellor using a Catholic method was a precursor of the psychoanalytic method. Psychoanalysis seemed to be justified in its methods of treatment.

Among the period works there are some studies which accept psychoanalysis more favorably than in the aforementioned case. One of this works is excellent, the writing of László Noszlopi²⁵, in which he makes an effort to prove that psychoanalysis belongs directly to Christian psychology.

According to Noszlopi the Freudian theory of personality is dualistic (the aim of sexual instinct is gratification and the aim of the self is adaptation to the limits of social coexistence). He extracts from the Freudian theories concerning the development of neurosis, that the phase of instinct-life is important (it can be fixated on a certain level or can be regressed to). Yet it is important what quantity of libido can be tolerated or sublimated by personality.

According to Noszlopi, from this idea a really Christian psychology can be created. If the conscious part of personality (Ego) is symbolised by a large clear room and a dark back larder is connected with this (Id) - there is a door between the two - , then there are several possibilities concerning the two parts of personality. When there are irritations in the room which cannot be tolerated or accepted by the tenant, he can either throw them out the window or close them into the larder, but the latter can tempt the tenant, e.g. in the form of dreams. The first solution is renouncement, the second one is only a Philistine solution, the insincerity of the self toward himself. For Noszlopi this kind of compromise, Phariseism, is an unconscious event, so the personality makes an effort to preserve an appearance of sinlessness. 'But at this point Freudism, that seemed earlier an enemy of the Christian idea, leads in some way to Christian psychology. (...) Freudians proclaiming the gratification of suppressed sexual drives and sexuality without any ethical obstacle are superficial. Freudism itself does not say this. Like the Christians, it proclaims that it is true purity the real renouncement, in opposition to prudery, but it interprets all this in its own way.'²⁶

According to Noszlopi, on the basis of Freudian theory one could state that only those wishes which are not really given up do come back as symptoms; there is a difference between wanting purity and wanting to be pure. He thinks that if all phenomena are explained with the help of sexual instinct by some followers of psychoanalysis, then this interpretation can be valuable only regarding the character

of the interpreter. For Noszlopi psychoanalysis represents the struggle of the human being against himself for honor, sincerity, for the concealment of the spirit-mask. Freudian theory is correct from a Catholic standpoint, it may be accepted as to the development of neurosis and depression depression is concerned. These disorders are originated really from the conflict between mind and instinct. But according to theology, the solution is different. It does not accept the gratification of sexuality as solution because thus the conflict remains unsolved; on the contrary, it takes a stand against material being on a supernatural ground. In metaphysics theology discovers the nobility of the physical, material dimension of man. The solution of the conflict between instinct and mind is the subordination of instinct to God's will. This obedience leads to patience towards oneself. Neurosis is the impatience itself; impatient hatred because of the demands of instinct and impatience due to inability to wait for the gratification of instinct. According to Noszlopi not only in the case of Freud but also in Adler's theory there is a conscious tendency to draw psychology near to the Christian point of view. On the one hand, one is inclined to social cooperation, and on the other hand one exerts oneself for individual success. The connection between the two ambitions, the constitutional, physical construction (maybe deficiencies) determine character and fate. If one's self-estimation is low because of any deficiency, then one either withdraws from the struggle or tries to recover one's self-respect by compensation. The lower the self-esteem is, the greater the effort to exert oneself, if necessary against social behavior. But if the low self-estimation remains, if one has no success in compensating it, then the individual goes into psychological disorder.

For the sake of recovery one must give up the motivation exerting domination. Self-preservation of the self must be overcome, the self must be denied by the person and cooperation with others must be chosen. This is the Christian thesis itself according to which arrogance is the main evil, the excess of the self - as opposed to love and humility. Christian love is heroic, self-sacrificing, because one's own self is not the most important. Arrogance is cowardly because it fears for itself, doesn't admit anything, doesn't serve. According to Noszlopi, both Adler and Freud arrive at the Christian theory: devotion, denial of the self are the only way to rebirth and permanence of the self.

REFERENCES

1. About 4-5 years ago an exclusive enthusiastic undertaking started by Hungarian psychologists who would like to work out both globally and in detail the connection between Hungarian intellectual existence and psychoanalysis. (About this work is the study written by F.Erős, I.Kapás, Gy.Kiss, P.G.Spongherot: 'Ferenczi Sándor és a Budapesti Egyetem 1918-19-ben. Pszichológia 1987/4 sz. p. 584-592.) The theme of the study: Psychoanalysis and intellectual existence in Hungary. The study being led by Ferenc Erős, who is the principal contributor in the Institute for Psychology, helped by the Soros Foundation.

2. Unfortunately a repertorium had not been made about the clerical scientific reviews, so the systemic elaboration was begun with the survey of about 40-50 reviews, that is about 20-25 volumes.
3. Till now mainly Catholic reviews were elaborated, but the elaboration of Protestant and Jewish literature is under way.
4. Marczell Mihály, Nem és erkölcs. *Theológia*, 1940, p. 8-16, 97-104; Marczell Mihály, Az élmény és a cselekedetek erkölcsisége. *Theológia*, 1938, p. 215-227; Marczell Mihály, A lélekelemzés katolikus erkölcs-tudományi megítélése. *Theológia*, 1939, p. 28-45.
5. S. Freud, *Das Ich und das Es*. Wien, 1923; S. Freud, *Über Psychoanalyse*. Deutriche, Leipzig und Wien, 1910.
6. C. Lombroso, A női lélek. Transl. ifj. Moravcsik Gyuláné Budapest, é.n.
7. O. Weininger, Nem és jellem. Transl. Gábor A. Budapest, é.n.
8. P. Janet, L'automatisme psychologique. Paris, 1889.
9. R. Krafft-Ebing, Psychopathia sexualis. Transl. Fischer I. Budapest, 1894.
10. Psychoanalytical studies cited by Marczell: N. Wohlhein, *Psychoanalyse und Kindergarten*. Wien, 1930. Bálint A.: *A gyermekszoba pszichológiója*. Budapest, 1931. Representatives of this view think that the degree of constitutional instinct- determination could be seen in all manifestations of individual. E.g. the intensity of the ambition to work, the will to succeed and humility depend on the construction of the instinc-drive. This refers to the degree of maternal love and the intensity of the religious experience. E. Fischer: *Der relig. Komplex im Kindertraum*. Stuttgart, 1929. S. Freud: *Az álomról*. Transl. dr. Ferenczi Sándor, Budapest, 1915. Dick Manó.
11. Marczell Mihály: Nem és erkölcs. *Theológia*, 1940, p. 97.
12. 'Eros is the starting Ethos regulating the human. (...) The post-actional conscious and also emotionality suggest that in the birth of actions the role of responsibility is strong. The successful effort produces recognition while the unsuccessful loosening guilt and confession.' Marczell Mihály: Nem és erkölcs. *Theológia*, 1940, p. 101.
13. Further argument against psychoanalysis, that sexuality for sexuality's sake looks for more and more perverse ways to gratification, that is it is generated by itself. When the aim of sexuality is not to achieve pleasure but to reproduce oneself, then the apparatus operating by nature after achieving its purpose (without permanent stimulation) relaxes with gratification.
14. Reference to Schütz Antal, *Charakterológia és aristotelesi metafizika*. Budapest, 1928.
15. Marczell Mihály: Az élmény és a cselekedetek erkölcsisége. *Theológia*, 1938. V./3.sz. p. 215-227.
16. According to the theologian the psycho-dinamical experience, which is a live drive-mass, is not merely a product of thinking, a content of memory or the recalled past event. All this is influenced by the intelligence, the will and the emotions of the human being. He thinks important to emphasize the differences: The complex (emotional) impressions of the past, the remained remembrance of past events, differs totally from suppressed sexual impulses (Marczell Mihály: Az élmény és a cselekedetek erkölcsisége. *Theológia*, 1938. p. 219).
17. Marczell Mihály: A lélekelemzés katolikus erkölcstudományi megismerése. *Theológia*, 1939. p. 28-45.
18. At the same place, p. 33.
19. Marczell admits that the scientic cognition of humans and the intention to make life better are lofty values, worth sacrifice and risk. There is an ethical responsibility to decide how

and who is able to assume the invasion of instinct against society. Freudism attacks society for its lax morality and the liberation of instincts; thereby a question may be raised: May circles with no moral force be attacked?

20. Under the circumstances therapy cannot achieve its aim, the satisfaction of the spirit, especially in the individual with a pathological function of the spirit.
21. In Marczell comparison, the psychoanalyst is like a musician tuning some physiological and psychological registers, thus modifying the music of life. But the registers can only be modified by the Creator.
22. Marczell mentions the work of J. Donat (*Über Psychoanalyse und Individual psychologie*. Innsbruck, 1932) as an illustration of these tendencies.
23. By way of illustration he mentions that pathological self-tormentors (flagellants) have extreme sexual fantasies, and the lash itself is an instrument for perverse gratification. Freudism points out to the psychological basis of this pathological condition, and Christian regulation is ready to change this.
24. Theoretical surveys of modern tendencies are mentioned by the writer in Boda István: A "tudattalan" problémája és spekulációs veszélyei. *Magyar Pszichológiai Szemle* III, 1930.
25. Noszlopi László, A pszichoanalízis közeledése a keresztény lélektanhoz. *Katolikus Szemle*, 1935. p. 129-138.
26. At the same place, p. 131.

Un rédacteur pour le Journal de Psychologie Normale et Pathologique après la Première Guerre Mondiale: I. Meyerson.

Faiblesse et Force de la Psychologie dans l'Entre-deux-guerres

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ABSTRACT

The French scientific society of psychologists was founded in 1901 and published in 1904 the first issue of its review, the Journal de psychologie normale et pathologique. For some years, the journal was primarily devoted to mental disorders. After the first world war, Ignace Meyerson became its editor. He controlled it until his death, in 1983. The wide variety of human sciences included in the review attests to its broad scope: ethnology, linguistics, aesthetics and so on. We relate these orientations to the specific interests of its editor.

