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PREFACE

The first European meeting of Cheiron, the International Society for the History of the Behavioral and Social Sciences, took place at the Free University of Amsterdam, September 15 - 17, 1982.
During the symposium, 20 papers were presented. Four more papers, which had also been submitted, could unfortunately not be accomodated in the program.
Some of the papers presented were already committed for publication elsewhere, in one case two related papers were combined into one, and in still other cases the papers were considered not yet ready for publication by their authors (see enclosed program). We included 2 invited papers. Therefore, 18 papers, often entirely reworked, are contained in these proceedings.
The editors are aware of the fact that in some cases the quality of the English may strike the native speaker as less than perfect. Notwithstanding, we have decided to publish these papers as quickly as possible, but in future more attention will be paid to the linguistic aspects.
We hope that these proceedings may mark the beginning of an annual series. This should encourage the circulation of ideas among the members of the expanding network of researchers in the history and theory of the behavioral and social sciences.

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

Sacha Bem
Willem van Hoorn
Hans Rappard

Leiden, August 1983

PROGRAM

(with the original titels of the papers)

First Session: History of Psychology (1)

Chair: Hans Rappard (Amsterdam, F.U.)

- Bernard Singer (Reading): David Hartley
- Jeroen Jansz & Willem van Hoorn (Leyden): Freud's two definitions of Instinct
- Fernando Vidal (Paris) & Jacques Vonèche (Geneva): Uses and Abuses of Piaget's Autobiography

Second Session: National Developments

Chair: Jacques Vonèche (Geneva)

- Erika Apfelbaum (Paris) & Ian Lubek (Guelph, Canada): Augustin Hamon - Aux Origines de la Psychologie Sociale Française
- Ruud Abma (Nijmegen): Psychology between Science and Society - Development of Psychology at the Catholic University of Nijmegen (N.L.)
- Helio Carpintero & Jose Peiro (Valencia): Contemporary Psychology in Spain
- Jan Zielonka (Warsaw; on leave): Social Philosophy, Emancipation and Rationality - The Polish Experiment

Third Session: Philosophy & Theory

Chair: Stefan Amsterdamski (Warsaw) (was unable to come)

- Sybe Terwee (Leyden): Wittgenstein's Contribution to Theoretical Psychology
- Benny Shanon (Jerusalem): Introspection - a new appraisal
- Han De Wit (Amsterdam, F.U.): Semantics of Psychological Languages

Fourth Session: Historical Psychology

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- Willem van Hoorn (leyden): Psychology and the Reign of Technology
- Jerzy Bobryk (Warsaw): Medieval-Renaissance Episteme and the Concept of Person
- Harry Peeters (Tilburg): Historical Psychology, Theoretical and Methodological Aspects

Fifth Session: Methodology

Chair: Harry Peeters (Tilburg)

- Helio Carpintero & José Peiro (Valencia): Working Groups in American Psychology - A Study of 'Invisible Colleges' in Two Psychological Journals

- Marc de Mey (Ghent): Bibliometric Methods and the Intellectual History of Scientific Specialties: Citation Tracing and the History of Psycholinguistics
- Helio Carpintero & José Peiro (Valencia): Eminent Authors in American Psychology - A Study Trough Citations in Two Psychological Journals

Sixth Session: History of Psychology (2)
Chair: Larry Ray (Lancaster)

- Antonio Caparrós (Barcelona): Technology and History of Psychology
- Sacha Bem (Leyden): Context of Discovery and Contextual History of Psychology
- Ferenc Erös (Budapest): Freudo-Marxism in Hungary - Some Parallels Between W. Reich & A. József
- Sandie Lovie (Liverpool): Images of Man in Early Factoranalysis - Psychological and Philosophical Aspects

FREUD'S TWO DEFINITIONS OF INSTINCT

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Freud's two definitions of instinct[1)]

Introduction

In their note preceding *Instincts and their Vicissitudes* (Freud, 1915 c), the editors of the Standard Edition deal with the concept of instinct *(Trieb)* in Freud's writings. The editors indicate that throughout Freud's work, *two* definitions of instinct can be found. In the first definition no distinction is drawn between the instinct and its mental representation. This definition, can be found in Freud's discussion of the Schreber case (1911c)[2)]), and in the *Three Essays on Sexuality* (1905 d, 1915 addition, CPW-VII-168), where Freud considers the instinct as "the psychical representation of an endosomatic, continuously flowing source of stimulation ... a concept lying on the frontier between the mental and the physical". The definition in *Instincts and their Vicissitudes* (1915 c), which is discussed below, corresponds with this definition.

The second definition is presented in *The Unconscious* (1915 e) and *Repression* (1915 d) and is discussed in this paper as well. In this second group of definitions, the instinct is not regarded as the psychical representation of somatic impulses, but as a non-psychical entity. The editors of the *Standard Edition* suggest that "the ambiguity of the concept (of instinct, jj, wvh) itself", is responsible for this confusing problem of two definitions (CPW-XIV-113).

In this paper, we propose an alternative way to interpret Freud's two definitions of *Trieb*.

In our view psychoanalysis is a *conflict psychology* which, in general terms, deals with the conflict between numerous sexually colored whishes in the individual striving for fulfilment, on the one hand, and culture that forbids fulfilment of most of these whishes, on the other. The perennial hostility which exists between human sexuality and human culture forms a *Leitmotiv* in Freud's works: "...the inverse relation holding between civilization and the free development of sexuality..." (1905 d, CPW-VII-242)[3]).
One way or the other, whishes are related to instincts. In a comparison of instincts and stimuli from the environment, Freud tries to establish what instincts are. He points out three differences between an environmental stimulus and an instinct. The first difference is related to the source: the stimulus comes from an environment outside the human body. The instinct has a basis inside the organism. So, and this is the second difference, the organism cannot escape (from) the influence of the instincts. In the case of exogenous stimuli, however, escape or avoidance are very well possible. The third difference: often, the exterior stimulus has the character of an instaneous push. The instinct, however, which is related to the permanent need of acquiring lust, affects the organism as a constanct force. From the comparison with an exterior stimulus, we can conclude that the instinct has a powerful and never ceasing influence upon the organism[4]).

Before turning to a more precise definition of instinct, we have to remark something about the use of the term instinct. For the sake of formulation we use intinct in the singular. Strictly speaking, this is not correct. We have to conceive of the instincts as a composition of numerous *Partialtriebe* (component instincts). So, reducing instincts to the instinct of sex and the instinct of agression, is obviously not correct.

Moreover, we have to remark that the editors of the Standard Edition have decided to translate consistently Freud's German term *Trieb* with *instinct*. This, in our view is most unfortunate, since Freud clearly distinguishes between *Trieb* and *Instinkt*: Brandt

(1961) emphasizes the different connotations of *Trieb* and *Instinkt*: "(...) *Trieb* conveys the idea of action, motion and energy. It is a force. It does not imply any direction. (...) Instinct does not imply an active force but only a tendency. The idea of direction is not excluded, since we speak about the migration of birds as the result of an instinct which directs the birds to fly a specific route"[5]). Whatever the problems caused by the translation are, in this paper we use the term instinct for reasons of convenience.

The two definitions

In 'Instincts and their Vicissitudes' (1915 c), Freud states that "(...) an 'instinct' appears to us as a concept on the frontier between the mental and the somatic, as the psychical representative of the stimuli originating from within the organism (...)" (CPW-XIV-121)[6]). In this complicated definition Freud tries to elucidate one of the essential meanings of instinct in psychoanalytic theory. The instinct may be conceived of as a *"Grenzbegriff"*, a borderline notion between the mental *(das Seelische)* and the somatic *(das Somatische)*. The stimulus source of the instinct is located *in* the organism; the instinct is strictly speaking the *mental* representation of the physical stimuli. In connection with this definition, which is also clearly related to the *economic* viewpoint, Freud discusses four interrelated notions, viz. the pressure *(Drang)*, the aim *(Ziel)*, the object *(Ojekt)* and the source *(Quelle)* of an instinct.

Immediately after he had finished *"Instincts and their Vicissitudes"* (1915 c), Freud prepared the text of two other crucial metapsychological essays: *Repression* (1915 d) and *The Unconscious* (1915 e).

In *The Unconscious* Freud presents another definition of instinct, which, at first glance, seems totally different from the definition just discussed. Here we read that: "An instinct can never become an object of consciousness - only the idea *(Vorstellung)* that represents the instinct can. Even in the unconscious, moreover, an instinct cannot be represented otherwise than by an

idea. (...) When we nevertheless speak of an unconscious instinctual impulse (...) we can only mean an instinctual impulse the ideational representative of which is unconscious, for nothing else comes into consideration (1915 e, CPW-XIV-177)[7]).

Thus, in short, in the first definition an instinct is conceived of as the psychical representative of endogenous energy (1915 c), whereas according to the second definition, the instinct can only be represented by an idea (1915 e).

Now that we have presented Freud's *two* definitions of instinct we may proceed to point out the important differences and similarities between both formulations.

In *The Unconscious* (1915 e), the instinct is not considered as the psychical representation of a somatic entity. Freud here distinguishes between the instinct and its ideational representation, which as such can become an object of either conscious or unconscious mental processes. In view of this distinction, the instinct should be regarded as a *non-psychical entity!*

How to explain the difference between these two definitions? One has to take into account that both were formulated within a time interval of several weeks.

A closer look at the *contexts* of the definition may be helpful. In *Instincts and their Vicissitudes* (1915 c), Freud focuses his explanation on the notion of the instinct as a *"Grenzbegriff"*. This type of definition may be characterized as structural, topical and economic. The latter characterization clearly follows from the second part of Freud's 1915 c definition where we read: "(so erscheint unds der "Trieb")... als ein Mass der *Arbeitsanforderung*, die dem Seelischen infolge seines Zusammenhanges mit dem Körperlichen auferlegt ist" (1915 c; SA-III-85, italics added).

The frame of reasoning in *The Unconscious* (1915 e) is rather different. In the latter text, Freud is preoccupied with the clarification of the distinction between conscious and unconscious mental processes. The casually used nouns "conscious" and "unconscious" are often misleading as they suggest that one could point at the unconscious or the conscious "parts" of the human mental apparatus.

The unconscious, however, can be considered in two ways. Firstly, in terms of the *dynamic* model of the human psyche, and secondly, by giving the unconscious its place in the structure of the human psyche: the *topical* model of the mind (cf. Freud, 1900 a; 1915 e and 1923 b). Already in the *Traumdeutung* (1900 a), Freud adheres to a fundamental dualism of conscious and unconscious mental processes (dynamic viewpoint). Furthermore, he considers the unconscious as the phylogentic older part of the mental apparatus (*topical* viewpoint).
Thus, Freud has no trouble defining the instinct *topically* in the 1915 c paper and viewing the instinct *dynamically* in the 1915 e paper.
Finally, in the *Ego and the Id* (1923 b), both viewpoints are combined into the structural model of the mind. Ego, superego and id are intrinsically related to the dynamic relationship of consciousness-repression-unconscious.

The dynamic view of the unconscious

Next, we will deal with the notion of the unconscious in terms of the dynamic model of the human mind. In our view, Freud has transformed the originally Romantic notion of the unconscious into an intrapersonal, *repressed* unconscious. In the works of romantic philosophers, poets and novelists, the notion of the unconscious and repression play an important role. Surely, we are confronted with the existence of 'an inner man', *'zwei Seelen wohnen, ach in meiner Brust'* (Goethe), and with the daemonic aspects of the depths of our souls (Jean Paul).
During the romantic movement and the greater part of the 19th century, der *Doppelgänger* flourished as never before (cf. Otto Rank, 1914).
However, it took a middle-class bourgeois thinker like Freud to equate the renunciation of sexuality and the concept of repression (Freud, 1910 k) and, to complete the picture, in his 1915 d and 1915 e metapsychological papers, already referred to, Freud states

that repression and the unconscious can hardly be distinguished from one antoher. "This suggests that the concepts of renunciation, repression and unconscious mental processes might be interchangeable" (Van Hoorn, 1982). This idea forms the basis of Freud's *conflict* psychology. The Freudian unconscious is a battlefield in itself. Only the sounds and forces that reach consciousness are known to the subject. Repression keeps ideas *(Vorstellungen)*, which cannot be tolerated by individual morality and culture, in the unconscious. Does this mean that the instincts cannot be satisfied? The answer to this question is in the negative. The quantum of energy that has been cathexed to the repressed idea, can also be cathexed to another idea which is tolerated by culture. Thus seen, the unconscious is characterized by an impressive mobility of the cathexes *("grosse Beweglichkeit der Besetzungen")*. The tremendous mobility of the cathexes explains why laws pertaining to our conscious experience do not hold for events taking place in the unconscious. With regard to the notion of *time*, e.g., we may remark that processes of time and duration do not play a role in the unconscious*. If neither time, nor duration are of significance, does this imply that the world of the unconscious is chaotic? On the contrary: 'exemption from internal contradiction, primary processes (mobility of the cathexes), timelessness, and replacement of external by mental reality - these are the characteristics which we may expect to find in processes taking place in the unconscious' (1915e, SA-III-146; CPW-XIV-187, with slight alterations). This line of reasoning reaches its culmination point in *Civilization and its Discontents* (1930 a). Here Freud uses what Van Hoorn (1979) has called "the eternal Rome paradigm". In a plendid passage, Freud compares the contents of the unconscious with the city of Rome in which *all* buildings, from the time of *Roma Quadrata* up to the

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* Or, in terms of Lovejoy's (1936) description of the temporalization of the chain of being, Freud's notion of the unconscious forms the grand anti-temporalization theme of 20th C. psychology.

present, have been preserved! In the material world such a development is clearly absurd - the same space cannot have two different contents. In the realm of the mind, however, nothing which has once come into existence "will ever pass away and the earlier phases of our *mental* development exist alongside the latest one" (1930 a; CPW-XXI-69-71;SA-IX-201-203).