La fin du siècle dernier et les premières années de ce siècle, qui correspondent aux années de la "naissance" de la Psychologie scientifique sont relativement étudiées. En revanche, les années faisant suite à la guerre sont encore malheureusement assez mal connues. La situation de la Psychologie en France est alors paradoxale: la Psychologie est en effet en pleine gloire avec une situation institutionnelle et scientifique très forte. Elle constitue un phare pour les enseignants et les chercheurs. Elle attire les étudiants de philosophie et de médecine. En même temps elle a une position très fragile, liée précisément à son extension et à sa forte aura. Ceux qu'elle attire vont créer d'autres disciplines. Le personnel académique en psychologie vieillit, et la Sorbonne n'a pas de réelle autonomie pour en recruter et en former de nouveau, ni pour décider dans quelle direction le faire. Le Journal de Psychologie Normale et Pathologique est un bon analyseur de cette situation paradoxale, par l'orientation qui s'y développe.

Créé en 1904 par Georges Dumas et Pierre Janet pour devenir l'organe de la Société de Psychologie, le Journal de Psychologie Normale et Pathologique voit sa parution, jusqu'alors régulière, interrompue pendant la première guerre mondiale. Son secrétaire, l'aliéniste J. Dagnan, meurt à la guerre ainsi que de nombreux collaborateurs.

Le Journal reparaît en 1920, relancé par Ignace Meyerson à la demande de Pierre Janet. Son projet demeure inchangé; la vocation du Journal est de présenter "une revue générale des travaux européens ou américains des différents chapitres et des applications de la psychologie normale, pathologique, physiologique, sociale, littéraire, pédagogique, religieuse, supra-normale etc..." (*J.P.N.P.*, 1920, p.2). "Les recherches effectuées par les psychologues de laboratoire seront présentées aux médecins et aliénistes en particulier, et les psychologues de laboratoire y trouveront des résumés des observations pathologiques" (id, p.3).

L'espace de la psychologie d'alors ne peut être plus clairement défini; il est occupé par des aliénistes d'une part et quelques chercheurs de laboratoire d'autre part².

Bien au-delà de l'époque à laquelle il cesse d'être l'organe de la Société Française de Psychologie (en 1951), le Journal va constituer, de fait, en France, la revue phare de la psychologie en train de se faire. Certes il existe d'autres revues strictement psychologiques, et, en particulier, depuis 1894, *l'Année Psychologique*. Mais la situation d'alors de cette dernière ne peut lui donner la même portée. Revue technique du laboratoire de psychologie physiologique (dit "de la Sorbonne") elle est dirigée, depuis la mort de Binet, par le psycho-physiologiste Henri Piéron. Celui-ci infléchit, par rapport à ce qu'elle était quand Binet la dirigeait, l'orientation de *l'Année Psychologique* dans la direction du courant qu'il développe. Ce courant, s'il ne peut être qualifié de marginal, n'en est pas moins d'assez faible extension en France dans les années 20-30; on peut probablement avancer que ce sera surtout au travers de ses analyses bibliographiques considérables que *l'Année Psychologique* aura une portée générale à cette époque³.

1. A la veille de la guerre 14-18, en France, et surtout bien entendu à Paris, les psychologues et la psychologie sont déjà relativement bien organisés en une discipline sinon autonome par rapport à la philosophie et à son enseignement, du moins spécifique:

- une chaire au collège de France, occupée par Janet, succédant à Ribot,
- la chaire de psychologie de la licence de philosophie, qui va être occupée par Henri Delacroix pendant toute la période qui nous intéresse.
- une chaire nouvelle de psychologie expérimentale (pathologique) à la Sorbonne, en Faculté des lettres, attribuée à Georges Dumas.
- des laboratoires rattachés à la section Sciences Naturelles de l'Ecole des Hautes Etudes,
- une société savante: la Société de Psychologie, forte de 40 membres titulaires et de son Journal, sans oublier le soutien de la "Revue philosophique" créée en 1876 par Ribot, maintenant dirigée par Lévy Bruhl, et sans compter toute une série d'institutions et de revues très largement qualifiées de psychologiques et organisées autour des travaux qui perdurent sur les questions de suggestion et d'hypnose.

Des pôles qui avaient émergé dès la fin du siècle semblent affaiblis: la psychologie individuelle et la psychologie de l'enfant, par la mort de Binet; la psychologie sociale par la mort de Tarde et les coups de boutoirs de l'Ecole

Sociologique Durkheimienne. La situation institutionnelle de la psychologie se trouve ainsi de fait encore très largement organisée autour de la seule psychologie pathologique et de la philosophie, d'autant que la psychologie est rattachée à la Faculté des Lettres. Les laboratoires de la 3ème section de l'EPHE (Ecole Pratique des Hautes Etudes), sans beaucoup de moyens, n'ont pas encore connu, par leurs travaux, de grande expansion. La définition majeure des psychologues est alors d'être des médecins philosophes.

Mais la masse des données et les champs qualifiés alors de psychologiques, débordent néanmoins très largement la seule psychologie pathologique, si bien qu'aucune synthèse de la psychologie autour des données pathologiques et de leurs analyses ne semble plus désormais possible. Néanmoins, des lignes de force l'organisent. C'est ainsi que Dumas, sous l'impulsion de Ribot, peut mettre en oeuvre la réalisation d'un traité collectif qui fera autorité longtemps encore après la seconde guerre mondiale. L'examen de la composition de ce traité, qui sera publié après la première guerre, mais qui était largement réalisé dès 1914, et du Nouveau Traité qui correspond à une reprise complète du premier dans les années 30, montre clairement qu'un axe d'analyse permet l'ordonnancement de l'ensemble de la psychologie d'alors, qu'elle soit normale ou pathologique.

Cet axe part de la physiologie et se termine dans la sociologie. Les objets psychologiques traditionnels, les facultés, n'occupent plus une place centrale. A la suite des travaux de Janet (via Ribot, via Jackson), ces objets sont largement déconstruits, dans le cadre de la hiérarchisation des fonctionnements psychiques en processus inférieurs, souci de la première époque (psychophysique, psychophysiologie), et synthèses mentales supérieures. Si les processus inférieurs sont très largement tributaires de déterminations physiologiques, les synthèses mentales le sont de spécifications sociologiques. S'il semble que les recherches d'ordre psychophysiologique ne soulèvent plus de débats majeurs qui marqueraient alors d'une manière décisive l'ensemble de la dynamique de la psychologie, comme ce fut le cas de la question du parallélisme psycho-physiologique, cela semble loin d'être celui de l'approche psychologique des processus supérieurs, tels que la pensée et les activités intellectuelles par exemple, qui occupent très largement le devant de la scène.

Au plan institutionnel, c'est ce pôle de la psychologie, compte-tenu des rapports privilégiés qu'il entretient avec la philosophie, qui est le mieux installé et qui, par là même, va se trouver en butte aux poussées les plus novatrices de la philosophie, et des sciences sociales, qui s'en autonomisent. Au plan du contenu, c'est la question de la nature de la spécificité psychologique de ce pôle qui va occuper très largement la scène de la Société de Psychologie et de son Journal. C'est cette question de la "nature psychologique" des processus supérieurs qui, s'éloignant du primat méthodologique de la psychologie pathologique et de la psychologie physiologique, va donner sa dynamique à la psychologie de l'Entre-deux-guerres en France. C'est pour cela qu'il est indispensable de prêter une attention privilégiée au Journal de Psychologie et à son rédacteur, Ignace Meyerson, également secrétaire de la Société de Psychologie, parce

que le journal traduit précisément et soutient résolument toute cette problématique et sa dynamique, et que cette orientation est pratiquement l'oeuvre du seul Meyerson.

Selon nous, si ce pôle exprime les problématiques de la psychologie académique dans des termes spécifiques, très différents de ceux que porte à la même époque le développement de la psychologie appliquée que met en place H. Piéron en doublant ainsi sa psychologie physiologique, c'est que la psychologie s'y trouve directement confrontée aux problèmes qui travaillent alors la logique (d'Emile Meyerson et de Léon Brunswick), à l'épistémologie naissante et aux sciences humaines qui se développent à cette période magistralement: l'ethnologie, la linguistique, l'esthétique en particulier, sans oublier bien entendu la psychologie génétique de Wallon et Piaget. Le Journal de Psychologie dirigé par I. Meyerson va être très largement le reflet de ces échanges. C'est lui qui ouvre et soutient ces confrontations sur la spécification des objets propres de la psychologie et présente les méthodologies les plus heuristiques. A cette époque, travaillant au développement de la psychologie de la pensée, des fonctions, du langage et des oeuvres humaines, il analyse, par exemple, la différence et la ressemblance de celle-ci avec la psychologie des singes. Ce qui le conduit à privilégier l'aspect génétique et symbolique dans cette construction de la pensée chez l'enfant et dans le fonctionnement psychologique en général.

L'examen des articles du Journal dans l'Entre-deux-guerres montre que l'organe de la Société de Psychologie est bien en accord avec les courants qui organisent alors les nouvelles sciences humaines: le comparatisme et le structuralisme. Et leurs tenants, membres du réseau d'amitiés, et des centres d'intérêt de Meyerson lui-même, correspondent si bien aux orientations du "Journal" dans l'Entre-deux-guerres et plus encore après 1948, qu'il est difficile de faire l'étude de l'un sans porter une attention toute particulière à l'autre.

2. Ignace Meyerson est aujourd'hui pratiquement inconnu, même en France, alors qu'il a consacré à son pays d'adoption plus de 60 ans de vie professionnelle. Pourtant, compte tenu de la position qu'il occupe en psychologie, de ses fonctions de rédacteur du Journal pendant 30 ans et de celles de secrétaire général de la Société de Psychologie jusqu'en 1939, sa création de la psychologie comparative historique pendant les 30 années suivant sa thèse, en font un des maîtres à penser de la psychologie française; et loin d'en avoir été seulement un témoin privilégié, il ne peut être considéré probablement autrement qu'un de ses maillons majeurs. D'autant qu'à partir de la seconde guerre mondiale, par sa thèse "Les fonctions psychologiques et les oeuvres", qu'il soutient et publie en 1947; par ses fonctions de directeur d'étude en "psychologie historique", poste créé pour lui en 1951 par les "nouveaux" historiens des annales, au sein de la 6ème section de l'Ecole des Hautes Etudes qui vient d'être créée; grâce au Centre qu'il y crée de "Psychologie comparative historique" enfin, il est à l'origine de la prise en compte systématique, par la psychologie elle-même, des dimensions historiques et sociales qui construisent la pensée humaine, ses formes et ses fonctions; et c'est pour cela qu'il a préconisé que les "oeuvres des humains" soient considérées comme des objets d'études pertinents pour la psychologie.

L'élaboration par Meyerson de l'historicité des fonctions mentales et de leurs oeuvres en retour, constitue une nouvelle orientation pour la psychologie, fruit du travail effectué sur la base de sa direction du Journal, qu'il esquisse dans sa thèse et développera dans son centre et dans ses cours de psychologie comparative. Si cette orientation n'a pas été reprise par la psychologie académique de l'après guerre ce peut-être que celle-ci était attirée par d'autres enjeux (comme la professionnalisation de la psychologie et la mise en place institutionnelle des relations entre psychologie expérimentale et psychologie clinique). Pourtant il est sans conteste que cette psychologie comparative a contribué au développement de la psychologie génétique, et a fécondé le grand courant français d'anthropologie historique représenté par Vernant. Les orientations et les travaux de bon nombre de chercheurs et intellectuels doivent beaucoup à Meyerson; ceux-ci, sur la base de la psychologie qu'il a développée dans ses cours, produiront de nouvelles approches dans les sciences humaines et psychologiques, approches dont la valeur heuristique concerne en particulier le développement de leurs relations mutuelles.

2.1. Né à Varsovie, dans une famille de médecins, en Janvier 1888, Ignace Meyerson doit quitter la Pologne à 17 ans, en 1905, à la suite de sa participation à l'insurrection russo-polonaise. Il arrive à Paris en 1907. Il y retrouve son oncle, le philosophe Emile Meyerson (dont l'ouvrage d'historien des idées sur "L'identité" aura d'abord une grande influence sur son neveu, avant que ce dernier ne reconsidère cette thèse, à la lumière des ouvrages de Lévy Bruhl (1917-1921) sur la mentalité primitive et de la méthode à la fois historique, culturelle et structurale que son approche préconise).

De 1907 à 1914, soit de 19 à 23 ans, Ignace Meyerson prépare une licence de Sciences, suit des études de médecine puis de philosophie (il travaille d'une part en neurologie, avec Babinski, d'autre part au laboratoire de physiologie de la Sorbonne et du Muséum de Louis Lapicque). Il est donc à la fois philosophe, médecin, physiologiste, ce qui fait de lui un psychologue la fois exemplaire et "new look" pour l'époque. En outre, il est très ouvert aux sciences sociales d'alors: il suit les cours de sociologie du durkheimien Simiand; il est aussi en relations suivies et amicales avec l'important bibliothécaire de l'Ecole Normale Supérieure, Herr, les historiens (Seignobos, Berr), et les ethnologues (Mauss, Granet) et fréquente assidûment les artistes - comme il le fera tout au long de sa très longue et très active vie; il meurt en 1983, ayant arrêté son enseignement juste quelques mois avant de mourir.

Toujours polonais lors de la déclaration de guerre en 1914, il s'engage comme médecin à la Légion Etrangère, mais est réformé en 1915 pour raisons de santé. Fin 1915, il est admis comme interne à la Salpêtrière dans les services de Chaslin et Najeotte, en remplacement de Wallon affecté au front. En 1916, il suit les enseignements du philosophe-psychologue Henri Delacroix à la Sorbonne, et c'est par l'intermédiaire de ce dernier qu'il rencontrera Pierre Janet et Georges Dumas. Dumas est, à cette époque, non seulement professeur à la Sorbonne mais aussi directeur du laboratoire EPHE de psychologie de l'asile Ste Anne. Il engage Meyerson comme assistant-chef-

de-travaux. C'est alors que Janet le charge de relancer le Journal, qui ne paraît plus depuis le début de la guerre (pour cet effort, Meyerson obtiendra un prix de l'Académie des Sciences). En 1919 Meyerson est élu secrétaire de la Société de Psychologie, conformément à la règle qui lie cette dernière au Journal.

Il est nommé en 1921, à la demande de Piéron et Delacroix, au nom du comité directeur de l'Institut de Psychologie qu'ils viennent de créer, secrétaire de cet institut et chef de travaux au laboratoire de psychologie physiologique que dirige Piéron. Il commence en 1922, à la demande de Dumas et de l'éditeur Alcan, la traduction de la "Traumdeutung" de Freud, qui paraîtra chez Alcan en 1926, sous le titre de "La science des rêves". Naturalisé français en 1923, il peut enfin avoir un poste officiel. Il est nommé directeur-adjoint du laboratoire de Piéron, poste qu'il conservera, en titre, jusqu'à sa nomination comme professeur de psychologie à Toulouse en 1947, poste qu'il occupera jusqu'à sa nomination, en 1951, à la 6ème section de l'Ecole des Hautes Etudes.

Si sa contribution scientifique, entendue comme contribution au recueil de données, paraît à certains réduite, l'ensemble de ses articles sur les "images", sur "l'instrumentation chez les singes", rédigés avec Paul Guillaume à partir de leurs expérimentations réalisées à la ménagerie du Muséum et à l'Institut Pasteur de 1930 à 1937, sont jugés, dès cette époque, remarquables, de même que les cours qu'il assure à la Sorbonne en remplacement de Delacroix, très occupé par ses charges de doyen, ainsi que ceux qu'il assure à l'Institut de Psychologie; sa direction de la Société de Psychologie, ses présentations d'ouvrages et d'articles dans le Journal, ainsi que les numéros thématiques ou spéciaux de ce même Journal, enfin, lui donnent une stature toute particulière dans la psychologie de l'époque.

L'énergie et l'intérêt qu'il consacre à la rédaction du Journal surpassent ceux qu'il accorde à ses cours à la 6ème section de l'EPHE. La direction qu'il lui donne est claire. Ce sont, d'une part, ses intérêts pour l'histoire (Berr, Seignobos), l'ethnologie (Mauss, Rivet, Granet), la linguistique (Meillet, Brunot, Bally), la logique (Brunswick), et surtout l'esthétique (Lalo), et, d'autre part, son exceptionnel réseau d'amitiés (Delacroix, Blondel, Piaget, Wallon, Mauss, Granet, Cassirer, Bash, Lalo, Bally, Vendryes, Meillet, Luquet, Parodi, Kostyleff, Spaier, Masson Oursel...), et, enfin, ses intérêts pour la psychologie de la forme (Kofka, Köhler, Lewin, Katz, Aron, Gurwitsch...) qui rendent compte de la qualité et de la portée incontestable du Journal de Psychologie.

Aussi faut-il certainement voir dans le fait que l'ethnologue Mauss, le linguiste Meillet, l'esthéticien Lalo, deviennent présidents de la Société de Psychologie, la marque de l'influence des réseaux d'amitié et d'intérêt de Meyerson, réseaux auxquels il fera très largement appel pour l'organisation et le contenu du Congrès International de Psychologie, qui se tiendra à Paris en 1937, dont il sera co-organisateur et co-rédacteur avec Piéron.

2.2. Des pans entiers de la psychologie de l'Entre-deux-guerres n'apparaissent pratiquement pas dans le Journal, comme la psychologie du comportement, par exemple: celle-ci semble "réservée" à l'Année Psychologique de Piéron, au laboratoire duquel, rappelons-le, Meyerson occupe la fonction de sous-directeur. Il ne semble

pourtant pas que l'on puisse accuser Meyerson de parti pris, sinon, peut être de publier davantage de travaux de psychologues européens (hollandais, allemands, danois, russes..) que de psychologues américains. On peut noter par exemple que c'est lui qui correspond avec Pavlov et l'invite à la Société de Psychologie et qui demande des articles importants sur la réflexologie à l'Ecole Roumaine de psychologie.

L'examen de l'énorme correspondance (plus de 3000 lettres) reçue par Meyerson montre bien que loin d'avoir été contestée, sa direction a été fortement appréciée, et qu'elle l'a conduit à développer des contacts scientifiques avec la quasi totalité des "grands" des sciences psychologiques, humaines et sociales de l'époque.

On peut donc probablement avancer que si le journal porte très largement l'empreinte des orientations de Meyerson, c'est parce que celles-ci traduisaient largement la dynamique de la **psychologie française** d'alors, accordant une attention privilégiée aux processus supérieurs et, donc, se trouvant en relation nécessaire, quelquefois conflictuelle, aux disciplines qui s'autonomisaient alors magistralement du côté du "social": la linguistique, l'esthétique, l'épistémologie, l'ethnologie, la nouvelle histoire, et qui tentaient de prendre des objets, sinon des pans entiers à la psychologie, qui avait pendant un temps été hégémonique.

Meyerson allait tirer les leçons de cette situation: les fonctions mentales de l'homme et ses oeuvres relèvent les unes et les autres de processus d'ordre psychologique même si ceux-ci sont tributaires de données historiques et sociologiques. Pour Meyerson, l'autonomie et la spécificité de la psychologie ne peuvent être mises en question, ni se trouver menacées par l'essor des autres sciences humaines. Bien au contraire, il faut, pratiquer avec elles des échanges, fondateurs de nouveaux domaines de la psychologie. Ce que d'aucuns n'auraient pas manqué de lui reprocher, en "excluant" de leur psychologie celle qu'il préconisait, du fait même de ses relations avec les disciplines sociales, historiques et culturelles.

Il faudrait analyser plus précisément les limitations d'ordre interne ou externe au développement de cette orientation en psychologie, pour elle-même, et pour la psychologie en général.

NOTES

- ¹ Je remercie tout particulièrement Annick Ohayon et François Sellier pour leur contribution à la réalisation de ce travail (Equipe d'histoire de la psychologie en France - UFR de Psychologie, Université de Paris 8 - 2 rue de la Liberté F93526 SAINT DENIS CEDEX).
- ² Le Journal se définit plus spécifiquement comme une revue latine: son comité de direction sera composé pendant longtemps de psychologues et d'aliénistes non seulement français, mais aussi belges, brésiliens, espagnols, italiens, roumains, suisses et grecs. Son ambition est ainsi de disposer d'une assise sociale aussi large que celle qui assure alors la prospérité des revues de langue allemande ou de langue anglaise.
- ³ Les analyses bibliographiques de *L'Année psychologique* aussi, seront pour une part assurés par Meyerson.