The complementarity of the two definitions

Now, better equipeed, we may return to Freud's definitions of the instinct in *Instincts and their Vicissitudes* (1915 c) and *The Unconscious* (1915 e). In the earlier essay, Freud's frame of reference contains an exposure about instincts in biological, physiological and quantitative terms. Or, in other words, it is Freud's *unsolved* struggle to clarify the *mind-body-dualism* which determines the form of the definition of instinct in the earlier paper.
Within the limited space of this paper, a short overview must suffice. Careful study of Freud's early publications shows that he rejected Meynert's strict cerebral localiza' 'on of mental processes (Freud, 1891 b). In addition we may state that Freud did not adhere to Fechner's theory of *psychological parallelism*, because in his opinion, physiological processes are not necessarily correlated with mental processes.

The *Studies in Hysteria* form Freud's point of no return. The introduction of the concepts of repression and unconscious mental processes, forces him to a *psychological* explanation of the functioning of the mental apparatus. And in the meta-psychological papers of 1915, he explicitly declares that it would be the completion of psychoanalytic research if we succeed to describe a mental process at the same time from its dynamic, topical and economic interrelatedness. In the case of Freud's two definitions of instinct, we obviously have the task to systematically relate the topical and the dynamic viewpoints. Then, against the background of the mind-body dualism, we can state that in *Instincts and their Vicissitudes* (1915 c), the instincts, i.e. the psychical represen-

tative of endogenous energy, forms a stimulus for the mental apparatus. However, it is not allowed to identify the instinct and the psychical stimulus! Obviously there are more stimuli influencing the mental apparatus, than the instinctual stimuli. In *The Unconscious* (1915 e) Freud tries to clarify the distinction between conscious and unconscious mental processes from a meta-psychological point of view.

In this context Freud relates his concept of instinct to the mechanism of repression: it is not the instinct itself which is repressed, but its ideational representation. By talking about the instinct in terms of the distinction between conscious and unconscious mental processes, Freud clearly gives a *dynamic definition* of instinct in *The Unconscious* (1915 e).

However, Freud uses the topical viewpoint in *The Unconscious* (1915 e) as well. One might conclude that our solution contains a paradox. Yet, this paradox can be explained. In the 1915 e paper on the unconscious, Freud deals explicitly with the topical viewpoint in the sections II, IV and VII.

His definition of instinct is given in section III called "Unconscious feelings". In this section, as in sections V and VI, Freud unequivocally deals with a dynamic interpretation of unconscious mental processes. It is in this *specific* context that Freud declares: "I am really of the opinion that the opposition of conscious and unconscious does not apply to the instinct" (1915 e, SA-III-136; CPW-XIV-177).

Our interpretation is corroborated by the fact that in discussing the defence mechanism of regression, Freud also uses the dynamical definition of instinct. In *Repression* (1915 d), another meta-psychological paper written in 1915, the same definition of instinct is given as in *The Unconscious* (1915 e).

"We have reason to assume that there is a primal repression, a first phase of repression, which consists in the psychical (ideational) representative of the instinct being denied entrance into the conscious" (1915 d, CPW-XIV-148), and "In our discussion so far we have dealt with the repression of an instinctual represen-

tative, and by the lattter we have understood an idea or group of ideas which is cathected with a definite quota of psychical energy (libido or interest) coming from an instinct" (1915 d, CPW-XIV-152)[8]).

In conclusion

Both of the *seemingly* different views of the nature of an instinct are to be found all over Freud's later writings. The contradiction is certainly more apparent that real, viz. related to the dynamic model of the human mind, on the one hand, and the structural one on the other. The editors of the *Standard Edition* try to solve the apparent contradiction by pointing to the *ambiguity* of the concept itself - a frontier concept between the physical and the mental (CPW-VXIV-113). Though this way of handling the issue may be considered as good psychoanalytic practice, it seems more advisable to consistently keep different contexts in mind[9]). Thus, when Freud talks about the instincts from the dynamic point of view in which conscious and unconscious mental processes and the repression of sexuality are intrinsically related, the instinct and its ideational representation are considered as two entities. When Freud deals with the instincts from a topical viewpoint, no distinction is made between the instinct and its mental representation.

Notes

1) In this paper we only deal with Freud's two definitions of instinct (*Trieb* = drive) in the metapsychological papers of 1915. We have deliberately refrained from describing the development of Freud's theory of instinct from *Jenseits des Lustprinzips* (1920 g) on. In his later theory, Freud more and more moves away from *current* biology. The culmination point of this development is to be found in the *Neue Vorlesungen* (1932/33) and the *Endliche und Unendliche Analyse* (1937 c) where Freud writes: "Die Trieblehre is sozusagen unsere Mythologie. Die

Triebe sind mytische Wesen, grossartig in ihrer Unbestimmtheit" (1932/33, SA-I-529; CPW-XXII-95). And, "Manchmal könnte man zweifeln, ob die Drachen der Urzeit wirklich ausgestorben sind" (1937 c, SA-Erg.-369); CPW-XXIII-229). In the unconscious 'nothing can be brought to and end, nothing is past or forgotten" (1900 a, SA-II-342; CPW-V-347). It is the indestructible and immortal desires from the unconscious, 'the dragons from primeval time', which as manifestations of our instinctual heritage shape cultural history and our individual lives. Freud's ultimate theory of the instincts constitutes psychoanalysis as myth.

2) Freud, 1911 c: "Wir fassen den Trieb als den Grenzbegriff des Somatischen gegen das Seelische, sehen in ihm den psychischen Repräsentanten organischer Mächte ... (SA-VII-196; CPW-XII-74).

3) In the original German the passage reads as follows: "Wegen der gegensätzlichen Beziehung zwischen Kultur und freier Sexualitätsentwicklung, deren Folgen weit in der Gestaltung unseres Lebens verfolgt werden können ..." (1905 d, SA-V-144). For the hostility between instinctual life and human culture, see also Freud, 1891 a, 1908 d, 1912 d and 1930 a.

4) The distinction between the influence of an exterior, instantaneous stimulus and an interior long-lasting pressure, was already made by Freud in his 1895 b paper: "...weil die exogene Erregung wie ein einmaliger Stoss, die endogene wie eine konstante kraft wirkt" (1895 b, SA-VI-46; CPW-III-112).

5) L.W. Brandt, *Some notes on English freudian terminology* in: The Journal of the American Psychoanalytic Association. (Vol. IX, 1961, p. 337-338).

6) In the original German: "(...), so erscheint uns der 'Trieb' als ein Grenzbegriff zwischen Seelischem und Somatischem, als psychischer Repräsentant der aus dem Körperinnern stammenden, in die Seele gelangenden Reize, ...". (1915 c, SA-III-85).

7) In the original German: "Ein Trieb kan nie Object des Bewusstseins werden, nur die Vorstellung, die ihn repräsentiert. Er kann aber auch im Unbewussten nicht anders als durch die Vorstellung repräsentiert sein. (...) Wenn wir aber doch von einer unbewussten Triebregung (...) reden, (...) Wir können nichts anderes meinen als eine Triebregung, deren Vorstellungsrepräsentanz unbewusst is, denn etwas anderes kommt nich in Betracht" (1915 e, SA-III-136).

8) In the original German, these two quotations read as follows: "Wir haben also Grund, eine *Urverdrängung* anzunehmen, eine erste Phase der Verdrängung, die darin besteht, dass der psychischen (Vorstellungs-) Repräsentanz des Triebes die Übernahme ins Bewusste versagt wird". And: "In den bisherigen Erörterungen behandelten wir die Verdrängung einer Triebrepräsentanz und verstanden unter einer solchen Vorstellung oder Vorstellungsgruppe, welche vom Trieb her mit einem bestimmten Betrag von psychischer Energie (Libido, Interesse) besetzt ist" (1915 d, SA-III-109 and 113).

9) In at least six articles of their *Vocabulaire de la Psychanalyse* (1967; Germ. ed., 1980[4]), Laplanche and Pontalis discuss aspects of Freud's two definitions of instinct. With regard to the two formulations in 1915 c and 1915 e, these authors, contrary to the standpoint of the Editors of the *Standard Edition*, come to the conclusion that:

   a) there is no *development* in Freud's definitions of instinct
   b) it is out of the question to consider Freud's formulation in 1915 d and 1915 e as his *final* conception. As a matter

of fact, the earlier formulation of 1915 c is also to be found in the *Abriss der Psychoanalyse*, written in 1938.

Laplanche and Pontalis try to solve the 'contradiction' between the two definitions as follows:

- although, at first sight, the two formulations are contradictory, there is one and the same principle present in both, viz. the relationship of body and mind is neither conceived as psychophysical parallelism nor as one of causality (in this respect we agree with Laplanche and Pontalis). Body and mind are related to each other as a delegate and his mandator.
- still, according to Laplanche and Pontalis, there is also a difference in the two formulations. The definition from 1915 e, in which the *somatic* instinct delegates its ideational representative to the realm of the mental, seems to be more precise. In this case we do not only deal with a global indication - a 'borderline notion' *between* the mental and the somatic - but we are also confronted with the idea of the *recording of ideas*, which is intimately related with Freud's conception of the unconscious (see Freud's letter to Fliess: 6-12-'96).

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THE ROLE OF AUTOBIOGRAPHY IN THE SOCIAL SCIENCES
THE CASE OF JEAN PIAGET

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Summary

This paper explores some of the functions that autobiography fulfills within the field of the social sciences. In the case of psychology, an autobiography often presents the life history of a thinker as an illustration of the thinker's theory, as an explanation of the theory in terms of the theory itself. Thus, it contributes to legitimize the theory in question, and surreptitiously to transmit beliefs, assumptions, and ideologies associated with the theory. For example, Jean Piaget's autobiographical sketch reduces the origins of Piaget's theory to a series of cognitive events progressing according to the conception of progress inplicit in the theory itself. This "cognitivizing" or rationalizing function becomes sociologically significant when commentators or popularizers reproduce Piaget's autobiography with the hope that it will provide the key to understanding the true sense of Piaget's work. The "autobiographical" reconstruction of a theory and the life of its creator fulfills some of the functions of myth: to chanell questioning, to restrict interpretation, and to assure the naturalness, legitimacy and inevitability of both history and ideas.

The aim of this paper is to explore some possible functions of Piaget's autobiography, and the uses to which it might have been put within the "Piagetian" movement. We shall first make explicit

some of the ideas underlying our work. As far as the writing of history is concerned, we try to take into account the relations between an individual life (both as it is narrated in an autobiography, and as a biographer may piece it together), and the history of concepts and institutions (again, both as it is rendered by those using the concepts and pertaining to the institutions, and as a historian may restore it). We therefore believe that an autobiography is a document that should not be used as if it were just a repository of facts, but as if it were also a proponent and guardian of the goals and viewpoints that oriented the selection and organization of the facts.

This approach to the writing of history leads us to the following question: What are the differences between the historical "truth" that it seems we are out to discover, and the autobiographical "misrepresentation" that it seems we wish to denounce? Whenever it can be assumed that both the autobiographer and the historian are trying to be honest and objective, and neither is a mythomaniac, we believe that they cannot be radically opposed to each other. We accept the notion that the historian's narrative is a story, and we reject the scientistic contempt for mythopoetic history. We believe that facts (which, as Barthes shows in *Mythologies*, can be the same in history as in myth) have no meaning by themselves, but that human activity can give them meaning. Thus, we prefer to investigate the functions fulfilled by different kind of narratives, than to argue (implicitly or explicitly) for the superior truthfulness of one kind.

The Autobiography of Psychology

Jean Piaget's first autobiographical sketch was published in 1952, in the fourth volume of *A History of Psychology in Autobiography*. The series was begun by Carl Murchison and the Clark University Press; its first volume appeared in 1930. One of the early contributors, Charles Spearman, opened his chapter by indicating the attraction of Murchison's invation "to write one's own 'intellectual history', accompanied as it is by a suggestion that this may

be helpful to younger men with their lives still to make" (Spearman, 1930, p. 299). This didactic and formative purpose disappeared after World War II, as the convenient but somewhat misleading personification called "American psychology" started to predominate, and to feel sure and proud of its progress and of its scientific and professional nature.

Representative of such transformations is the preface to the fifth volume, of 1967, which related that, in the past, editors has asked the contributors to "tell of the motivations that guided them in their professional careers, not fully realizing in the then unformed state of motivations psychology how little a man knows correctly of his own motivations (Boring and Lindzey 1967, p. vi). Naturally, by the behavioristic 1950s, the editors had become enlightened, and "the invitation was changed to stress conscious motivation less and the events of the life more" (from the 1952 preface, in Boring and Lindzey, 1967, p. vi). Contributors were then told that the aim of the series was to present intellectual and professional life histories, "illuminated by as much information about your personal background and inner motives as you are ready and able to divulge" (Boring and Lindzey, 1967, p. vii). The 1952 preface explained that, in spite of the limitations and difficulties inherent in writing an an autobiography, what the autobiographer "tells about himself and what he shows about his values can ... go far toward instructing the reader as to how human motive moves to make science progress. The accidents of living do not always seem irrelevant to progress when they operate in the manner shown in the pages of this book (Boring and Lindzey, 1967, p. vi).

Since the project of a history of psychology in autobiography aimed so explicitly at nourishing the progressionistic view of at least some groups within the field of psychology, it is to be expected that such view will be found in at least some of the autobiographies. To imply that one has contributed to progress amounts to establishing the legitimacy and veracity of one's ideas, and of one's domain at large.

The Function of Autobiography

It is possible that, within the field of the social sciences, great creator's autobiographies (and some biograhies) play a role that it would be hard for them to play in, say, the field of physics or mathematics. The key of the function of (auto)biography in psychology may be its often being a presentation of the life history of a thinker as an illustration of the thinker's theory, as an explanation of the origins of the theory in terms of the theory itself. B.F. Skinner explicitly makes the points by opening his autobiography with a section on his "early environment". He also says that, after having given up the literary ambitions of his college years, his "extraordinary luck" kept him "from becoming a Gestalt or (so help me) a cognitive psychologist" (Boring and Lindzey, 1967, p. 397). However, he did not give up literature altogether, since he became interested in it "as a field of behavior to be analyzed". "As a boy", he recalls, "I knew two interesting cases of verbal behavior" (Boring and Lindzey, 1967, p. 401). Moreover, the woman he married had studied literature and, Skinner writes, "she attended my lectures on the psychology of literature and reinforced me appropriately" (Boring and Lindzey, 1967, p. 401). His lifelong "behavior as a scientist" is summarized in the selection of his most important articles entitled *Cumulative Record*. Finally, Skinner affirms that behaviorists see, explore, and manipulate themselves in the same way as they see, explore, and manipulate their subjects (Boring and Lindzey, 1967, p. 407).

The case of Freud's life history, as it is narrated within the psychoanalytic movement, illustrated the phenomenon at a far larger scale. In *Freud, Biologist of the Mind*, Frank Sulloway (1979) argues that "the chief aim of psychoanalist-historians ... was to show that psychoanalysis emerged in a manner that, above all, was consistent with psychoanalytic theory itself" (p. 442). Because the legend and the mythology transmitted by those historians contributed to hide embarrassing but necessary conceptual elements of Freud's ideas, and served to justify and promote the orthodox

followers' monopolization of legitimate psychoanalysis, to question Freud's offical biography amounted to questioning the theory he had created. It is not hard to see that, to a certain extent, the psychoanalist-historians were all writing their own biographies, legitimating their lives through that of a heroic father, vicariously trying to escape error and oblivion.

Autobiography is always written from the retrospective viewpoint of a person interpreting his past; its form and content largely depend on what the person is at the time of writing, and part of its function is to preserve and be true to the writer's personality. (On this subject, see Weintraub, 1978). At the same time, however, an autobiography will affect its author's very being; to a certain extent, the autobiographer will become the true subject of his own narrative. Thus, one may find in the social and intellectual constitution of psychology that a great figure's autobiography can furnish a recapitulationist collective history. By narrating the development of a theory of mind and development through the development of someone wo turns out to develop as the theory says, such autobiography becomes a figure of thought essential to the "rhetoric of scientificity" (Bourdieu, 1976). Through this rhetoric, a group manages to bring about the belief in the scientific nature of its product and in the scientific authority of its members, and introduces a variety of ideologies - all of it as it were merely stating unquestionable natural events.

Piaget's Autobiography

The situation we examine here is in between that of Skinner and that of Freud. Although Piaget's autobiography has not given rise to a historical production from within the "Piagetian" movement, it is abundantly used by the movement. The authors we shall cite in the following section are commentators or popularizers of Piaget's work. They are psychologists; however, they work under the biographical illusion, once pervasive in the history of literature, according to which theories are grounded for the most part on the life of great individuals. This is not as surprising as it might

be, since Piaget's autobiography operates under the analogous illusion of an isomorphism between the stages of the author's own life and those of his own theory. In addition, it tends to reduce the origins of the theory to a series of purely intellectual events progressing according to the conception of progress implicit in Piaget's own rationalistic theory. In particular, the description of Piaget's passage from biology to philosophy, and then to psychology, tends to assure the "scientific" nature of genetic epistemology, by eliminating those aspects of Piaget's enterprise that are most difficult to reconcile with the assumptions that underlie its claim of being exclusively "scientific".

Emblematic of the message Piaget's autobiography wishes to transmit is the title of one of its versions (Piaget, 1972, ch. 1): "An account of and an Analysis of a Disenchantment" - of Piaget's disenchantment with philosophy. This narrative basically describes the progress of Piaget's thought from an "egocentric" stage dominated by fanciful metaphysics and speculations, to a logical and formale stage endowed with the "decentered" objectivity of scientific knowledge. Its opening, for example, smuggles a scientistic view of science and, at the same time, silences possible questioning by declaring the supposed limits of what the public is about to read. Piaget writes (1952, p. 237):

> An autobiography has scientific interest only if it succeeds in furnishing the elements of an explanation of the author's work. In order to achieve that goal, I shall therefore limit myself essentially to the scientific aspects of my life.

Piaget goes on to tell about his dedication, from age eleven, to mollusk taxonomy, and concludes that his early scientific studies functioned "as instruments against the demon of philosophy. Thanks to them", he writes, "I had the rare privilege of getting a glimpse of science and what it stands for before undergoing the philosophical crises of adolescence" (Piaget 1952, p. 239). He claims to have been formed "by a precise problem: that of species and of their indefinite variations as a function of the environment, that of the relations between genotypes and phenotypes, with predilection for

the study of adaptions ... In short, "Piaget argues, "since then I have always thought in terms of forms and their evolution" (1959, p. 9).

In such statements - which are "accurate" only if one accepts his psychological and philosophical assumptions - Piaget subsumes under "science" a predominantly descriptive and classificatory natural history. He also forgets that, as our study of his youth tends to show, the interest in life, evolution, and adaptation that may be put in strict continuity with his later interests developed within the Bergson-inspired framework of a philosophical biology, at a time when he had given up natural history (Vidal, 1981a, 1981b; Vidal *et al.*, 1983). Yet, his autobiographical narrative enables Piaget to establish at the outset his image as a life-long scientific biologist and, therefore, to recall without danger the emergence of his philosophical concerns.

The young Piaget had become very concerned with the apparent conflict between science and faith. After finding a temporary solution to that conflict in the evolutionary interpretation of religion set forth by the Protestant theologian August Sabatier, he had the quasi-mystical experience of reading Henri Bergson's *Creative Evolution*. He writes (1952, p. 240).

> First of all it was an emotional shock. I recall one evening of profound revelation. The identification of God with life itself was an idea that stirred me almost to ecstasy because it now enabled me to see in biology the explanation of all things and of the mind itself.
>
> In the second place, it was an intellectual shock. The problem of knowledge ... suddenly appeared to me in an entirely new perspective and as an absorbing topic of study. It made me decide to consecrate my life to the biological explanation of knowledge.

In a complementary version, Piaget ends his account saying: "je consacrerais ma vie à la philosophie avec pour but central de concilier la science et les valuers religieuses" (1972, p. 12). Yet, after such statements, the problem of religion immediately

disappears from the autobiography, which will make room neither for it, nor for the host of moral questions Piaget became concerned with since about the age of sixteen. But since the "emotional shock" appears early on, and just where it ought to, at the onset of adolescence, it can be forgotten, superseded by the cognitive growth towards objectivity that apparantly followed the "intellectual shock". As Jorge Luis Borges has written in a short story entitled "La otra muerte" ("The other death"), "modificar el pasado no es modificar un solo hecho; es anular sus consecuencias, que tienden a ser infinitas" ("modifying the past is not changing a single fact; it is abolishing its consequences, which tend to be infinite").

Thus, the autobiography continues, Piaget started to explore the epistemological problem "from the perspective of a scientific biology", (Piaget, 1959, p. 9). He created a system that anticipates his theory of equilibration and, in the course of trying to escape its speculative nature, discovered that psychology might provide its empirical testing ground: "Between biology and knowledge", he says, "I needed something other than philosophy" (1952, p. 240). This narrative closes the developmental explanation of the origins of genetic epistemology.

Let us observe, however, that before setting forth his system in a 1918 philosophical novel and *Bildungsroman* called *Recherche*, Piaget had expressed deep social, moral and regilious concerns in his 1915 prose poem *La mission de l'idée*, which he does not mention in his autobiographies. This poem is important since, for example, the identity it asserts between evolution and morality strengthened Piaget's rejection of Darwinism, which had already taken place in the course of scientific debate, and under the influence of Bergson's philosophy (see Vidal *et al.*, 1983). As Piaget points out, *Recherche* anticipates his theory of equilibration. Yet, he does not state that his system aimed at uniting biology, knowledge, *and* morality. He de-emphasizes the importance of moral concerns, even though his main problem at the time was, as he wrote in a 1917 letter, "to base morality on science" (Piaget, 1917). When Piaget turned to psychology to verify his system (as he acknowledges), he

started to carry out not only his well-know epistemological enterprise, but also a moral enterprise that culminated in 1932 with his book on *The Moral Judgment of the Child* (see Vidal, 1980). Yet, the reader can hardly notice the emergence of this latter enterprise, which Piaget dilutes through certain omissions, through his comments about the "adolescent" and "preliminary" nature of his first books (Piaget, 1959, p. 10), and through his story about the haphazard origin of his work on moral judgment (in Evans, 1973, p. 37).

In conclusion, the "cognitivization" of Piaget's biography is carried out by means of three solidary narratives: one, on the purely "scientific" development of Piaget's interests and point of view; another, on the purely "epistemological" motivation of his passage from biology to philosophy and then to psychology; and a last one, on the absence of anything like a "moral enterprise" persisting beyond adolescence. Thus, the "purification" of life proceeds along the same lines and according to the same criteria in the autobiography as it does in the theory of the autobiographer. Piaget's system aims at establishing "isomorphisms" between biology and knowledge; his autobiography aims at establishing analogous correspondences between the stages of his developmental theory and the growth of his thinking. At each level, the central process consists of the formation of increasingly abstract structures that include and transcend the previous ones. Moreover, since Piaget's theory assumes that such process is natural and spontaneous rather than historical, his autobiography manages to transform a historical product (his theory) into a natural one. This "naturalization" of history fulfills one of the main functions of myth, which is to present history as a simple matter of course.

Piaget's "Autobiographers"

the naturalizing function of the autobiographical myth is made to stand out by the large number of commentators, popularizers, and divulgators of Piaget's theory who have reproduced the master's autobiography. Since a theory is an interpretation of reality, it is not surprising to see Piaget (or any social scientist) inter-

preting his own life through his own theory. However, it is harder to understand why his commentators do not put such interpretation in a critical perspective, unless we assume that they also see their lives as a Piagetian process, or that they think that such process applies to Piaget's life but not to theirs. Piaget's "autobiographers" usually place a sketch of Piaget's life (and especially of his youth) at the beginning of their books. They generally do so without revealing their purpose; however, the ones who do, affirm their professional belief in the explanatory power of developmental descriptions. A North-American author, for example, says that although an overview of Piaget's life is interesting in its own right, its main importance is that "it may serve to illuminate the dark corners of Piaget's theory" (Brainerd, 1978, p. 1). More emphatically, his South-American colleague declares that "a thinker's life often provides the key to understand the true sense of his work; but it is rare to find in a scientist's life elements as significant as those in Piaget's personal history" (Battro, 1969, p. 9).

Those illuminating elements concern especially how biology entered the constitution of genetic epistemology and Piagetian psychology. Popularizers hope that their biographical sketch will contribute to justify, legitimize, and explain (or explain away) the role of biology in Piaget's system. To start with, they adopt Piaget's version of the psychological significance of his early malacological work. One commentator, for example, says that this work gave Piaget "a very firm grounding in the principles of scientific method and observation" (Fancher, 1979, p. 342). (Note the hyperbolic transformation of Piaget's "glimpse of science" into the biographer's "very firm grounding"). Another reports that "Piaget was ... grateful that his early scientific experience had shored him up against the seductive lures of philosophy" (Gardner, 1974, p. 53). Their pages, unawarely and unacknowledgly, smuggle Piaget's assumptions about the nature of science and philosophy.

In the second place, all divulgators basically repeat Piaget's version of his access to psychology after his encounter with meta-

physics: as an "equilibration" moved exclusively by epistemological concerns. A naïve rendering is given by two well-known American popularizers, who tell that Piaget "was convinced that the philosophical approach was too speculative, and that the scientific approach was too factual. What was needed was a linkage between the two" (Ginsburg and Opper, 1969, p. x). Already during his "pre-psychological" period, Piaget was motivated to solve the problem of the articulation of valid norms and empirical facts. In the course of his development, he became less interested in how such articulation could give rise to moral obligation, and more concerned with how it could give rise to logical necessity. Yet, at the same time that is has been clearly indicated that the relation norm-fact remained the core of Piaget's *problématique* (Cellérier, 1973) Piaget's non-scientific or non-philosophical motivations are consistently obliterated from the record. Most authors purify even Piaget's purified account, and are firmly convinced of a belief that clarifies things for them: "that during adolescence Piaget concentrated on two major intellectual pursuits: biology and epistemology" (Ginsberg and Opper, 1969, p. 3). Their pages uncritically contribute to transmit Piaget's rationalistic view of human development.