A culture theory approach to the historiography of psychology

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INTRODUCTION: THE HISTORIOGRAPHIC PROBLEM OF MENTAL MEASUREMENT

In an earlier publication (Voestermans, 1991a) I argued that psychological practice is the dimension of psychology that has had a strong influence on the modernization of practices in the field of mental health, education, family life, and so on. Yet, to give a historical account of this practical side of psychology is quite a hassle, since we all know that as a practical science psychology is quite vulnerable to ideological influences. Although ideology belongs to history, it is quite difficult to see how that is the case in actual psychological practice. Much of psychological practice has become ingrained to such an extent that it is difficult to see it as something ideological. In this paper I try to circumvent the problems one encounters in writing the history of psychological practice from a theory of ideology approach by taking recourse to the culture theory approach inspired by Fleck's model of scientific development (Fleck, 1935/1979) and by recent developments in culture theory (Voestermans, 1991b). What I subsequently want to argue is that the subsumption of ideology under 'culture' has important and useful historiographic implications. Particularly with regard to the debate about internalism versus externalism in the historiography of science.

In order to show the shortcomings of the received view of ideology and to make the alternative approach more concrete, I will address myself to a field of controversial historical interest, the field of Differential Psychology. This is an area of practice in which the biological 'discourse' has been conspicuous. The central issue that was argued for in biological terms was the inheritability of psychological abilities and traits and the way their conspicuous presence among the lower strata. The historical record shows that the hereditary nature of various traits was accepted rather uncritically by psychologists. This poses some serious historiographic problems. In hindsight one can quite convincingly argue that the practical field of the psychology of individual differences became an area in which fundamental "mismeasure of man" occurred and in which the eugenics movement found its origin.

The part biology has played in this somewhat unsavory history of psychology and to what effect is still not very well understood (Young, 1985). The view is commonly adopted that eugenics, but also the mismeasure are **ideological lapses**. Up till the present day, even the well-known IQ-debate is carried on along these lines. Historians tend to reconstruct the past of this debate along the same line. The main question is whether one can give a valid historical account of, for example, the eugenics movement and the "mismeasure of man" by just arguing for a rather unwholesome blending of the scientific procedures with the rather unfortunate ideological leaning of the practitioners. It will be shown that it is historiographically misleading to adopt the view that those dark pages are just a 'derailment' or a product of ideology. The history of psychological practice in the field of mental measurement and of the biological discourse which supported the hereditarian nature of certain traits is much more complicated. Unraveling the strands of thoughts which led to the massive support for the practice of testing and attitude measurement requires a cultural analysis of this practice's impact on the modern view of man as a configuration of traits and abilities. It will be shown that it is the way the science of psychology itself has been conceived and the concomitant psychological practice it ensued rather than ideological forces that determined the course of this dark part of psychology's history.

HISTORIOGRAPHIC OPTIONS

Let me first present to you the usual historiographic options in writing those so called 'dark pages'. In a historical account of the eugenics movement, particularly of the mainline variety¹ and the mental testing machinery, the following approaches have been tried. I will not go into detail, because I presume the approaches mentioned are reasonably well-known.

1. One approach is to review all those psychologists who voiced their prejudices in scientific disguise; something the Germans call **Gesinnungsnüffelei**, that is scrutinizing the researcher's mentality. A good example of this approach is Pastore (1949), who was, it should be respectfully acknowledged, one of the first to really ask questions regarding politics pertaining to a behavioral science which no one at that time thought of as being to a large extent political. Parts of Chase's work (Chase, 1977) are a good example as well. In those works well-known psychologists are screened for their ideological stance. Something like a progressive-conservative dimension with an emphasis on fostering social change vs. maintenance of the **status quo** is employed.

2. Another approach in writing the history of the eugenics-psychology relationship and its concomitant deployment of mental measurement, is to look for the "racist connection" in psychology in order to reveal the 'conspiracy' of the ultraright-wing. For example Billig (1981)² has done just that in a shocking but very informative book.

Janssen reconstructed the racist connection in psychology along Billig's line and included other branches of science such as ethology and social science in general. He came up with some astonishing facts regarding three notorious extreme right-wing

journals: *Mankind Quarterly*, *Neue Anthropologie*, and *Nouvelle École*, which draw from a long racist tradition in the Western World. He discovered that several prominent psychologists have been members (or still are) of the Honorary Advisory Board of one or more of these journals. The pages of these journals were filled by well-known fascists, neofascists, ultra-right wingers and racists, who did not take the trouble to hide their admiration for such writers as Hans Günther or Gobineau (Janssen, 1982).

What he concluded from an extensive review of the writings of psychologists who sympathised with eugenics, racism, ultra-right wing ideas, etc.; and of those who definitely did not, was that there are two separate worlds to which fact-finding and proof with scientific means are equally strongly adhered to. Therefore, one party cannot debunk the other with scientific arguments, because two vastly differing and opposing ideologies with regard to how the world should be designed are at stake, and not just methodologies. That such differences exist is not clear from the quarrels among scientists who more or less adhere to the common decency of the intellectual community. They become visible only when one gets hold of a group, like Billig did, which uses the same correlational techniques, respects the canons of science with comparable credibility and rigor, but which departs from presuppositions which are not simply prejudices but **deliberate choices** which are radically at variance with our ingrained decency. One example suffices. Is there any one among us who disputes the equal worth of every member of our society? Yet, one scientist claimed in an article in *Mankind Quarterly* of June 1979 - I quote: "The possibility has to be considered that mankind should not be encouraged to remain a single biological species". That is to say, if unequal worth does not exist, it should be invented. The author was no one less than the great trait-psychologist Raymond B. Cattell.

Janssen's conclusion is that the futile battle of methods and procedures needs to be replaced by a battle about basic options, values, or ideologies, if one prefers that notion, and a thorough historical analysis of their vicissitudes.

3. The third approach is a straightforward chronological account of important historical events and developments in eugenics and mental measurement by describing the activities, writings and thoughts of the main protagonists in genetics and related fields, using the available archives of libraries and key-institutions. The work of Daniel Kevles (1985) is a good example. His book is a detailed historical account of the biometric approach, the invention and development of statistics, the development and reception of behavioral genetics in psychology, etc. No doubt, works like that of Kevles needs to be done, but one remains rather ignorant with respect to what exactly was at stake.

The approaches mentioned have some serious and quite fundamental shortcomings. These I would like to spell out for you, using a rather famous exponent of the critique-of-ideology approach, the same approach that Pastore, Billig, Chase and Janssen used. It is Stephen Jay Gould's book about *The Mismeasure of Man* (Gould, 1984). It is not my intention to downgrade the work done by this or all the other authors. I think it is worthwhile and very insightful. The point is that by focussing on

ideology, important dynamics of science in the history of psychology will escape the attention of historians. Let me first try to point out the historiographic shortcomings of these attempts to show the ideological underpinnings of psychology.

THE HISTORIOGRAPHIC SHORTCOMINGS OF THE CRITIQUE-OF-IDEOLOGY APPROACH

There is a focal argument in Gould's book which can be summarized in one sentence: major behavioral traits are "not in our genes". He subsequently argues that we know by now what was wrong with the early conception of the hereditary nature of behavior. It was the massive neglect of the environmental origin of behavioral patterns which are displayed in the traits. Psychologists didn't see or did not **want** to see the effects of the environment. The question can now be raised: What caused this incapacity or unwillingness? Gould suggests that it was the firm belief in science as an objective enterprise, a belief fully ingrained in 19th century thinking. What Gould rejects is the so called decontextualized view of science.

I think he is right, but this is only half the truth; he does not go far enough. For what is not stated by Gould, but is quite self-evident, is that this firm belief in science expressed itself in the strong conviction that only those things which could be stated mechanistically and materialistically, that is to say in the dominant positivistic way, were regarded as amenable to objective research. Please note, that I am not saying: "Which could be studied with the methods and procedures of natural science", because that is certainly not the same. In other words, there was a hidden assumption that the social events that are comprised in, for example, the notion of 'nurture' or 'environment' did *not* belong to the category of things amenable to objective research. This exclusion explains the neglect of environment. The same can be stated in still another way: What did not belong to the things amenable to scientific research at that time was culture. **It was the force of culture operative in the shaping of behavior which escaped attention.**

Although it was quite reasonable, even according to the standards of 19th century science, to expect a science of **behavior** to address itself to cultural determinants as much as to biological causes, it seemed that there was a definite unwillingness to do so, notwithstanding many pleas to at least try it. The pleas can easily be documented; Boas' work of the late 1800's (Boas, 1966) is a case in point, but also Wundt's *Völkerpsychologie* (1900-1920), of course, and even still earlier, the work of Herbart (1816). In other words we are talking about a discussion that existed at the same time that the "mismeasure" occurred, although in another field, in anthropology.

I think that this neglect was in part the result of a rather dubious bond between biology and psychology. In one way or another there existed among psychologists a strong preference for biological mechanisms. Of the biological mechanisms sought after, the hereditary ones were the most favored ones, certainly in the early days. Along with that psychologists were reluctant or unable to conceptualize non-biological determinants of behavior with a rigor equal to the one customary in such a natural science as biology. This poses the interesting historiographic question of the reasons

behind such reluctance. This question is particularly interesting, because this reluctance has influenced historiographic practice itself.

PSYCHOLOGY'S RELUCTANCE TO TURN TO CULTURE AND VICE VERSA

Skepticism regarding psychology abounds in culture science up till the present day. On the rebound, psychology wasn't very interested in culture either. I will try to unravel the forces at work in this mutual skepticism.

Let me first document the reluctance in culture science to turn to psychology by quoting the leading culture theorist, who also had quite some influence on historiographic endeavors in social science, Marvin Harris. The target publication is Harris' book *The Rise of Anthropological Theory* (Harris, 1969). Harris becomes quite nasty about psychology. According to him:

"the search for elementary mental 'structure' is nothing but a return to the practice of explaining sociocultural phenomena by means of conveniently posited instincts" (Harris, 1969, p. 429).