In general, Piaget's early work in natural history is seen as providing both the themes and the scientific basis of his later work. One of the most rotund statements of such view is made by two of the most important American popularizers of Piagetian theory, Elkind and Flavell, who affirm (1969, p. x): "Piaget's concepts were born in biology and were nurtured by logic". As the theory, the biography asserts continuity between biology and knowledge: "La zoologie à travers le problème des adaptations", writes one author on Piaget, " ... semblait devoir être l'orientation définitive de sa vie" (Lerbet, 1970, p. 13). (The first chapter of this author's book is called "La vie d'un non-philosophe"). Others argue that Piaget's "biological interests" should be considered carefully, "for they provide the thrust of Piaget's theory, as well as the foundations for a learning-theory approach to genetic epistemology"

Gallagher and Reid, 1981 p. 15). (The section from which we are citing is entitled "Piaget, the Psychological Epistemologist with Biological Roots"). The origins of Piaget's concern with adaptation are moved back to his earliest classification papers: "cette notion d'adaptation", we are told as if the same word always stood for the same concept, "jouera un rôle souverain dans la pensée piagetienne" (Nicolas, 1976, p. 5). A commentator says that Piaget's "biological studies" had by 1919 suggested the answer to his questions about the relations between biology and knowledge (Boden, 1979, p. 14).

As the above citations show, many authors reproduce Piaget's version of hiw own development with additions of grandiloquence, emphasis, and verbosity. Such stylistic traits betray their enthusiasm for the Piagetian interpretation of Piaget's theory, and their happy faith in its explanatory and illuminating power. In addition, at least one author has inflated Piaget's account, not only romanticizing it, but also distorting it on both factual and conceptual points (Gardner, 1974, p. 59):

> Piaget began his life work as a biologist, and he remains deeply committed to the study of organic life. Like others of his time, he was deeply influenced by Darwinian evolutionary theory, and in fact came to believe that processes and states should be understood in terms of their development. An early experiment convinced Piaget, however, that Darwin's account of natural selection was too simple.

By asserting the life-long continuity of Piaget's biological interest and scientific identity, and by ascribing his central concepts and points of view to scientific experience, the authors we have considered constantly reinforce the myth of a purely rational and scientific Piaget whose desires, motivations, and reasonings are never understandable or questionable from a viewpoint other than that of his own "scientific" genetic epistemology. They thereby construct the heroic and archetypical epistemic subject.

Autobiography and Myth

The mythification of an intellectual figure into the unique dimension of his theory provides a reflective confirmation of the vali-

dity of his ideas. The rhetorical function entailed by this operation is many times greater than its logical faults. Let us just point out some feature of mythification. First, it stops questioning, or at least channels it, by placing the figure above suspicion. Second, it assures him, as well as the public, that the only acceptable explanation of his behavior will take place within the space of problems he defined, thus leaving no room for ambushes, rearguard attacks, or surprise effects in the battle for social recognition in the scientific city. Third, by the very circularity of life and ideas, it assures the naturalness, and thus the legitimacy and inevitability, of those ideas and of that life. If ideas are in full continuity with life, then they must be real and true, and if life is in continuity with ideas, then its logic becomes directly readable from the great Book of Nature - provided, of course, that one is armed with the proper set of ideas.

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LE POINT DE VUE CRITIQUE DES ECRITS PSYCHO-SOCIOLOGIQUES (1889-1905) de AUGUSTIN HAMON*

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Abstract.
Hamon's writings during the period 1889-1905 are examined both for the novelty of their social psychological contents - unknown within the discipline - and as a way of critically evaluating the current historiography of the discipline. Among the emerging varieties of French social psychological discourse at the turn of the century,

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Hamon's writings, especially his two "Studies of Social Psychology" (1894, 1895a), emphasized systematic, empirical research and situated the "problematique" of social psychology at the interface of the individual and societal levels of analysis. Hamon's formulations of social psychology and social pathology differed from the analyses of his contemporaries (eg. Tarde and Le Bon) in that they linked a strong commitment to social movements expressing anarchist-communist ideas with a critical reevaluation of concepts in the social sciences, criminology, etc.; that is to say, Hamon conceived of the social sciences, *sui generis*, as *critical* sciences.

C'est en tant que psycho-sociologues que nous nous sommes avant tout intéressés à l'oeuvre sociologique et psycho-sociale (1889-1905) de A. Hamon (1862-1945). Presque contemporain de Tarde et de Le Bon, it publiait, à la fin du siècle dernier, en particulier la *Psychologie du Militaire Professionnel* et la *Psychologie de l'Anarchiste-Socialiste* (Hamon, 1894, 1895a) dans une série intitulée "Etudes de Psychologie sociale". Puisant la matière de sa réflexion psycho-sociologique dans l'observation de la réalité sociale environnante, dans les mouvements sociaux qui ébranlent son époque et dans les "questions sociales" dont on réclame avec insistance la solution scientifique (Apfelbaum, 1981), Hamon partage à bien des égards les préoccupations de Tarde out Le Bon. Chacun à sa manière a traité des désordres sociaux, sans pour autant s'accorder sur les causes de la criminalité ni même sur ce qu'il convient de désigner sous cette étiquette (cf par exemple, Hamon 1893, a).

De fait, le point de vue théorique de Hamon s'oppose radicalement à celui des deux autres psycho-sociologues. Cependant malgré son originalité, la diffusion de sa pensée psycho-sociale a été limitée de son vivant déjà et, de nos jours, ses écrits sont totalement inconnus même des historiens de la discipline (cf. Apfelbaum & Lubek, 1982, pp. 35-36). Au dela du cas singulier de Hamon, l'existence de théories oubliées, dont le développement a été en quelque sorte prématurément interrompu, pose un double problème: celui du statut à accorder à ces théories dans l'histoire de la

discipline considérée et celui, plus général, de notre pratique en tant qu'historien des sciences.

Si l'on admet que l'oubli d'un courant de pensée n'est pas nécessairement un effet de sa médiocrité, il devient indispensable de résoudre la question préalable du pourquoi et du comment de cette mise à l'écart. Dans cette perspective, les rapports de pouvoir dans lesquels s'inscrive en particulier toute relation d'auteur/éditeur sont déterminants: l'éditeur ou le comité de rédaction a en effect les moyens de dresser un barrage plus ou moins infranchissable qui maintient sur la touche des manuscrits qui expriment par example des divergences d'ordre théorique et/ou politique. Il exerce par conséquent un contrôle efficace, même s'il n'est que partiel, sur la dissémination des idées ou des théories dont le rayonnement n'est donc pas exclusivement un effet de sa valeur heuristique intrinsèque. L'analyse du sort des thèses psycho-sociales de Hamon, traitée ailleurs de façon plus détaillée (Lubek et Apfelbaum, 1982) corrobore les travaux effectués sur le même sujet concernant Garcia (Lubek et Apfelbaum, 1979) ou Tarde (Lubek, 1981) et souligne l'importance d'une "psychologie sociale de la science"; celle-ci constitue, à notre avis, un des volets indispensables à l'histoire des sciences en démontant les mécanismes proprement psycho-sociaux qui participent à la destinée des idées et des théories nouvelles.

Quant à la méconnaissance de certains courants de pensée dans l'histoire de la psychologie sociale ou des sciences sociales, tout en étant lié au point précédent, elle renvoie en outre plus spécifiquement aux fonctions que l'histoire d'une discipline est amenée à jouer dans son évolution même. Ainsi, ce qui tient lieu en ce moment d'histoire à la psychologie sociale (cf. Allport, 1968) s'attache avant tout à légitimer le statut scientifique actuel de la discipline et, dans ce but, met en avant son enracinement dans le courant positiviste, fait principalement état des réalisations expérimentales de la discipline, même si c'est au prix d'un mythe d'origine (Samelson, 1974) ou de l'exclusion de courants importants pour qui veut conserver l'intégrité de la psychologie sociale.

En tout état de cause, on constate qu'il n'existe pas encore à ce jour, du moins pour la psychologie sociale, de travail historique qui ait tenté véritablement de retracer les étapes du développement de la discipline dans ses rapports avec l'histoire générale des idées et de mouvements politiques. Or, selon nous, une véritable histoire de la discipline suppose que l'on prenne en compte, non seulement le développement interne des idées, mais aussi les événements socio-historiques dans lesquels ces idées s'enracinent; de même qu'il convient d'analyser comment les institutions et la communauté scientifique dans lesquelles ces théories voient le jour, en façonne la nature à chaque période considérée. En d'autres termes, l'histoire d'une discipline consiste à remettre dans leur contexte propre chacun des courant théoriques afin d'en mieux saisir la centralité et ensuite les fluctuations dans le temps et, inversement, afin de prendre une plus juste mesure de la portée et des limites de ces théories. Ce dernier point revient à souligner la *fonction critique* et heuristique que toute historiographie devrait, à notre sens, exercer pour la discipline considérée. L'examen du cas particulier de Augustin Hamon peut être considérée comme une illustration partielle de ce projet plus global.

L'eclectisme de Auguste Hamon

Hamon se situe dans une tradition de penseurs plubliciste mais s'en démarque par une souci constant d'objectivité et de scientificité - "Je suis un scientiste" répète-t-il. Cela signifie en particulier qu'il s'emploie à *démontrer* ses thèses au lieu de se contenter de les affirmer: non pas que ses analyses soient moin marquées par ses options politiques que celles des autres penseurs sociaux de son époque, mais il s'attache davantage qu'eux à les fonder de manière systèmatique et méthodique à partir de faits et de données empiriques. Il publie des livres, collabore à diverses revues (notamment aux *Archives d'Anthropologie Criminelle, de Criminologie et de Psychologie Normale et Pathologique*[1]; à *Société Nouvelle* mais aussi à la *Revue de bibliographie médicale* ... et fonde la

revue *Humanité Nouvelle* qu'il dirigera de 1897 à 1903 (Lubek et Apfelbaum, 1982).

Il n'est cependant pas seulement "homme de cabinet"; il est aussi homme d'action. Son engagement politique le situe aux côtés des anarchistes-communistes (dans la lignée de P. Kropotkine ou E. Reclus) et des socialistes[2], ce qui lui valut plus d'un démêlé avec la police. Dans *Les Hommes d'Aujourd'hui* (1896), Whirlily note: "J'écris cette biographie spécialement pour les policiers (qui ont pris) l'habitude d'aller embêter la concierge d'Hamon chaque fois que celui-ci lance un livre nouveau..." Les préoccupations scientifiques de Hamon qui l'ont conduit jusqu'en 1889 à s'intéresser aux "applications de la science au confort des hommes" devaient, selon Whirlily, fatalement l'entraîner à s'occuper de sociologie (cf. Hamon, 1889). Il écrit en effet à partir de 1889 plusiers ouvrages d'abord en collaboration (Hamon en Bachot, 1889; 1890; 1891) puis seul (Hamon, 1893b) tout en décrivant la physionomie de la France contemporaine, il y dénonce les abus de pouvoir du monde politique et financier et en analyse les mécanismes. *"Sociologie et Hygiène"* (Hamon, 1889) amorce ce que l'ont peut appeler la "période sciences sociologiques" de l'itinéraire professionnel de Hamon; il s'achèvera brusquement en 1905 quand, acculé par de multiples problèmes financiers, il accepte l'offre de George Bernard Shaw et devient le traducteur de ses oeuvres (Lubek et Apfelbaum, 1982).

La psychologie sociale de Hamon: mentalités et determinations sociales

En november 1893 paraissait la *Psychologie du Militaire Professionnel* (Hamon, 1894) et en juillet 1895, la *Psychologie de l'Anarchiste-Socialiste* (Hamon, 1895a).
Ces deux ouvrages inauguraient une série intitulée "Etudes de Psychologie sociale" qui n'aura pas de suite bien que Hamon ait prévu plusieurs autres volumes; il avait d'ailleurs entrepris de réunir les données destinées à une troisième étude sur la psychologie des artistes et des savants. Nous y reviendrons.

Examinons tout d'abord l'objet de ces études. Un même projet les sous-tend qui vise à mettre en évidence la double détermination du comportement des individus, l'articulation entre les prédispositions héréditaires et congénitales d'une part et les facteurs sociaux et plus généralement mésologiques (sociaux, climatiques ou familiaux, en bref, les cadres de la vie), d'autre part. C'est déja sur l'importance de ces derniers que Hamon insistait dans l'essai qu'il publiait l'année précédent (Hamon, 1892), sur l'anarchiste Ravachol, son analyse s'écartant résolument des théories courantes du criminel né, même si Corre (1893), commentant le texte de Hamon dans les colonnes des *Archives*, le rejoint sur certains points.

D'une manière plus générale, l'incidence des déterminants sociaux est manifeste quand on considère la mentalité professionnelle (qui exprime la manière dont une profession modèle le comportement des invididus qui l'exercent) out la mentalité philosophique (ce que l'on désignerait aujourd'hui par politique ou idéologique). Car, selon Hamon, on peut parler de mentalité professionnelle ou philosophique au même titre que Le Bon parle de mentalité nationale. Mair leurs points de vue sont totalement étrangers, voire opposés l'un à l'autre. Le Bon, en effet, soutient que la mentalité nationale - c'est à dire la similitude des caractères psychiques d'individus d'une même nationalité - provient "de l'hérédité de leur race", donc d'un archétype transhistorique transmissible et immuable. Or "nous pensons que cela (la mentalité) est surtout dû aux influences climatériques, telluriques, sociales qui sont similaires pour tous les individues d'une même nationalité". (Hamon, 1895a, p. 31). Tandis que Le Bon se rattache à une tradition de pensée qui, dans cette seconde moitié du 19ème siècle, ancre une large partie des théories de l'homme dans une conception des races, et de l'hérédité incontournable, Hamon s'en distinque en refusant précisément tout déterminisme *bio-historique* au profit des facteurs sociaux plus labiles. Les mentalités sont donc modifiables et malléables et non plus héréditairement fixes et inaltérables.

L'institutions militaire apparaît comme un des exemples illustrant comment les facteurs sociaux façonnent et uniformisent la

"mentalité" des individues. Pour le démontrer dans la *"Psychologie du Miliaire Professionnel"*, Hamon s'appuie sur des extraits de presse et sur des descriptions qu'il trouve dans les multiples ouvrages qui décrivent les milieux militaires. A partir d'un collage de textes (sorte d'analyse de contenue sommaire), il décrit les méthodes disciplinaires comme l'un des modalités aboutissant à faire des soldats "une armée d'esclaves" (p. 99), expose les mécanismes qui assurent et perpétuent "la non-révolte de la généralité des victimes de ces abus" (p. 100). Parallèlement il analyse les effets néfastes, inévitables et irréversibles qui accompagnent l'usage prolongé de l'autorité, et favorisent les crimes et les exactions dont se rendent coupables les officiers; en bref, l'institution militaire fabrique des criminels. Dals la logique de cette thèse, il n'est guère suprenant que Hamon ait été résolument anti-dreyfusard tout au long de l'affaire Dreyfus qui éclate en 1894.

En bref, et pour employer un language moderne, on dira qu'il a traité dans la *Psychologie Militaire Professionnel* des effets de pouvoir dont la légitimité est institutionnellement fondée. Son étude des effets de l'establishment militaire évoque les analyses des interactionnistes des années 1960 et particulièrement celles de Goffman concernant les effects de dépersonnalisation dans les institutions totales. Mais quand Hamon parle des marques distinctives (des officiers notamment) qui, en leur donnant un statut particulier dans la société, sert de justificiation à la perpétration d'actes répréhensibles - et criminels -, on pense aussi aux questions posées par Zimbardo sur les conséquences de l'anonymat et de la désindividualisation. C'est dire la permanence des problèmes abordés par Hamon qui font aujourd'hui encore partie intégrante du champ de la psychologie sociale contemporaine.

Parce qu'il s'attaque à l'institution militaire, le livre de Hamon a fait scandale avant même sa parution; non seulement il avait été refusé successivement par sept éditeurs, mais quand il sera enfin sur le point de paraître, il sera menacé - ainsi que son auteur - de poursuites si bien que le dépositaire parisien refusera

d'assurer la diffusion (Lubek et Apfelbaum, 1982). Pourtant, l'existence de la criminalité militaire est alors un fait bien connu: Corre (1891), prenant appui sur des statistiques officielles en avait déjà étudié l'évolution et avait souligné la nécessité de distinquer entre le crime-délit militaire proprement dit et le crime ordinaire qui range le militaire coupable dans la même catégorie que les autres criminels. Par ailleurs, l'influence des professions sur la criminalité était elle aussi reconnue: Coutagne (1892) a été le premier dit-il, (mais pour la vérité historique il faut signaler que Quételet (1893)[3] en parla avant lui) à présenter un mémoire sur ce sujet au Congrès d'Anthropologie Criminelle à Paris en 1889. Selon lui, à toute profession s'attachent inévitablement certains crimes, délits ou fraudes spécifiques de sorte que l'on peur parler de psychologie professionnelle; celui-ci se traduit par des conduits spécifiques qui se transmettent par une véritable "contagion professionnelle". Mais le préoccupation de Coutagne, médecin légiste, est moin d'expliquer que de prévenir: son rapport de 1892 au Congrès de Bruxelles aboutit à des recommandations en faveur du développement d'associations professionnelles qui, en imposant un contrôle, limiteraient la propagation de ce type de criminalité. Hamon, en revanche, cherche avant tout à rendre compte des causes mêmes de tels phénomènes; l'explication qu'il propose du mode de fonctionnement de l'institution militaire aboutit implicitement à une dénonciation et, au-delà, à la mise en cause des choix de la société qui la finance. C'est dire que l'essai de Hamon, inspiré par d'indiscutables sentiment antimilitaristes, inaugure une véritable *psychologie sociale critique*. Cela explique sans doute pour une part le tollé que provoqua la publication de la *Psychologie du Militaire Professionnel* mais qui en a, par ailleurs, assuré le succès commercial puisque le livre se vendit à plus de 9.000 exemplaires.

La *Psychologie de l'Anarchiste-Socialiste* ne rencontrera pas le même succès et sera très froidement accueilli. Cette étude doit pourtant être considérée, sur le plan théorique, comme la contrepartie de la précédente. En effet, une fois que l'on a montré que

les cadres professionnel, "philosophique" (traduisons: idéologique) ou culturel produisent une certaine uniformité et/ou une pathologie au niveau des conduites individuelles it reste encore à s'interroger en amont sur les raisons qui conduisent les individus à suivre une voie, à embrasser telle profession de préférence à une autre. Avant même que les cadres sociaux ou idéologiques ne transforment et ne modèlent les individus, peut-on trouver entre eux des similitudes, un dénominateur commun à tous ceux qui sont animés par un même élan, une même mentalité philosophique? Tel est le propos de la *Psychologie de l'Anarchiste-Socialiste* et Hamon s'efforce de déterminer une configuration de traits propres au groupe des anarchistes, une communaité de caractères qui les prédisposeraient à se rallier à ces doctrines et pratiques. Bien entendu, l'objet d'étude n'est pas quelconque. Il évoque une actualité brûlante, celle des "années sanglantes" du mouvement anarchiste dont les activités en s'intensifiant rencontrent une répression elle aussi accrue (cf. les lois scélérates destinées à réprimer l'agitation syndicale et anarchiste). Ce n'est évidemment pas un hasard si Hamon dont on connait les affinités politiques s'intéresse aux anarchistes tandis que, de leur côté, Tarde et Le Bon traitent simultanément des foules pour en dénoncer les méfaits.

Pour établir le portrait de l'anarchiste, Hamon ne se contente plus de faire appel à des récits; il amorce une véritable enquête adressant directement une questionnaire(4) aux anarchistes qu'il connait et le diffusant plus largement dans plusiers pays par l'intermédiaire de publications anarchistes. Il obtient 68 réponses qu'il dépouille systématiquement (cf. archives privées) dégageant une série de traits communs à la majorité des sujets de son échantillon.

Il s'agit d'établir "le type idéal, moyen de l'Anarchiste-Socialiste, de même que le naturaliste établit le type idéal, moyen, de l'homme ou d'une espèce animale quelconque. (Hamon, 1895a, p. X) en recourant à la "méthode positive". En passant, on remarquera que cette préoccupation relative au "type moyen" s'inscrit dans une autre des préoccupations dominantes de l'époque,

celle de l'établissement d'une psychologie différentielle, à la Quételet ou Galton. Mais à propos de l'ouvrage de Hamon, on pense également à l'intérêt que suscitaient à l'époque les biographies des grands hommes (cf. par example l'ouvrage de Joly, 1883, sur la *Psychologie des Grands Hommes* ou celui de Toulouse, 1896, sur Emile Zola).

Les intitulés des chapitres de la *Psychologie de l'Anarchiste-Socialiste* correspondent à l'énumération des traits qui caractérisent les anarchistes, à partir de leurs propres réponses: esprit de révolte, amour de la liberté, individualisme, altruisme, sentiment de justice, sens de la logique, curiosité de connaître, esprit de prosélytisme. Sur le plan méthodologique, l'essai s'apparenterait aujourd'hui à une pré-enquête. Pour l'auteur, il s'agit par cette démarche de faire oeuvre d'homme de science; de maintenir donc une certaine "sérénité" qui est le propre du "scientiste" (Hamon, 1895a, p. IX) occupé à rechercher la vérité "sans se soucier des inconvénients ou des avantages qui en peuvent résulter pour soi, pour les siens, son pays, la société" (ibid, p. VIII). Cette insistance n'est sans doute pas vaine quand on sait que Hamon achève son livre alors que la répression pour toute propagande anarchiste s'est encore intensifiée. Mais l'auteur vise à autre chose qu'une défense de son livre ou une pure déclaration d'intentions: pour garantir l' "impartialité" et l' "impassibilité" (ibid. p. IX), il prend appui sur des données empiriques, démarche inusitée à cette épogue. S'il emprunte l'idée d'un questionnaire au discipline de Lacassagne, le Dr. St. Paul, son souci de rigueur, d'échantillonnage et de contrôle de certaines variables ne revient qu'à lui.

De fait, dans la version publiée, le livre est largement tronqué en regard du projet initial, beaucoup plus ambitieux. Le plan primitief de la *Psychologie de l'Anarchiste*[5], tel qu'il avait été accepté par P.V. Stock qui l'édite, comprenait trois sections: outre celle que nous connaissons, une deuxième partie devait être consacrée à la psychologie des "propagandistes par la

violence" qui déconcent la société par l'action directe et sont de ce fait même assimilés par elle à des criminels. Une troisième partie devait être consacrée à une "comparaison de l'état psychique des premiers chrétiens et des anarchistes". Il s'agissait, dans cette dernière de montrer les analogies qui existent selon Hamon entre les doctrines des anarchistes et celle qui prêchait Jésus; de montrer en outre par la "méthode positive", en s'appuyant sur les histoires des martyrs, les similitudes des adeptes de deux doctrines (cf. notes non datées des archives privées de la famille Hamon). Mais bientôt "cette étude ébauchée devient trop importante pour former une deuxième partie du livre précédent. A elle seule elle forme un volume care elle comprend, par la juxtaposition des textes et leurs commentaires, la preuve de l'identité des doctrines de Jésus et les anarchistes communistes(6), la preuve que les pères de l'Eglise ont adopté cette manière de voir. Une autre partie montrerait à l'aide des actes des martyrs que les premiers chrétiens avaient le même état d'esprit que les anarchistes actuels. Vous voyez, cela fait un volume qu'il ne faut pas désorganiser en le résumant comme deuxième partie de la Psycho..." (Lettre de Hamon à P.V. Stock, 10 Août, 1894). Ce livre ne verra jamais le jour; seul un des ses chapitres sera publié l'année suivante (Hamon, 1895b).

Quant à la partie consacrée aux anarchistes criminels, elle sera abandonnée pour d'autres raisons. Rappelons qu'au moment où Hamon réunit les éléments de son livre, l'assassinat de Carnot, en Juin 1894, par l'anarchiste Caserio intensifie les poursuites contre ceux qui défendent les anarchistes ou propagent leurs doctrines; les lois scélérates sons à cette époque appliquées avec un zèle accru. Avec d'autres anarchistes, Hamon s'exile en Angleterre où il termine la *Psychologie de l'Anarchiste*. Devant l'inquiétude de son éditeur, Hamon écrit: "Ce livre ne tombe point sous "la lois scélérate" car il n'a pas pour but un acte de propagande anarchiste. Dans la préface ce sera catégoriquement affirmé. C'est un livre de science, rien qu'un livre de science. Etant donné l'article 2 de la dite loi, *je ne veux pas* établir la psychologie des propagandistes par la violence. En effet, cette étude impar-

tiale, documentée et purement scientifique pourrait en certains points paraître à des esprits mal intentionés comme une apologie - cela ne serait pas mais cela passerait pour l'être -. Donc je réserve pour plus tard, dans un an au plus, l'étude de l'état d'âme des Ravachol, Vaillant, Henry, etc.; cela fera partie d'une criminologie politique que je prépare". (Lettre déjà citée). Plus loin, il conseille cependant à Stock de banaliser le livre au maximum: "Maintenant à mon avis il serait préférable que vous abandonniez la couverture rouge et la série pour en recommencer une autre avec couverture sévère: verte ou bleue; et avec la rubrique générale: Bibliothèque de sociologie. Cela indiquerait bien le but purement scientifique des livres" (ibid.). Cela résout le mystère de la disparation brusque de la série "Etudes de Psychologie sociale" main on ne peut s'empêcher de souligner l'étrange destin de cette psychologie sociale qui apparaît et disparaît au gré des vents politiques (cf. Apfelbaum, 1978).

La *Psychologie de l'Anarchiste-Socialiste* paraît donc mais l'accueil est extrêmement réservé; il n'en sera pas fait mention dans les colonnes des *Archives* bien que l'ouvrage soit dédicacé à son directeur, Lacassagne, chef de file de l'école criminologique de Lyon. Il est vrai gue l'anathème frappe alors tout ce qui relève de près ou de loin de l'anarchisme; il s'exprime dans des lettres de Lacassagne, de Tarde (co-directeur des *Archives*) et de Storck, éditeur de la revue. Leur rôle inhibiteur dans la diffusion des "Etudes de Psychologie sociale" a été discuté plus longuement ailleurs (Lubek & Apfelbaum, 1982).

La fin de la "Periode sciences sociologiques".

Cela n'empêchera cependant pas Hamon de tenter de poursuivre son entreprise initiale, ni même d'être sollicité pour d'autres projets dans le domaine des sciences sociales. S'il les abandonne bientôt sans les mener à leur terme pour devenir le traducteur exclusif des oeuvres de Bernard Shaw, on peut en attribuer en partie la cause à divers déboires financiers (Lubek et Apfelbaum, 1982). Mais il est vrai aussi que les démarches qu'il a tentées

pour obtenir un renseignement, notamment aux Etats-Unis, n'ont jamais abouti.