He therefore abhors psychological reductionism, like so many in the field of anthropology. Psychic universals never explain cultural differences running from the different exogamic phenomena to the diversity in the way people in the past and in the present share or refuse to share wealth. Furthermore, in Harris' view all this mental stuff has one bad thing in common: it has no measurable energy cost. Cultural materialists prefer to explain cultural diversity in terms of ecological pressures, cost/benefit calculations, labour investments and pay-offs etc (Harris, 1977; 1979). Psychology is a nuisance.

Yet I think that the cultural materialists' program is running into serious trouble. The more Western culture and its predominantly instrumental rationality spread itself out over the entire world so that economy, political organization and juridical structures became quite the same the world over, the more deep-seated behavioral patterns became apparent which in fact function as a source of counterpressure against Westernization. That makes the question about the nature of culture so pertinent. In most answers to questions about the nature of culture 19th century forces were and still are at work. In the 19th century much of the discussion about culture revolved around notions like nature and nurture without much avail. One was preoccupied with the biology-history antinomy. At the core of this antinomy lay the question which science was best equipped to deal with the patterns of human behavior. What are the dynamics behind such clash of disciplinary competences which obfuscated the task of dealing substantially with the task to come to grips with people's behavioral patterns? I think it is largely a moral issue and not an issue of how to arrive at a better understanding of what determined people's behavioral patterns. Science was used to settle an issue that was only indirectly connected with the task of coming to grips with

the culture-behavior nexus. The issue was that during the 18th and 19th century attempts have been made to deploy science as an antidote to religiously inspired superstition. Positivistic options thus prevailed. Others wanted to use science as an asset to installing at a personal level modernized options on basic values that were previously rooted in religious ideals. In this latter case a secularization of ethics was strived for, either along a more or less sociological path or along a psychological one, which caused also this clash of competences. In retrospect, much of social science, be it psychology or sociology, was preoccupied **not so much with explaining social phenomena and behavioral controls**, but with **maneuvering social science into political debates** (not at all without practical consequences of course) about the construction of the modern state and about the preconditions for the creation of an individual which would subscribe to the basic tenets of the dominant classes.

I am not sure that this is ideology. At least I doubt whether a historical reconstruction of what happened in social science can be carried on along the line of a critique of ideology approach. And it is because of this skepticism that I search for an alternative historiographic option. The notion of ideology has too many doctrinal overtones. Of course there existed doctrines in defence of the preoccupation mentioned above, but the way one wrote about them was not ideologically organized, unless one stretches the meaning of the term so that it encompasses all human thinking, feeling and action. I therefore turn to culture theory. Because it is culture that is at work here. Culture in the sense of deep-seated behavioral patterns to which even science was intimately connected. But that brings us back to an interesting phenomenon, namely the fact that what actually did organize people's sentiments about morals, social values, individualism etc. and stimulated the use of science in defence of positivism, wasn't scientifically analyzed at all. That is a very important historical fact, which should become an important point of departure in the writing of the history of the psychological and bordering social sciences. But let me first explain what aspect of culture escaped from scientific scrutiny.

THE CULTURE THEORY APPROACH

Culture is not a reservoir of symbols to be tapped by those who have mastered access through specialized training. Culture is a real constitutive force, which shapes behavior in a way that needs explanation and understanding. It is as real as biological forces at work in the shaping of behavior. It deserves a scientific scrutiny on a par with that devoted to, for example, DNA codes. What in the social epistemology programme is called 'the social' entirely depends on the workings of culture. It is not sufficient to view culture as merely a localizing context open only to description.

Whereas earlier definitions of culture referred almost exclusively to the degree of civilization, to civilizing forces in a given nation, or in the case of tribal or otherwise small communities to the people's 'way of life', in modern theory from the 1970s onward the signifying practice and the symbolic and expressive dimension of social

life lie at the center (Shweder & LeVine, 1984; Wuthnow, *et al.*, 1984; Stigler, Shweder & Herdt, 1990, Bruner, 1990). What the earlier and the more recent definitions have in common is that they refer to a somehow **propositionally** and **argumentatively** organized system of ideas. Language was thought to be the main road to cognition of whatever cultural variety. Culture thus understood becomes something in people's heads or something to be tapped from 'discourses'. Defined this way culture -we leave aside the colloquial use of the term in order to designate a large human group with distinctive features- turns out to be a subset of ideology. Yet I would like to put it the other way around. Ideology is a cultural phenomenon and as such it is a **subset** of culture. A subset we need to understand in terms of a designated set of culture features, from which particular discourses and discursive practices of distinct subgroups within society can be derived.

The conception of culture that enables us to do so, establishes its point of departure in the fact that people participate in something that objectively regulates conduct without this conduct being itself the product of **following** rules or regulations (Bourdieu, 1990). Starting off from this assumption culture is a source of **patterned** actions, which are highly individualized and experienced as personal, but which are not generated by opinion-like beliefs. They are generated by corporeal and affective structures. Their existence should be explained in terms of the **cultural framing** or **cultural formation** of these structures. Culture should therefore not be equated with rules, norms, values or beliefs. It manifests itself from the close interplay of things done and used, stories told, metaphors lived by and emotions and feelings ritualized. Understanding the processes involved is a matter of searching for surprising, manifest phenomena with a hidden, underlying structure. Theorizing aims, just as in physics, at making the invisible, latent structure behind the manifest phenomena visible by using our imagination and a variety of research instruments. Culture theory borrows from the fact that things are done, not because they are true in a logico-mathematical or empirical sense of the word, but because they are designed or ritualized that way in order to create a world which is real and makes sense. Culture, so understood, encompasses everything included in the concept of ideology. Yet an important reconceptualization is made by referring to culture rather than ideology: The focus is as at least as much on body and affection as on opinions and beliefs. The latter should not be excluded, of course. They need, however, to be reallocated to the close interplay of body, affection and culture (Bourdieu, 1990; Welten, 1991; Voestermans, 1991b).

The following arguments for subsuming ideology under the concept of culture can be listed. Firstly, using culture instead of ideology ties the study of the nature of experience with its dual quality of realism and naïveté to the study of culture in the sense of a **behavior regulating system**. At first sight this may appear to be a step backwards, because the study of culture has been suffering from sterile discussions about definitions and research procedures. Yet, this field also has a long standing tradition of describing personal experience. Much anthropological work is paradigmatic of the difficulties which are involved in making sense of indigenous practices and beliefs. It has deepened the understanding of our own social world. Much recent work in psychological anthropology has revealed the Achilles' heel of much psychological

research: that the form of mental processes may be affected by culture (Stigler, *et al.*, 1990).

A second advantage is that it side-steps the Western preoccupation with doctrinal affairs which mark the discussion about ideology. Because ideology is usually viewed as a belief system, ideological conflict is chiefly considered to be a clash of beliefs. Yet, beliefs or attitudes are only partly responsible for behavioral monitoring (Tomkins, 1981). Cultures differ widely with respect to the degree beliefs regulate behavior and even within a given culture not every behavioral domain is equally and ubiquitously organized by beliefs. Modern culture theory calls attention to other behavior-regulating mechanisms than those generally included in the notion of ideology with its doctrinal overtones. Ideology thus retains its proper niche. It continues to be a **philosophical** concept. As such it is part and parcel of epistemology, notwithstanding the sloppy use of the term ideology in all sorts of contexts.

The third advantage of the concept of culture is that it directs attention to what Shweder (1984, 30 ff.) has called the **non-rational** control of behavior. This kind of control is intimately related to the other regulatory mechanisms mentioned in the second point above.

Having subsumed ideology under culture, a new historiographic model will emerge. A model in which the critique-of-ideology approach to history writing can be integrated.

PSYCHOLOGY AND CULTURE REVISITED: A NEW HISTORIOGRAPHIC TASK

Let us first consider what the shift in emphasis away from ideology to culture theory implies for the science of psychology as a constitutive force in the construction of the individual **and in that context for the writing of the history of psychological practice**. The obvious gain is a much clearer **psychological** interrogation of the material changes in a given society. Focusing on body and affection, for example, may unveil the mechanisms that are involved in the framing or formation of emotion and feeling as very influential behavior-organizing devices. Such an approach may result in a well-balanced historical analysis of emotional demands that are connected with changing circumstances, be they of economic, social or political origin. Emotions and feelings are analogous communications. these communications posit something which at best can be described as iconic or procedural relationships. the richness of communicative import cannot be exhausted in verbal statements of the actors involved. Since emotions and feelings do involve organized experience, linguistic expression is limited. This is why people often refuse to say that they are angry, or put in words how they feel or think about somebody. They prefer to act in certain ways, or to express themselves ambiguously. They do, of course, use words, but the intention is to convey a 'picture' of the experience as completely as possible or to account for the way they arrived at a certain expression. Emotions, then, are expressive of a very

informative relationship of individuals to society; conversely, the relationship takes shape from the various kinds of articulation to which emotions are subjected.

The experience of bodily change is a major source of articulative pressures. Through the involvement of the lived body, emotions and feelings inform us immediately about what is going on. Words like 'love', 'attraction' or 'aggression' are merely verbally coded distinctions of what essentially are patterns in the experience of interchange. On these coded forms culturally prescribed vocabulary indeed has strong bearing, but despite an (often strong) touch of language and speech, emotions and feelings are realized foremost with or through the body, depending on the experience of being gripped or seized (passive undergoing), or of being placed in the position of enactment (actively expressing or doing something 'with feeling'). Emotions and feelings are as much enacted as they are inflicted upon us.

Enactment implies sometimes that some distance is realized with respect to what one is undergoing. But then we are talking about a skill. The distance is marked by a skilled 'disembodiment of feelings' manifesting itself as 'regulation' or 'control'. Control, therefore, should not be defined in terms of the shaping of an amorphous substance of some sort (arousal, for example). Emotions do not function as an amorphous substratum to cognition. It appears no more appropriate to speak of a cognitive label for emotion than it is to speak of a cognitive identifier of whatever other ideational event (such as memory, intuition, belief, etc.). What is remembered, intuited or believed is not the cognitive identification of a vague substratum of mental going-ons. Emotions and feelings are ideational at the outset, as much as thoughts or beliefs are.