Parmi ses projets déjà anciens, il entreprend avec Réné Ghil, le poête, celui de "déterminer 'état psychique et comparé des artistes et des scientistes" qui prolonge directement les deux précédentes "Etudes de Psychologie sociale". Il s'en explique d'ailleurs lui-même: "Dans la *Psychologie du Militaire Professionnel* j'ai étudié l'influence d'une profession sur ceux qui l'exercent. Dans la *Psychologie de l'Anarchiste-Socialiste j'ai montré la mentalité spécifique aux individus ayant une doctrine philosophique déterminée. Dans la Psychologie de l'artiste et du scientiste*, M. Ghil et moi nous avons l'intention de montrer les caractères mentaux nécessaires à un être humain pour être artiste ou scientiste. Nous voulons déterminer les modes de perception, de conception et d'action qui font que tels ou tels individus sont plutôt musiciens que peintres, plutôt sculpteurs que poètes, plutôt romanciers que biologistes" (A. Hamon. Une Enquête. Manuscrit inédit, 1896; archives privées). A cet effet Hamon a diffusé un questionnaire - voir Annexe 1 - basé sur celui que le Dr. St. Paul avait utilisé pour une étude sur la parole intérieure; mais le projet n'aboutira pas faute de réponses (une trentaine seulement). La responsabilité en incombe-t-elle aux articles critiques parus dans la presse dont un de la plume de Octave Mirbeau? Ce romancier fut l'un des anarchistes qui avait précédemment lui-même répondu au questionnaire sur l'anarchisme, et qui publie en juin 1896 un article qui tourne en dérision l'enterprise de Hamon et Ghil (Mirbeau, 1896; Hamon, Une Enquête, 1896).

A cette même époque il est par ailleurs sollicité par l'Université Nouvelle de Bruxelles qui accueille fréquemment socialistes et anarchistes. En 1896 en 1897 son cours sur la criminologie est proche de l'optique professée par E. Ferri, un socialiste italien qui enseigna lui aussi à l'Université Nouvelle. A défaut de faire cette criminologie politique qu'il annonçait à son éditeur Stock, (voir plus haut), il développe ses idées et ses théories sur la criminologie dans cet enseignement dont une partie

sera publiée dans *Déterminisme et Responsabilité* (Hamon, 1898; traduction anglaise, Hamon, 1899).

Cet ouvrage qui inaugure chez Schleicher une nouvelle collection (Bibliothéque internationale des sciences sociologiques; cf. Lubek & Apfelbaum, 1982) dirigée par Hamon, fait apparaître la ligne directrice qui unifient les différents travaux de l'auteur dans le champ de la psycho-sociologie en les situant dans une réflexion plus large sur la criminologie, un des thêmes clés de l'époque; de même que l'on saisit mieux comment ses thèses l'opposent à nombre de ses contemporains qui traitent des sciences sociales en général ou de la psychochologie sociale en particulier. Cette divergence rend-elle compte du silence qui s'est fait autour de Hamon dès qu'il a cessé de participer activement à ce domaine?

Il faut se garder de souscrire trop vite à une thèse qui ramenerait l'éclipse subie par Hamon à de simples considérations politiques: alors qu'on aurait pu tout aussi valablement solliciter Tarde, de Roberty, Duprât, de la Grasserie, Baldwin et d'autres encore, c'est à Hamon que Vaschide, en tant que secrétaire de la collection dirigée par le Dr. Toulouse, demande en 1900 d'écrire un traité de psychologie sociale pour la "Bibliothèque internationale de Psychologie expérimentale, normale et pathologique". Or, le Dr. Toulouse étant Directeur du Laboratoire de psychologie expérimentale à l'Ecole des Hautes Etudes, et Médecin en chef de l'Asile de Villejuif, cela témoigne au moins que son autorité en la matière est reconnue et prime sur l'inquiétude que ses positions politiques pourraient susciter auprès de certains. Après avoir accepté le contrat, Hamon diffère d'année en année la réalisation de ce traité malgré l'intérêt qu'il semble y porter et les multiples rappels à l'ordre de Vaschide. Leur échange de correspondance fait état de l'incertitude de Hamon quant à l'objet précis d'un tel ouvrage. "Il faut (donc) que vous m'exposiez bien le plan de cet ouvrage qui peut être conçu de manière différente car la Psychologie sociale n'est pas bien déterminée: (Lettre à Vaschide, 3 Août, 1903). Il est vrai qu'à cette époque les domaines abordés sous cette étiquette étaient souvent fort hétérogènes (cf. Apfelbaum, 1981) comme si

le terme "social", dont Hauser (1903) disait que c'était la "tarte à la crème", se retrouvait accolé à celui de psychologie sans raison théorique précise (comme s'est le cas de l'ouvrage posthume de Chasles, 1875).

A dire vrai, Hamon avait quelques idées quant à la façon de traiter du sujet comme en témoigne le plan très sommaire retrouvé dans ses archives (cf. Annexe 2). On y retrouve comme thème organisateur ce double versant qu'il s'est efforcé en permanence de mettre en évidence: d'une part celui de l'influence de la société, de la collectivité mais aussi du groupe restreint sur les individus et, d'autre part, celui de l'influence de l'individu sur la société où le travail qu'il a amorcé sur les génies, les criminels politiques, etc. trouve naturellement sa place. En définitive, ce livre ne verra jamais le jour. En 1905, Hamon écrit à Vaschide qu'il ne peut honorer son contrat qui le lie à Doin dans les délais prévus. Cette même année paraîtra, sans grand succès, *Socialisme et Anarchisme* (Hamon, 1905) dernier ouvrage de cette période consacrée aux sciences sociologiques.

La portée des écrits psycho-sociologiques de Hamon

Hamon tout comme Binet, Le Bon ou Tarde a été le témoin oculaire des révoltes et des luttes sociale qui traversent la societé française et servent de toile de fond à la psychologie sociale qui s'élabore en cette fin de siècle. Les thèmes abordés en sont le reflet direct: que l'on traite des "questions sociales" (cf. par example, Coste, 1886), des foules ou de la criminalité, il s'agit toujours de comprendre les fonctionnements et les dysfonctionennements de la société contemporaine afin de la mieux gérer et/ou de la transformer. Dans l'avant-propos de présentation de la nouvelle collection "Bibliothèque internationale des sciences sociologiques" qu'il dirige chex Schleicher (1898-1901) Hamon écrit: "Cette universalisation des sciences sociologiques mettra fin au désordre social qui affecte toutes nos formes actuelles de société et permettra la réalisation de ce mieux être que chacun appelle de tous ces désirs" (Hamon, 1898). Thème que l'on retrouve comme leit-motiv

chez ceux qui préconisent le développement d'un discours du social (Apfelbaum, 1978).

Comprendre la genèse des désordres sociaux et de la criminalité demeurera une des préoccupations majeures de Hamon, tout comme elle l'a été pour Tarde; s'ils se trouvent sur un terrain commun, leurs points de vue sont bien divergents à commencer par l'objet d'investigation même. Révéler la criminalité des cadres de l'armée, la "criminalité sociale" est occulte du fait qu'elle ne relève pas de la législation, ou encore étudier les mobiles des anarchistes en récusant ainsi le caractère scientifique et opératoire d'une assimilation trompeuse et réductrice entre crime et anarchie, ces thèmes qui sont au centre de la pensée de Hamon restent étrangers a celle de Tarde par exemple (Lubek & Apfelbaum, 1982).

En revanche, ce n'est pas un hasard si le thème des foules ne trouve guère d'écho dans la problématique de Hamon - et ce terme est d'ailleurs peu usité dans les écrits socialisants - alors qu'il est abondamment développé dans celle de Tarde (1892) ou Le Bon (1895) en rapport étroit avec une analyse des désordres sociaux (Cochart, 1982). A les lire, il devient évident que la notion de foule est utilisée comme un euphémisme pour désigner les révoltes et que les connotations négatives qui y sont en permanence attachées annoncent la théorie qui en est proposée; en d'autres termes, la théorie est *a priori* inscrite dans la désignation de l'objet d'étude, les foules. Pour Tarde (1892) comme pour Le Bon (1895), la foule est l'occasion pour les hommes - qui, pris individuellement, peuvent se montrer policés, domestiqués - de retrouver leurs instincts primitifs et un comportement de horde, de redevenir sauvages en somme. D'ailleurs en assimilant le comportement de la foule à celui d'un sujet sous hypnose (et ils acceptaient l'idée à la suite de Charcot[7] que l'hypnose est liée à un désordre mental), on affirme le caractère essentiellement pathologique de la foule considérée comme une entité et on contourne le problème préalable et critique des circonstances de constitution d'une foule.

S'opposant à cette vision du désordre social comme expression d'une pathologie, d'un mal destructuer dont la faute incomberait aux individus réunis en masses informes, Hamon suggère dans ses écrits que la racine du mal n'est pas à rechercher avant tout dans l'individu mais tout autant dans la société elle-même. Même si l'on reconnaît l'existence d'un facteur d'hérédité, il est modulé par les effets des facteurs mésologiques: la profession engendre de la criminalité tandis qu'à l'inverse certains actes jugés par la loi comme criminels peuvent être inspirés par divers sentiments de justice notamment (cf. *Psychologie de l'Anarchiste-Socialiste)*. De ce point de vue, l'analogie évoquée entre les premiers chrétiens et les anarchistes (Hamon, 1895b) permet à la fois d'établir le bien fondé de certaines révoltes, d'en indiquer le caractère constructif et de montrer le rôle incitateur joué par la société - le comportement révolté des martyrs s'expliquant par example par la répression dont ils furent l'objet (Hamon, notes inédites, non datées, sur la "comparaison des premiers chrétiens et de l'état psychique des anarchistes, archives privées).

A certains égards, la divergence qui existe entre Hamon et nombre de des contemporains se retrouve ultérieurement à diverses étapes du développement de la psychologie sociale, quand se sont opposées, notamment dans les théories du conflit, celles qui ont admis sans plus ample questionnement son caractère nocif et destructeur à celles qui, au contraire, l'envisageaient sous son angle constructeur, voire libérateur. Il est incontestable que les premières ont connu un vaste développement tandis que les secondes ont rencontré une résistance suffisamment grande pour en limiter l'expression (Apfelbaum, 1979; Apfelbaum & Lubek, 1976), la dissymétrie qui en a résulté contribuant à créer l'illusion que seules les premières sont de nature à enrichir le patrimoine des connaissances.

Pour en revenir à Hamon, il subirait en somme le sort commun réservé à ceux qui ont fait entendre une voix souvent discordante et minoritaire dans les milieux de la sociologie (Coser, 1956) ou

de psychologie sociale, en tentant de construire une théorie du social en rupture avec la normativité de l'objet social brut. Il s'agissait en effet pour Hamon de montrer que l'on ne peut se contenter, comme le faisaient souvent les théories criminologiques classiques de l'époque, d'adopter sans plus ample examen les catégories socialement et légalement admises pour définir au sens scientifique la criminalité. Transgressions, crimes et délits sont toujours définis à travers une norme historiquement et socialement marquée (souvent en outre dans une couche sociale déterminée), et la normativité qui se présente comme objective et univoque est elle-même un objet à étudier. Méconnaître Hamon, c'était donc marginaliser cette tentative de situer d'emblée la science sociale sur un plan critique et de la définir en rupture avec les pratiques sociales courantes.

Mais c'est méconnaître aussi l'importance d'une perspective qui prend en compte l'emprise de la collectivité et du groupe, ses répercussions au niveau du comportement individuel et inversement les effets de celui-ci sur ceux-là. Faut-il rappeler que l'insistance de Tarde sur l'importance de l'interpsychologie (Tarde, 1903) n'a pas non plus rencontré d'écho (Lubek, 1981)?

Après la mort de Tarde en 1904 et le tournant que prend Hamon à l'époque dans son itinéraire professionnel, se trouvaient relégués des modes d'appréhension des phénomènes sociaux qui mettront longtemps avant de refaire surface.

NOTES

(1) Pour des raisons de commodité, *Archives* désigne les *Archives de l'Anthropologie Criminelle et des Sciences Pénales* (1886-1892) qui devient à partir de 1893 les *Archives d'Anthropologie Criminelle, de Criminologie et de Psychologie Normale et Pathologique*.

(2) Au Congrès international du socialisme (2è Internationale) à Londres (Juillet 1896), Hamon participait à la délégation française qui prônait la réinsertion des anarchistes dans le mouvement socialiste international, (Hamon, 1897).

(3) Quételet (1839) consacre une partie du chapitre "Développement du penchant au crime" à envisager l'influence des lumières, des professions et du climat sur le penchant au crime" (p. 175-209).

(4) Outre des questions d'ordre démographique, le questionnaire se réduisait aux deux questions suivantes: A) "Pourquoi êtes-vous un anarchiste?"; b) "Comment l'êtes-vous devenu?"

(5) Les archives personnelles de Hamon ne font pas état dans le titre de l'ouvrage du terme socialiste qui semble bien avoir été ajouté par opportunisme pour atténuer le caractère explosif du titre et minimiser les risques de voir tomber l'ouvrage sous le coup des lois scélérates.

(6) Anarchistes communistes - au sens donné à ce terme à l'époque (voir Hamon, 1897).

(7) Il est évident dans le cas de Le Bon que sa référence est Charcot plutôt que le point de vue divergent de l'école de Nancy, et notamment de Bernheim.

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ANNEXE 1: Le questionnaire de Hamon & Ghil (1896)

132, avenue de Clichy, Paris.