Let me give some examples of the kind of the historical questions which can now be dealt with more adequately. It is well known that the move to cities caused the disruption of ritualized control of emotion and feeling that were common in rural communities. Such a change had far-reaching consequences for pair-forming practices, mate selection, worldly and churchly control of marriage, child rearing, paternal authority and so on. These processes are central not so much to the construction of individuality but of masculinity and femininity. Concentration on merely the **ideological** aspects of dimorphism often results in a sterile discussion of philosophical underpinnings of gender, whereas gender is primarily a culturally-framed **experience**. This takes us back to the fundamental issue in culture theory: The non-rational order of things. This is a much more general problem than the issue of order based on faulty beliefs which the theory of ideology usually emphasizes. Of course, psychology has had, and still has, enormous influence on the creation of that order. Yet, this influence should be historically compared to the influential mechanisms of control and regulation that existed before and which were replaced by **psychological means of control**.

In order to proceed this way the entire historiographic focus has to change. Psychology's contributions should be placed in the context of the **modernization of life**. Along such lines of historical investigation we move away from the epistemological or philosophical debates that are so rampant in the received historiographic procedures,

right into an area of investigation that is not muddled by the paralyzing perplexities regarding the nature of knowledge. There are, I think, more urgent things that need to be said about the impact of psychology.

From a cultural perspective both the pre-existing society and the individual are characterized by **specific** demands at certain points in time which entail a particular way of doing things. These demands are obviously voiced in texts and speech (discourses), which suggests that what is thus written and voiced has behavior-organizing impact. Yet, text and speech do not organize experience for everyone in every domain and with the same intensity. We know that for certain from comparisons with other human groups (societies) in which there is not the kind of reflexive attitude to life human beings in modern societies tend to indulge in. So not everyone everywhere has attitudes or opinions or other propositionally and argumentatively organized social representations that account for the influence of 'the social'. Furthermore, even in our own society the discourses generally belong to too small a group of spokespersons to warrant a claim of being a universal constitutive force. Reasoning along these lines we can no longer accept a writing of history in which only ideological thinking is analysed.

What **does** organize experience and how exactly within a given institution is again an issue that culture theory can deal with. The results of this culture theory exercise need to be integrated in our historiographic practice. We are all willing to accept a culture theory perspective, as soon as it is a tribal community that is under investigation, be it historical or whatever. In that case we accept that recourse is taken to rituals, or other devices that organize experience in 'non-reflexive' ways. Since these organizing devices are of **cultural origin**, it is of course possible to investigate their underpinnings by using the culture theory framework. Such an approach does not have very much in common with the philosophical arguments of ideology theory. What is true for tribal communities, also holds for our own society. Many of its participants structure their behavior in accordance with what needs to be done and with what they always have seen people doing. Some sort of automatic patterning takes place in which the body as an expressive unit plays an important role. Culture resides not entirely in the head; it is also something that engraves itself into the body (Welten, 1988, 1989, 1991; Voestermans, 1989, 1991). The body thus functions as the second instance, apart from the institutions, in which the social is 'objectified'. The social arrangements or institutions appear to us as 'social facts'. They may take the shape of a hospital, a school, an insurance company, or a supermarket. It is quite obvious that the behavior is 'scripted' by the qualities these institutions have, including a 'grammar' of the roles of its participants. But this scripting depends as much on the way the body and the automatic, habitual patterning of behavior that goes with it, are fitted into these institutional provisions as on the institutions themselves.

Against the background of the foregoing points, much of the insight psychology has provided during its more than a century old practice is culture-specific, in the sense that it relates to the way modern individuals have **embodied** the psychological means

of behavioral control and regulation. Having certain emotions and feelings to be used in pair-forming practices, and having learned to comply with the demand for certain skills and abilities that fit into the modern institutional order, both exemplify the impact of what can be called a 'psychologization' of the person. This psychologization is not just a matter of mental structuring as ideology theory wants to have it; it is a bodily affair as well. If this latter point is taken into account, then, psychology's means of control are not illusory, just as the behavioral controls in tribal communities are not illusory, even though they often seem so senseless from our perspective. It is this quality of psychological practice - the portion that can be compared with ritual - that is historically (and for that matter, historiographically) important. It is noteworthy that Foucault (Foucault, 1977) makes an important distinction between epistemic truth and 'truth' brought about by disciplinary, normalizing, ritualized, and procedurized control. Much of psychological practice belongs to the latter.

Psychological practices (psycho-therapy, mental testing, psycho-diagnostics etc.) share the culture-specific quality of psychology as a science in the sense outlined above. It thus shares psychology's regulative impact, which we have tried to unravel, not by following the theory of ideology, but by using culture theory. This approach turns practice into something **semi- or proto- scientific**. Having reached this point history writing becomes part of cultural analysis along the lines of culture theory. Remarkably enough, by restating psychology's historiographic task in this manner I return to the program of Ludwik Fleck (1935) in which a thorough **cultural analysis** of thought collectives was advocated. The view of science emerging from this view is science as a series of proto-ideas, on which cultural frames have a strong bearing. The history of science thus becomes not so much an account of progressive search for rational explanation, but an account of the purifying, so to speak, of proto-ideas. Such purification is at once an analysis of the cultural forces which are as much at work in science as they are in the rest of man's endeavors. Thomas Kunh's influential restatement of Fleck's program suffers from the same neglect of culture as in the received critique of ideology approach to history writing.

AN EXAMPLE OF PSYCHOLOGICAL PRACTICE HISTORICALLY RESEARCHED WITHIN THE FRAMEWORK OF CULTURE THEORY

For a final, albeit sketchy discussion of the historiography of psychological practice within the framework of a culture theory I have selected the construction of the mental testing apparatus to exemplify the culture theory approach. In the late 1800s and early 1900s, and the subsequent debate about the heredity of mental traits, distinctive conceptions of hereditary mechanisms were used. The scientific articulation of these various conceptions took place in evolutionary theory, cell biology, botany and other branches of the life sciences. There are reasons to believe that psychology's choice of, and prolonged adherence to, the pre-Mendelian **blending** conception of hereditary mechanisms, despite counter-evidence from genetic research in which the final particulate conception of heredity was adopted, were inspired by preconceived

ideas about the lower strata and their abilities to participate in the creation of the modern state. These preconceived ideas were culture-bound and, as such, constructed on the basis of predominantly affective and negatively charged experiences of inter-class exchange in which the higher strata's fear of the masses was obviously dominant. The black pages in the history of psychology about the "mismeasure of man" (Gould, 1984) and eugenics (Kevles, 1985) center around the issue of quasi-scientific arguments in favor of isolation of the higher classes and protecting them from the bad influences of the 'lower and middling classes'. It is still puzzling which sentiments were, and still are, involved in maintaining distinctions of class now that the economic necessities, which for such a long time were held responsible, are no longer present. Even Marxist reform had to organize its revolutionary practice within the confines of at least an upwardly mobile lower class, that is to say, a stratum showing some affinity with the bourgeois lifestyle. This again is a cultural issue.

Psychological practice in mental testing and attitude measurement has created a reflective mode of behaving that has hardly been disseminated into the lower strata. Members of these strata are not controlled by opinions that are explicit enough to be measured, whereas individuals from other strata apparently do have these types of opinions. Psychology's charting of the lower strata's traits, viewpoints and behavior was an asset in making certain segments of this stratum upwardly mobile. Although procedures of this type affected a large portion of the working class population, a hard core still seems to be somehow immune to the 'psychologizing' which transformed other segments so successfully. The history of psychological diagnostics badly needs to be scrutinized for such differential effects.

Viewed in this way, mental testing and attitude measurement, or assessing aptitudes can again hardly be called 'ideological'. It can be called poor science or proto-science, since the measurement of traits or skills generally lacks good theory from which measurement can be derived. This type of psychological practice nevertheless nestled immovably within the cultural changes that shaped the status and position of members of the various classes and even the classes themselves. It helped to create, in conjunction with economic and sociological forces, the present-day pluriform society with its highly differentially functioning members. Its contribution to the articulation of personality traits has no epistemic or rational scientific foundation. This can easily be demonstrated, for example, by the continuous and rather stagnant search for a scientific theory of intelligence. It is well-known that the way intelligence is measured in present-day psychological practice is a pragmatic affair. Historically it is an entirely culture-specific enterprise, even though it is one of psychology's most cherished practices.

In sum: It appears that psychological practice is losing its status as a **scientific** enterprise; a status it lacked at the outset. But to label all psychological practice that is not, or only partly based on science, 'ideology', is not of much help. It is better to show through a historical and cultural analysis of psychological practice in what way this practice, by matching itself with the demands of a rapidly changing and modernizing society, contributed to the modernization of the regulation and control of individual conduct.

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NOTES

- ¹ distinction in mainline and reform eugenics is introduced by Kevles, 1985. The mainline variety dates back to the late eighteen- and early nineteen-hundreds.
- ² Billig published his book on racism and psychology in France, although an earlier version was published in 1979 under the title: *Psychology, racism and fascism* (Birmingham: A Searchlight Booklet)

Historiography of Eighteenth-century psychology and the "Encyclopédie"

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In this paper, I would like to review some aspects of the historiography of XVIIIth century psychology, and suggest that examining psychology in the *Encyclopédie*, apart from having intrinsic interest, might bring into focus certain problems of definition and delimitation characteristic of the history of the human sciences. I use "historiography" in the two usual senses of the word, "body of historical writing," and "the writing of history."

It is widely accepted that the history of psychology since the XVIIIth century is to a large extent the history of how what Hume called "science of man" replaced metaphysical and theological doctrines of the soul, knowledge, and behavior. Histories of ideas, histories of philosophy, and histories of psychology tend to construe XVIIIth-century psychology as a direct descendant of Locke, identical to the Enlightenment luminaries of empiricism and sensationalism. It is entirely in that perspective that Locke is said to have "aroused a lively interest in that very important and highly fascinating diversion -psychology" (Hazard, 1953, 251), or that the XVIIIth century is described as "the century of psychology" (Gilson & Langan, 1964, 225), and said to have turned psychology into "the strategic science" among the sciences of man (Gay, 1969, 167). It is on the same basis that a prominent modern thinker observed that, in the XVIIIth century, philosophy was "to be converted into a natural science," indeed into "a kind of scientific psychology" (Berlin, 1984, 19). There is of course a body of monographic historical writing that deals with other authors and other problems. But the general view is as I just described it.