MONSIEUR,

Il nous a paru intéressant de rechercher l'état psychique, essentiel et comparé, des artistes (peintres, sculpteurs, musiciens, poètes, romanciers), et des scientistes (naturalistes, biologistes, philosophes, sociologues, etc.).

Dans ce but, nous avons dressé le questionnaire suivant. Notre intention est d'utiliser les documents envoyés dans un ouvrage à paraître fragmentairement dans des revues de France et de l'étranger, et ensuite en librairie.

Persuadés que vous voudrez bien nous aider en cette œuvre scientifique, nous vous prions de nous favoriser de votre réponse — dans le délai le plus court qu'il vous sera possible.

Veuillez agréer, Monsieur, avec nos vifs remerciements, l'assurance de notre parfaite considération.

A. HAMON René GHIL

NOTA : Les réponses doivent être adressées à M. A. HAMON, *avenue de Clichy, 132, Paris.*

A. HAMON
3, Boulevard Berthier
PARIS

QUESTIONNAIRE

I

RENSEIGNEMENTS GÉNÉRAUX : Age, sexe, antécédents héréditaires au point de vue psycho-physiologique. — Religion. Race. Pays où l'on vit. Esprit sédentaire ou voyageur, etc...

Qualités des sens : Vue, ouïe, tact, couleur de vos cheveux et celle de vos yeux, etc.

APTITUDES GÉNÉRALES : Etes-vous observateur ? particulièrement d'un certain ordre de choses ? duquel ? — Avez-vous le goût des sciences philosophiques ? des sciences mathématiques ? biologiques ? Avez-vous des aptitudes générales pour l'une de ces sciences ? — L'instinct musical très développé, peu ou pas ? Aimez-vous les beaux-arts, la littérature ? — Etes-vous peintre, sculpteur, musicien, poète, romancier, homme de science ?

II

PERCEPTION

1° Etant donné que le monde extérieur nous impressionne diversement par nos divers sens, — par quel mode vibratoire (visuel, auditif, tactile, gustatif, olfactif) êtes-vous impressionné le plus ordinairement, le plus naturellement, comme inconsciemment ?

2° Une impression, venue *d'un sens*, éveille-t-elle en votre cerveau, comme inconsciemment, fatalement, une autre ou plusieurs autres impressions, d'autres modes de sentir ?

Les impressions visuelles, par exemple, se lient-elles immédiatement à des impressions auditives, etc... les couleurs se lient-elles à des sons, à des saveurs, à des parfums ?

3° Ou, sons, saveurs, etc., éveillent-ils *des images* ?

4° L'impression éveille-t-elle *une image objective ou subjective* ? — ou une idée de mouvements ? Ces mouvements sont-ils rythmiques ?

5° Percevez-vous synthétiquement, ou analytiquement, c'est-à-dire l'impression se produit-elle d'ensemble, ou de détail ?

6° Quel est le mode d'*expression mentale* de l'impression ? S'exprime-t-elle sous forme *mentale* d'images, de sons, couleurs, saveurs, parfums ?

Ou toute impression se métamorphose-t-elle, immédiatement et inconsciemment, *en concept*, et en son expression mentale, le *mot* ? Ou cette opération est-elle consciente ? nécessite-t-elle un effort ?

MÉMOIRE DES SENSATIONS

1° Avez-vous une bonne mémoire visuelle ? la mémoire des physionomies, paysages, figurations ?

2° Vos pensées ont-elles, en dehors de tout effort de votre part, tendance naturelle à s'accompagner des images visuelles qui leur sont propres ? Ces images sont-elles précises ? colorées ? Ou ne pensez-vous qu'avec des mots non accompagnés d'images ?

Mémoire auditive : Vous souvenez-vous bien de ce que vous avez entendu ? conversations, airs de musique ? Vos souvenirs auditifs se représentent-ils avec les qualités véritables des sensations auditives antérieures : hauteur, intensité, timbre ?

Mémoire verbale : Avez-vous la mémoire des dates, noms propres, chiffres ? Apprenez-vous facilement par cœur ? Que retenez-vous le mieux, de ce que vous avez entendu exposer, ou lu ?

Autres mémoires : Vous souvenez-vous longtemps des sensations gustatives, tactiles, olfactives éprouvées ? des sensations douloureuses ? — Des sensations de plaisir et de douleur physiques, lesquelles persistent davantage ?

III

Conception

1° Concevez-vous par association, en quelque sorte inconsciente et involontaire, d'idées ? Ou par association voulue, gouvernée, — donc, raisonnement, contrôle ?

2° Lorsque vous pensez, êtes-vous de ceux qui *entendent en dedans d'eux-mêmes*, mentalement, tous les mots de leur pensée ?

3° Etes-vous de ceux qui, au contraire, *lisent* les mots de leurs pensées, comme écrits devant eux ? — Si oui, lisez-vous de votre écriture, ou du caractère d'imprimerie ? Comment sont disposées les lignes ?

4° Appartenez-vous, enfin, à la classe de ceux *qui parlent mentalement* les mots de leurs pensées ? — Employez-vous toujours l'un de ces procédés pour certaines opérations intellectuelles, et toujours un autre pour certaines autres ?

5° Avez-vous l'esprit synthétique, ou analytique ? — Procédez-vous de préférence en vos raisonnements, par déduction ou induction ?

6° Avez-vous tendance à vous représenter sous forme concrète les notions abstraites ? Comment vous représentez-vous les notions d'infini, d'éternité, de perfection ?

7° Quelle est la part de l'intuition en vos raisonnements ?

8° Admettez-vous l'hypothèse ? c'est-à-dire le droit d'imaginer, supposer des choses possibles, démontrables ou non, pour en tirer une conséquence, ou expliquer certains phénomènes, ou établir un système ?

9° Admettez-vous le *rêve pur*, la rêverie sans bases sûres ou rigoureuses ?

IV

Action. — Ou mise en œuvre

1° Dans la procréation de l'œuvre, êtes-vous dominé, immédiatement, par le souci de « forme » ? ou « d'idée » ?

2° L'idée se réalise-t-elle *amorphe*, cherchant sa forme d'expression ? ou la réalisation de l'idée entraîne-t-elle *en même temps* sa forme expressive ?

3° Dans l'expression écrite, picturale, plastique, musicale, de l'idée, — quels sont le ou les genres de forme qui vous préoccupent ? (Rythmes, sonorités (timbres vocaux), coloration, images ou comparaisons).

4° Etes-vous plutôt harmoniste que mélodiste ? Coloriste que dessinateur ? Ou réciproquement.

5° Vos œuvres sont-elles synthétiques, ou analytiques ?

6° Dans vos œuvres, pouvez-vous indiquer la part due à l'influence sur vous des milieux sociaux, familiaux, professionnels, ou autres ?

NOTA. — Les réponses peuvent être en français, anglais, allemand, espagnol, italien ou portugais.

ANNEXE 2: Notes de travail pour un livre de psychologie sociale.(Inédites, archives privées, Hamon, 1902-3)

Notes de travail pour une psychologie sociale (archives privées inédites, Hamon 1902-1903).

PSYCHO SOCIALE.

Exposé ce qu'est la sociologie
son origine
Spencer Evolution de l'idée
Sociologie Etat actuel de la sociologie { ce qu'elle est / ce qu'elle veut

Qu'est-ce que la société — classes — groupes

— Influence de la société — de la collectivité sur l'individu
Lois — Enseignement et éducation — mœurs { préjugés, / traditions

— Influence du groupe restreint sur l'individu
mécanisme de l'action { profession / secte / religion / milieu familial et mondain

Psychologie des peuples — des races — des groupes ethniques
milieu géographique
milieu climatérique

Pyschologie économique de Tarde
Palante Précis de sociologie

— Influence de l'individu sur la société
génies { inventeurs / science théoriciens / hommes politiques / prophètes - religion / philosophes / criminels politiques / sectaire - révolution

Conclusions

Liste bibliographique

Psycho Sociale

Exposé ce qu'est la Sociologie.

Spencer — son origine —
Sociologie — Evolution de l'idée —
Etat actuel de la sociologie { ce qu'elle est / veut.

Influence de l'individu sur la Société
génies { inventeurs — science théoriciens
politique —
prophète — religi —
philosophe —
criminel politique
Sectaire — revolt.

Influence de la société — classes — groupes
de la Société — de la collectivité sur
l'individu
lois — enseignement — éducation moeurs — préjugés — tradition.
— du groupe restreint sur l'individu
profession —

Mécanisme de l'action
- secte
- Religion
- milieu familial et social

psychologie des peuples — des races — des groupes ethniques
milieu géographique
— Climatologie

Conclusions —

Liste bibliographie

Psychol. économi de Tarde
Lalande { Précis de Sociologie

PSYCHOLOGY BETWEEN SCIENCE AND SOCIETY[1)]

The development of psychology at the Catholic University of Nijmegen

R. Abma

Summary

Two general tendencies in the history of psychology are illustrated by an overview of the development of psychology at the Catholic University of Nijmegen. The first consists in the growing cleavage in psychology caused by the coexistence of both scientific and pragmatic aspirations. The second tendency is the loss of national and regional solutions for this problem.

Ever since psychology began to emerge as an empircal science, it has been subjected to two opposing tendencies: (1) the desire to become a science similar to the natural sciences, (2) the expectations of society that psychology would be of practical use in the fields of education, mental health, vocational counseling, selection, etc. The relationship between these two types of psychology is often conceived as a 'scientific' (experimental) psychology, which 'applies' its results in 'the field'. It is my contention, however that experimental psychology and psychology 'in the field' have, generally speaking, followed quite separate courses during the past century. This has resulted in a fundamental cleavage in present-day psychology.

This split was enhanced by a second development: the growing influence in Europe of American psychology and the subsequent decline of national and local traditions in European psychology. Before World War II, Dutch psychology was clearly an example of 'European' psychology with its own specific characteristics. These were lost when postwar American psychology took over and mainstream European psychology was relegated to second place.

I'd like to illustrate these developments by giving a brief account of the history of psychology at the Catholic University of

Nijmegen (The Netherlands). First, I will provide a brief sketch of the attitudes held by the Dutch Catholics towards psychology up untill 1930. Then, the development of psychology in Nijmegen will be treated, as divided in two major phases: 1930-1960, the period in which Rutten left his mark on empirical psychology; 1960 until now, when a new 'experimental revolution' in psychology took place.

Catholics and psychology

Catholic intellectuals and, more especially, church officials in the Netherlands were not very familiar with 'empirical' psychology. They were more at home with 'rational' (philosophical) psychology. They were, however, aware of its intention of creating a *'science* of man'. With Galileï and Darwin in mind, Catholics could not help but suspect that their own world view would come under fire should the human mind be subjected to the experimental method. What they feared most in the new science was its *determinism*: the reduction of the human mind and behavior to material stimuli and responses. This was in blatant contradiction to the Catholic axiom that human beings distinguish themselves from the animal world by the possession of rationality and free will.

Although wary of the supposed danger of empirical psychology on a *philosophical* level, some Catholic intellectuals in Holland did find themselves attracted to the *practical* possibilities of this new science of man. In 1917, for instance, *Jac. van Ginneken*, a Jesuït, ventured out of his own speciality of linguïstics into psychology. He advised the assembly of Catholic labor organizations in Holland to set up a 'Central Psychological Vocational Bureau' which could serve in placing the 'right man' in the 'right job', thus preventing psychological disturbances and curbing social unrest. The Bureau started its work in 1918 with Van Ginneken himself as its director. It was quite succesful until 1925. In addition, F. Roels was appointed reader in 'empirical and *applied* psychology' at the University of Utrecht in 1918. Four years later he became the first professor of psychology in Holland. He was also the first Catholic to hold such a chair. When the Catholic Univer-

sity of Nijmegen was founded in 1923, Roels received an appointment in Nijmegen as well for 'empirical and applied psychology, with an emphasis on educational and industrial psychology'. The admission of empirical psychology to the Nijmegen University, despite its supposed anti-Catholic implications at a philosophical level, may certainly be attributed to the expectation that psychology could be of practical use. At any rate, this conviction was held by *J. Hoogveld*, a very influential professor of education and cofounder of the Nijmegen University. Hoogveld claimed that 'in order to be up-to-date, education has to make use of the results of modern psychology'.

Psychology in Nijmegen 1930-1960

In its first few years, in Nijmegen empirical psychology did not amount to much. It could only be taken as a minor subject by students of philosophy, theology and law. Roels did not have much time left for Nijmegen because of his work in Utrecht. In 1926 things changed, when *Th. Rutten*, a former student of Roels, was given an appointment to set up laboratory courses in applied psychology. Convinced of the possibilities of this new science, Rutten set out to promote psychology both within and outside academia. In 1931, his enthousiasm and obvious intellectual capacities (he had obtained two doctorates, one of which summa cum laude) won him the professorship which Roels had been holding in Nijmegen. This appointment was followed by an incident, which clearly demonstrates the suspicion and the lack of knowledge concerning empirical psychology among the Catholic clergy. The bishop in charge, A. Diepen of 's-Hertogenbosch, sent for Rutten and asked him to explain experimental psychology. His answer ('it infers theories from experimental work') did not satisfy the bishop, and he led Rutten to understand that he was still an 'unexperienced young man' and 'was to avoid the subject of free will in his lectures'. Furthermore, Diepen opposed psychologists taking an advisory role in education. In his view, this was a matter for clergymen.