This view is not wrong; and if we think about the contributions of Ernst Cassirer, Paul Hazard, or Isaiah Berlin, we must recognize that it can be extremely enlightening. But, as I'm not the first to point out, it is problematic.

On the one hand, none of the authors most often described as psychologists even used the word "psychology." The philosophes saw Locke as having contributed to the renewal of logic and metaphysics, not psychology or epistemology. At least one of them went as far as rejecting "psychology" explicitly: Condillac, who has been characterized as "the leading psychologist of the eighteenth century" (Gay 1973, 521), commented that he would call "psychology" the science dealing with the origins and

generation of ideas, if only he knew some good book with that title (s.d., 570). On the other hand, authors who did use the word, and in prominent places of their writings, such as Charles Bonnet and Christian Wolff, are often not even mentioned. Yet, when a word is absent, we should wonder whether the concept is present; and when the word is present, we should try to ascertain its meaning. And whereas this task is not essential to the intellectual projects of the historians I mentioned, it seems that it would be for anybody seeking to conceptualise the history of psychology.

In the XVIIIth century, "psychology" could be a branch of metaphysics, as in Christian Wolff's *Psychologia empirica* (1732) and *Psychologia rationalis* (1734). Wolff did not significantly depart from earlier uses of the term: by the midXVIIth century, *psychologia* had been adopted as the equivalent of *doctrina de anima* (Micraelius, 1662, col. 1165). But its extension was not yet established.

Together with theology, angelography, and demonology, "psychology" was a part of pneumatology, or the science of spiritual substances. In this usage, which is still found in the second half of the XVIIIth century, "psychology" tended to mean the study of the soul separate from the body. As early as the mid-XVIIth century, however, one of the bestknown encyclopaedias of the time used "psychology" to designate not only the study of the separate soul, but also that of the soul insofar as it was joined with the body (Alsted, 1649). The term kept oscillating between natural philosophy and a purely spiritual Pneumatology throughout the XVIIIth century; and it was only gradually that Locke and his descendants came to be identified as "psychologists."

The minimum that such semantic oscillation implies is that XVIIIth-century psychology is not easy to define. The difficulty concerns the extension of the word, that is, the total number of objects to which the term applies. If it concerns extension, then it must also concern intension, that is, the total number of characteristics which something must possess so that a particular term can be applied to it. Intension - what things the idea of XVIIIth-century psychology should include - has been dealt with by several authors.

Georges Gusdorf's monumental (and little known) history of the human sciences contains some enlightening passages on the relations between the history of the word "psychology" and its institutional and semantic correlates (esp. 1969, II, ch. 3; 1973, Pt. I). Sergio Moravia pointed to the many "theoretical *détours* and ambiguities" (1980, 247) of the process whereby the human sciences evolved during the Enlightenment. George S. Rousseau has suggested the impossibility of charting "three distinct areas: psychiatry, psychology, [and] philosophy" (1980, 144). His choice to call "psychology" "the matrix or nexus of ideas and human practices we today would call psychology and psychiatry" (ib., 145) is problematic. But at least it highlights the historiographical issues. More recently, Christopher Fox (1987) made the same point, by emphasizing the range of contexts in which psychological matter appears, and the meanings and fluctuations of the original vocabulary. All these authors are exploring

the possible unity of an extremely polymorphous historical object that, as Georges Canguilhem (1958) observed a long time ago, developed as a natural science, as a science of subjectivity, and as a science of behavior.

The *Encyclopédie* illustrates in an exemplary fashion these problems of delimitation and definition.

In the anonymous article on "psychology," the *Encyclopédie* of Diderot and D'Alembert includes a short and straightforward summary of Wolff's doctrines (handout I). Divided into rational and empirical, psychology is defined as the branch of philosophy that "treats of the human soul, defines its essence, and accounts for its operations." In addition, it is said to furnish principles to natural law, natural theology, ethics, and logic all disciplines founded on the properties and functioning of the soul. By the 1750s, Wolff was well known in France. He exerted a considerable influence on the *Encyclopédie* (Thomann, 1968; Carboncini, 1984, 1987), and his empirical psychology had already been the subject of two vulgarizations (Anon., 1745; Formey, 1753).

Yet it is precisely in the field of "empirical psychology" that the *Encyclopédie* is inconsistent. The main tool of Wolff's empirical psychology was what Leibniz called "apperception," a mental act by which perception becomes consciously present to the mind. For Wolff, as Cassirer points out (1962, 121), "a psychology [such as Locke's] which attempts to find the basis of the mind in the impression has simply misunderstood the whole problem." Yet, whenever the *Encyclopédie* deals with the operations and productions of the soul, it is predominantly Lockean, and advocates Condillac's method of "analysis," which consists of determining and following the generation of ideas. These preferences are clearly stated in the *Encyclopédie*'s famous "Preliminary Discourse," where D'Alembert praises Locke for having reduced metaphysics to an "experimental physics of the soul," and where he explains that, in charting knowledge (*connaissances*), we must first of all trace it back to its origins.

I'd like to give you a quick idea of how widely the sensationalist precept was applied in the *Encyclopédie*. The article "Metaphysics" contrasts the newer "experimental metaphysics" with the "despicable science" that speculates abstractly on space, time, matter, and soul. In the article "Logic," good and true logic is said to consist of a recollection of the "necessary sensations." The understanding is identified to the soul itself, "to the extent that it conceives of, or receives ideas" (art. "*Entendement*"). Since ideas are defined as the operations of the understanding, the article refers to "Evidence" and "Sensation," where, it announces, "the origins and progress of our ideas are explained and deduced by a philosophical method." Contrary to faith, evidence is circumscribed to "natural knowledge," and based on the "intimate observation of our own sensations" (art. "*Evidence*"). The article "Idea" formulates the commonplace that the soul can only be known by its operations and effects, and manages to boil down all the questions raised by the concept of "idea" to the question, "Comment à l'occasion d'une impression de l'objet sur un organe, la perception se

forme-t-elle dans l'âme?" The article "Knowledge" explicitly adheres to Locke. And "Education" proposes to apply to teaching both Condillac's method of "analysis," and the principles of association.

None of these articles ever mentions "psychology." Neither does D'Alembert's "Preliminary Discourse," where the "science of the soul" is divided into pneumatology, ethics, and logic. Diderot's "Explanation of the system of human knowledge" divides the science of the soul into science of the rational soul, and science of the sensitive soul. But it does not use "psychology" either. The word is absent even from the encyclopaedic diagram (handout II). Under "science of man," however, we find pneumatology, and the disciplines of logic and ethics, to which "psychology" was supposed to furnish principles.

We see, then, that "psychology," the word and the concept, are not yet entirely acclimatized in the great enterprise of Diderot and D'Alembert. On the one hand, as defined by the *Encyclopédie* itself, "psychology" belonged in a metaphysical "system" not unlike the ones the philosophes wished to rejected. Nevertheless, precisely to the extent that "psychology" belonged in metaphysics, and that metaphysics was being redefined in Lockean terms, the way was open for the identification of "psychology" to the science of man as a new foundation of knowledge. On the other hand, the philosophes's intellectual heroes did not use the term. So there was no reason to make "psychology" appear, not even in the numerous articles where we find psychological subject-matter -that is, matter to which the principles of the "science of man," the "experimental metaphysics" and the method of "analysis" seemed applicable.

The *Encyclopédie*'s Swiss edition (which I still have to examine in detail) goes in the same direction, in a manner consistent with its unpolemical character. The article on "psychology" is much longer than that of the original edition (Mingard). Its author first follows tradition by characterizing psychology as a branch of pneumatology. He then argues that it is the basis, principle, and guide of every art and science. And finishes by claiming that the best works on the subject are Wolff's, Bonnet's, and Condillac's. By designating Condillac as a psychologist, the Swiss *Encyclopédie* achieves something the French had started. The word "psychology" no longer designates a purely Wolffian enterprise: it now includes, but is not reduced to, sensationalism.

In sum, the *Encyclopédie*, whether we consider the development from one edition to the other, or focus on its diversity, inconsistencies, and lexical and conceptual complexities, seems to offer a genuine microcosm of the situation of "psychology" in the XVIIIth century.

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Angel Garma, Spanish-Argentinian psychoanalist

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ABSTRACT

The European crisis preceeding World War II, and the War itself, had a great influence on the development of psychoanalytical movement. Analysts trained in European centers emigrated and settled in Latin America. Among them, Angel Garma stands out as mainly responsible for the beginning, institutionalizing and spreading of Latin-American psychoanalytical movement, one of the three main psychoanalytical trends in the world -together with the European and North-American.

Garma succeeded in gathering together a group of Argentinian scientists interested in psychoanalysis, who were later to spread psychoanalytical ideas in this country. In 1942 he founded the pioneering "Asociación Psicoanalítica Argentina", together with Celes Cárcamo, Arnaldo Rascovsky and Enrique Pichon-Riviere. Thanks to this Association, psychoanalysis acquired a new institutional dimension in Latin-America; the A.P.A. became a training center for most analysts who later began psychoanalytical movements in other Latin-American countries.

Garma's most important theoretical contributions are, also, those related to his research on psychosomatics and dreams. Garma's viewpoint is based on Freudian theory, although he introduced several changes in it.

The study of psychosomatic illnesses carried out by Garma paved the way for the development of an Argentinian school, and has favoured a better approach to these problems by the community of psychoanalysts. Its basic features are, on the one hand, a monist conception, integrative of mind and body, of all illnesses, with psychogenic variations as a function of the case; on the other hand, the development of some important psychoanalytical concepts, such as the role of the Oedipus complex and the disorders in genital activity, the hiding and regression to the oral-digestive stage, the important relation between infantile

traumatic situations and those experienced in the adult milieu, and the likely importance of intrauterine life in the psychogenesis of psychosomatic illnesses.

Garma's main thesis on dreams is that, rather than fulfillment of wishes, as Freud characterized them, they should be viewed as masked traumatic situations. In order to explain this thesis and the genesis of oniric hallucinations, Garma worked out a theory of "reality testing" opposed to the Freudian. According to it, hallucinatory processes are based on a what he calls "reality testing error".

LATIN AMERICAN AND ARGENTINIAN PSYCHOANALYSIS

Latin America is nowadays one of the three important psychoanalytic areas. From 78 scientific organizations that gather at The International Psychoanalytical Association, 15 (20%) are from Latin American countries (Aslan, 1984).

The core of Latin American activity may be situated in Argentina. Two pioneer institutions are considered the main centers of psychoanalytical development: the 'Asociacion Psicoanalítica Argentina', and its 'Instituto de Psicoanálisis' of Buenos Aires (Aslan, 1984).

The Argentinian psychoanalytical movement has its origins in the work of a small group of authors from which a Spanish author, Angel Garma, stands out. He 'personifies psychoanalysis in Argentina' (Abadi, 1974). The roots of the history of this psychoanalytic group lie in the work carried out by a group of physicians centered around Arnaldo Rascovsky and Enrique Pichon Riviere. The former was in charge of the 'Servicio de Neurología, Psiquiatría y Endocrinología del Hospital de Niños de Buenos Aires', and the latter was in charge of a consultancy in the 'Hospital de las Mercedes'. Rascovsky and Pichon shared a common interest.

Both developed a sort of wild psychoanalysis out of the norms of the I.P.A., (that was called 'protopsychoanalysis' by Arnaldo Rascovsky) until Angel Garma and Celes Cárcamo arrived in 1938 and 1939 respectively.: 'We could constitute a big and coherent team. What we were doing could be called Protopsychoanalysis, or 'Wild psychoanalysis', until Garma arrived' (Rascovsky, 1974).

GARMA'S BIOGRAPHICAL DATA. HIS ROLE IN ARGENTINIAN PSYCHOANALYSIS

In June 1938, Angel GARMA arrived in Buenos Aires. He was a physician, born in Bilbao (Spain) in 1904, having had Marañón, Ramon y Cajal and Miguel Sacristán as teachers in the University of Madrid. Garma received psychoanalytical training in the Berlin Psychoanalytic Institute, and made his didactic analysis with Theodore Reik, one of Freud's closest disciples.

In 1931 Garma was elected a member of the German Psychoanalytic Association. So, he became the first Spanish psychoanalyst trained in an orthodox way, that worked

in Spain. Later, due to the imminence of the Spanish Civil War, he emigrated to Paris, saying that "as I had no desire to fight against Spanish citizens, I remained in France" (Garma, 1983).

In 1938 Garma arrived in Buenos Aires, and four years later, he founded the 'Asociación Psicoanalítica Argentina', together with Cárcamo, Rascovsky and Pichon-Riviere as didactic analysts. Garma was elected its first president. In the following year (1943), they also founded the *Revista de Psicoanálisis*, the first psychoanalytic journal to be published in Spanish language.

The 'Asociación Psicoanalítica Argentina', was officially acknowledged by the International Psychoanalytic Association in 1949, and it became the first training center for analysts of South America. Its students later began to spread out the psychoanalytical movement all over Latin American countries.

GARMA'S CONTRIBUTIONS

Garma's most important theoretical contributions are those related to his research on psychosomatics and dreams. From the results of a study on Garma's work influence in psychology (Zalbidea, M.A; Cantón, E; Carpintero, H., 1991), his works on psychosomatics and dreams were those most frequently cited by authors of the scientific community.

Psychosomatic studies

From its very beginning, the Argentinian psychoanalytic movement has been mainly concerned with the study and treatment of psychosomatic illnesses. To this main field, Angel Garma has contributed, as a central figure, a large number of works. Both his books *Genesis Psicosomática y Tratamiento de las Ulceras Gastricas y Duodenales* (1954), and *El Dolor de Cabeza*, published in 1958, are among his best known pieces of work. Aizenberg thoroughly states: 'A. Garma and A. Rascovsky were... beyond doubt the founders of Argentinian psychosomatic research' (Aizenberg, 1982).

According to Garma, psychosomatics are based on psychoanalytic grounds. As he pointed out, 'the intermediate link between mind and body was to be found in the psychic unconscious' (Garma 1964). He also describes his general conception with the following words: 'A good psychoanalytical treatment based on the understanding of instincts in a great extent.... leads to a monist conception, i.e. psychoanalytically integrative. During the treatments, when psychoanalysing organic symptoms, psyche and soma are integrated in a whole (Garma, 1964). Such views may be related to some ideas to be found in Melaine Klein's developments of S.Freud (Garma, 1969).

Garma places the beginnings of psychosomatic processes in fetal life. He also acknowledges that different organic illnesses are rooted in various special formations of the Oedipus complex. This brings forth specific sexual behaviors, causing different types of pathological symptoms. In all these cases, somatic disorders would be seen

as expressions of the ego submission to a super-ego that prohibits and represses genital sexual life. Sexual impulses repressed in certain situations are transferred to oral-digestive processes. There the new contents are referred to the idealized mother's breast and its role in the patient's sexual life.

On the other hand, "if organic patients differ in relation to their sexual behaviour, the differences are mainly caused by their submission to their own special super-ego, which is determined by circumstances due both to their heredities and childhood years (Garma, 1964)". Therefore, he states, patients suffering from ulcers, fatness, migrains and other syndroms, should be treated as having various types of super-ego, whose characteristics should be carefully described and analyzed.

Gastroduodenal ulcer is a psychogenic illness. In it, the root of the conflict seems to be placed in a latent childish attitude due to dependence on the mother which the patient tries to hide. This effort brings forth some tendencies opposed to passivity and dependence that in the end are creating paranoid distress in the patient.

Migrains are mainly due to the influence of some 'psychological blows' striking on the head. 'The repressed psychical conflicts and the related emotions would act upon the head of a predisposed individual as some noxious physico-chemical stimuli would do (Garma, 1973). According to this view, the genital organ would be substituted by the head, thanks to an intellectualization process.

Throwing-up and special wishes during pregnancy have also a psychological meaning of conscious or unconscious refusal of pregnancy. Such reactions also profit from an oral-digestive mechanism of response.

Asmatic psychism has also been related to circumstances in which an extreme dependency, mainly on the mother, seems to exist. The distress becomes unconscious, leading to breathing difficulties. The asmatic individual gets scared, then he cannot breathe and, finally, he is ill. Obesity, on the other hand, is explained by familiar overstimulation which leads the child to a hypertrophy of his sexual capacity, not reflected itself in a genital but in a digestive way.

A colitic patient is also suffering from a distress reaction; his diarrhoeal - emotival reaction before situations which dominate him, is the effect of his personal attitude in facing life.

Taken as a whole, Garma's studies of psychosomatic illnesses have deeply influenced the development of an Argentinian school in the specialty, and have favoured a better and more thorough approach to these problems from a psychoanalytic point of view.

He maintains a monistic conception, in which mind and body are to be taken in an integrative way. According to his basic tenets, all illnesses, with psychogenic roots, are variations of a broad function modulated by individual characteristics, on one hand, and, on the other hand, they should be viewed as means of development of some important psychoanalytic concepts, such as the role of Oedipus complex and the disorders of genital activity, the hiding and regression to the oral-digestive stage, the

important relationship between infantile traumatic situations and those experienced in the adult milieu, and the importance of intrauterine life for the psychogenesis of psychosomatic illnesses.

Garma's studies on dreams

From its very beginning Garma was interested in psychoanalytical activity focusing on the analysis of dreams. His research was initiated in the early 30's, and extended along 60 years (more than 20 works), his last work being a book on this same subject (Garma, 1990). It may be said that his scientific career begins and ends with some works on dreams.

Dreams are, in his opinion, as in classical psychoanalysis, the most direct route, "the royal road" to the domain of the unconscious, from which all the rest of psychic states and processes can be analyzed. His ideas are rooted in the Freudian theory, in which he has also introduced some novelties of his own.

Garma's main thesis is that dreams, better than fulfillment of wishes, as Freud characterized them, should be viewed as masked traumatic situations. Garma says (1974) that such an interpretation may be found in the works of Freud, although not adequately formulated by him in his wellknown studies on dreams.

In Garma's opinion, the theory of dreams as fulfillment of desires, does not allow for a real comprehension of the genesis of oniric hallucinations. This can be carried out when dreams are seen as masked traumatic situations (Garma, 1974). From his point of view, dreams may be characterized by their hallucinational dimension. If hysterical symptoms are defined by conversion, and perversions may be seen as a sort of orgasmic organization around a partial instinct, the dreaming experience is defined by its hallucinational experience (Garma, 1977).

In order to explain the genesis of oniric hallucinations, Garma worked out a theory of "reality testing" opposed to the Freudian one. According to it, hallucinatory processes are based on a what he calls "reality testing error".

As a matter of fact, Freud's point of view stressed the fact that, through "reality testing", the individual distinguishes between external and internal stimuli. From the former ones, the ego may escape, while from those coming from inside he can not.

Garma, on the contrary, supposes that, when "reality testing" is formed, during early childhood, the person cannot escape from external aversive stimuli, while he may get rid of the internal ones, turning them into unconscious contents as an effect of his defence mechanisms. The ego, hence, considers those inescapable stimuli as external, an opinion that places Garma in clear opposition to Freud.

This same mechanism functions in dreams, in Garma's opinion. Repressed contents, which originate dreams, cannot be dominated by the ego, as the capacity of its defence mechanisms decreases during sleep. The ego itself cannot get rid of them, nor elaborate them in a normal way, and as a consequence, the ego refers them to an external origin (reality testing error), changing their nature into a hallucinatory one.

Therefore, the hallucinatory form of dreams is due to the fact that its content acts against the ego in a traumatic way. As is wellknown, 'traumatic experiences', according to Freud, are those causing strong psychic excitations in the individual in a very short time, from which escape is not available and the patient is not able to elaborate these stimuli in a normal way, until he is finally able to mask them, making their contents acceptable to his super-ego.

Garma also introduced the concept of 'primary oniric hallucination', a process that would arise from a sort of mnemonic reactivation of the dazzling experience every individual would suffer at birth. And it also introduces what he calls a 'dream screen', a sort of background against which dream gets definition and form. Generally this screen, which symbolizes the mother's breast, is not perceived. These two, and the "manifest hallucinatory content" (the masked situations), are the three main components of dreams, according to Garma (1974).

Garma's analysis of dreams offered a great variety of subjects and themes. He was also interested in approaching the study of wellknown literary works with such a method, as some of the Spanish Golden Literature - Calderon, etc.

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