In his inaugural address ('New viewpoints in the methodology of experimental psychology') Rutten stressed that using the experimental method did not necessarily imply a deterministic view of man; in his opinion, human behavior could only be studied experimentally in those areas where it is determined by external, i.e. natural, social, causes. Rutten was convinced that an element of volition was involved in all human behavior, a point which he even tried to prove experimentally in his thesis 'The psychology of perception - a study of optical illusion' (1929). In this study, he used the Muller-Lyer illusion to demonstrate the Gestalt-principle that perception is a *constructive* process.

Being trained not only as a psychologist but also as a linguist, Rutten took a broad view of research methods. His methods included the systematical observation and careful registration of human behavior in everyday life. In fact, he considered this the starting point of all psychological research. An experiment should, in his opinion, be used mainly as a final check on conclusions drawn from observations. This view was a result of Rutten's desire to create a *pragmatic* psychology. In order to understand human behavior and provide adequate advice in case of problems, you have to stay close to the 'natural' situation in which the behavior occurs and not simply isolate a piece of behavior in the laboratory room. Rutten expanded the Gestalt-principle of 'the primacy of the whole above the sum of the parts' beyond perception to all fields of human behavior in order to support his view. Thus, elements of behavior should always be related to the total situation, including the goals which direct and unify behavior.

With this relatively coherent set of views regarding the various aspects of psychology (its domain, method, theory, practice) Rutten approached those areas of society which might 'benefit from psychology'; education, industry, mental health, child guidance, teaching and training. While doing this he had to be careful in maintaining an appropriate relationship with the Catholic church. On the one hand, Rutten was determined to keep psychology free of church influence. Psychology, he often stated, is methodo-

logically speaking devoid of value judgements, including those of the church. On the other hand, he always made sure that his recommendations did not contradict offical Catholic doctrine. It was only his line of argument which differed. For example, the traditional family structure ought to be preserved because of the *psychological benefits* for its members; class struggle was considered an evil caused by distortions in *human relations*. Along these lines he succeeded in introducing a *humanism* which merged very well with the post-war Catholic ideology.

By the time World War II broke out, Rutten was well-known and well-regarded throughout the Catholic community in Holland. He was looked for advice in both educational as well as industrial matters. Even the clergy seemed less suspicious. Rutten took advantage of this situation entering claims for other professorships in Nijmegen. As soon as the war was over, professors and readers began to be appointed for psychopathology, developmental and theoretical psychology, psychodiagnostics and the psychology of culture and religion, Rutten himself continued to lecture on general, social, and industrial psychology. All these professors were, of course, 'good Catholics', which more or less influenced the kind of psychology they taught. This did not prevent them, however, from developing a critical view on wrongs perpetrated within the Catholic world. Particularly F. Buytendijk (theoretical psychology) and H. Fortmann (psychology of culture and religion) were deeply involved in a struggle against the Catholic morality, which tended to reduce psychological problems faced by Catholics to moral issues or sinful behavior. Buytendijk and Fortmann, on the other hand, considered such matters primarily as mental health problems. On a more theoretical level, both were greatly influenced by the Existentialism of Sartre and the Phenomenology of Merleau-Ponty.

By the end of the fifties, psychology in Nijmegen had become a full-grown department, still firmly anchored in the organizational framework provided by Rutten. Experimental work remained subordinate to a psychological discipline which was both pragmatic and humanistic in orientation.

The second experimental revolution

At about the same time a younger generation of psychologists in Holland (De Groot, Snijders, Kouwer, etc.) were beginning to grow dissatisfied with the 'unscientific character' of Dutch psychology. Impressed by the methods and results of American experimental psychology, they propagated a more quantitative and objective approach, expecially in psychodiagnostics. They opposed the widespread qualitative and subjective approach being used, particularly its theoretical manifestations as respresented by the 'Phenomenological school' in Utrecht (Buytendijk, Linschoten, etc.). In Nijmegen too, a younger generation of students and staff members had started a silent revolution. Psychology must be devoid of the value judgments present in theoretical humanism and should once again look at the natural sciences for its model.

Rutten partly supported this development. Directly after World War II, he had visisted the United States and had been able to get financial aid for the rebuilding of the psychological laboratory, which had been burned during the war. He also arranged for American professors to take their sabbatical year in Nijmegen, with the possibility for Nijmegen students to take courses in the U.S. Although he was aware of the fundamental differences between U.S. psychology and the Dutch tradition, Rutten was convinced of the importance of developments in American Psychology. In his opinion, psychology should be pluralistic. Human behavior had to be studied in the laboratory, as well as in everyday life. In addition to a psychology of learning, perception and motivation, Nijmegen needed to develop a psychology of human behavior (gedragsleer). This, he maintained, could serve as theoretical and methodological framework for the observational studies in various areas of everyday life.

The younger generation of Nijmegen staff members, however, was more interested in psychological *processes* than in human behavior.They had considered the experiment the 'via regia' for scientific psychology. Compared with the thirties, experimental psychologists had more adequate apparatus and better statistical techniques at their disposal. For this reason, experimental psychology

was able to expand to such a degree that it could become the dominant ideological force in academic psychology. Between 1960 and 1968, readers and professors were appointed in Nijmegen for experimental methodology, psychology of learning, motivation and perception, mathematical psychology and statistics, and animal and physiological psychology. This was accompanied by a profound change in climate and scope, corresponding to the more general waning of church influence.

In the course of the sixties, nearly all Dutch psychology, including Nijmegen, lost its specific local character and became subordinate to American mainstream psychology. The latter had undergone a transition which was not without repercussions in Holland. On the one hand, the development of an experimental, 'value-free' psychology, and on the other hand, the emergence of various humanistic psychotherapies and interview techniques, spurred on by Carl Rogers' client-centered therapy. The differences between two forms of psychological activity appeared too great to make reconciliation in one unified scientific discipline possible[2].

Concluding remarks: (1) As long as Nijmegen psychology had a limited and regional character, it was able to combine theory, method and practice in a flexible way. The introduction of American thought, produced an experimental as well as a therapeutic revolution which split up psychology. In the academic world, this cleavage is often hidden behind formulas like 'experimental and *applied* psychology' or 'specialization in one of the fields of psychology', suggesting some unity where there is, in fact, none to be found.
(2) Psychology has been juggling between science and society. In the course of the last century, it was torn apart by the desire to become a 'real science' and the desire to be pragmatic. Whereas Rutten tried and partly succeeded in combining both requirements, his original synthesis of science and society was pushed aside when American psychology took the lead and the ranks of psychology became filled with 'specialists'.

Notes

1) This text is based on a historical investigation which is more extensively reported in: R. Abma: Psychologie en katholicisme: een episode uit de geschiedenis van de Nijmeegse psychologie, in *Psychologie en maatschappij*, 1979, 7, pp. 35-65 (including a list of references and sources). Helpful comments on the current text were made by Kathy Davis, Harrie Kempen, Sylvia Lammers and Paul Voestermans.

2) Obviously there is more to psychology than experimenting and counseling. I have chosen these two extremes to demonstrate the cleavage in psychology which I view as fundamental. I have worked this out in more detail in: R. Abma. Psychologie als historisch verschijnsel, in H. Boutellier en L. Wouda (red.). *Progressieve ontwikkelingen in de psychologie*, SUA, Amsterdam, 1981, pp. 314-319.

EMOTION: SOME REFLECTIONS ON JAMES' CONCEPTION

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SUMMARY

In this paper, James' theory of emotion is expounded as an effort to clear the ground for a new, unitary paradigm of emotion-research in psychology.

It is maintained, with a reference to Wittgenstein's philosophy of mind, that James failed to provide, at least in some important respects, a consistent framework for later theorizing and empirical research. However, his views under discussion here have exerted an undeniable influence on the 20th century psychology of emotion; this may be considered a rather paradoxical positive effect of a conceptual confusion.

EMOTION

To set the stage for the problem I want to discuss in this paper, let me take you back in time to the International Congress of Psychology held in Rome, 77 years ago.

On April 30, 1905 William James read a paper in French titled *'La notion de conscience'*, which was about radical empiricism. He sent this paper to his friend Charles Sanders Peirce, who replied enthousiastically: (July 23, 1905) "When you write English (it is better to say the disagreeable thing) I can seldom at all satisfy myself that I know what you are driving at. Your writing would, I can see, be immensibly forcible if one knew what you meant; but one ... don't". This was by no means a unique isolated comment on the work of his friend. And from time to time James replied in the same spirit; though *he*, in his more modest way, explained that he didn't understand a word of Pierce's writings. Not much later Peirce wrote in another letter: "I just have one lingering whish, for your sake and that of the countless minds that, directly or indirectly, you

influence. It is that you, if you are not too old, would try to learn to think with more exactitude. If you had a fortnight to spare I believe I could do something for you, and through you to the world; but perhaps I do not sufficiently take account of other psychical conditions than purely rational ones ..." However, Peirce continued: "I have often, both in my lextures and in my printed papers, pointed out how far higher is the faculty of reasoning from rather inexact ideas than of reasoning from formal definitions ..." (Peirce, CP 8, 260).

It is good to note that at the time this was written, James had reached the respectable age of 65, while Pierce was 3 years older. There is no historical evidence that James took the lessons offered by Peirce. Maybe the reason is indicated by Peirce himself: James was perfectly well able to reason in a specific domain, that is to say, he found his way with sufficient exactitude in the fuzzy problems of psychology. Nevertheless, his ideas and arguments were sometimes confused, and it is about one of these ideas that I whish to raise a few questions in the following. With this in mid let us take a closer look at James' theory of emotion.

As an exact measurestick, if such a thing is possible at all, I will use some ideas of what has been called the most influential philosopher of this century, Ludwig Wittgenstein, whose ideas are fairly well-known from works like the *Philosophical Investigations, The Blue and Brown Books* and *Zettel*. When I shall quote, however, I will mainly draw upon the volumes titles *'Remarks on the Philosophy of Psychology'* that have recently appeared in a bilangual edition. It is remarkable that so little has been written on the influence that James had on Wittgenstein, or on the relation between the two, e.g. concerning their views of religion and mysticism. To my knowledge, virtually nothing has been written concerning the influence of James' pragmatism on Wittgenstein, nor about the inspiration Wittgenstein got from reading James' *Principles of Psychology* (PP). Although many people know that James explained the concept of 'family-resemblances' in Lecture 2 of his *'Varieties of Religious Experience'*, just a few notes have appeared on the meaning of

religion in the *'Varieties of Religious Experience'* and in Wittgenstein's *Tractatus.* Les us first take a look at James' theory of emotion, the so-called James-lange theory as we find it expounded in every textbook of psychology nowadays. As Titchener claimed in a paper he published in 1914, the *one* novel feature of James' theory was his assertion of its novelty. However I will leave the historical question as to the origins of this theory for what it is, because even a tentative answer would lead us back at least to Aristotle. About emotions like grief, fear, rage and love James states: "Our natural way of thinking about these coarser emotions is that the mental perception of some facts excites the mental affection called emotion, and that this latter state of mind gives rise to the bodily expression. My theory, on the contrary, is that the *bodily changes follow directly the perception of the exiting fact, and that our feeling of the same changes as they occur IS the emotion*" (PP, 449). Common-sense says, we tremble because we are afraid, but James thinks it is the other way around: we are afraid because we tremble. Immediately, critics pointed out that some people tremble, while others don't, and asked for an explanation. Of course, for any common-sense theory of emotion, the answer is obvious.

In a reaction to this critics, James explained that it is the total situation which is the 'object' to which the subject, formed by its history, reacts. (*CER,* p. 50; *'The Physical Basis of Emotion'*, 1894). Had he worked out this idea, he would have arrived at a cognitive theory of the sort we will discuss later on in this paper. But he never returned to the problem. For this reason I think the Dutch psychologist Linschoten was mistaken, in his book on James, to speak of James' "phenomenological theory of emotion"; his thesis demands a good deal of *hineininterpretieren.*

So, in some situations we run away, in other situations, when we are armed, we take a shot at the bear. Only in the first case, it is clear from our reaction that we are afraid; in the second case,

a hunter may be happy with the sudden opportunity of a little exercise.

What are James' arguments in favour of his theory? He argues that our body reacts to objects and events with numerous and subtle changes, changes which we are able to feel. If we fancy some strong emotion, and then try to abstract from our consciousness of it all the feelings of its bodily symptoms, we find we have nothing left. The affections are made up of those bodily changes which we ordinarily call their expressions. This can also be concluded from the fact that the voluntary arousal of the expression of a special emotion is able to produce the emotion itself. E.g., panic is increased by flight, and 'each fit of sobbing makes the sorrow more acute'. (*PP*. 463).

With this new theory, James hoped to provide a new framework for psychological research into emotion. The trouble with 19th century psychology was its lack of a central point of view; psychologists did not look for the general causes of the emotions. Now, these are indubitably physiological, according to James, and the new theory states the problem as a causal question: "just what changes does this object and what changes does that object excite? ... We step from a superficial to a deep order of inquiry ... We now have the question as to how any given 'expression' of anger or fear may have come to exist; and that is a real question of physiological mechanics on the one hand, and of history on the other ..." (*PP*, 453/4).

As you probably have noticed, there is some similarity between these ideas and Wittgenstein's views on the relation between emotion and expression. With his private language argument, as brought forward in the *Philosophical Investigations*, Wittgenstein attacked the popular notion of subjective, private feelings, that are sometimes given expression or result in certain actions, but are really known only to the individual himself. That conception presupposes the existence of a private language, and Wittgenstein showed the

impossibility of such a thing with references to the concept of rule-following behaviour. A rule cannot be followed just by one person or only once. (*P.I.*, 199). If we view the expression of an emotion as if it were the description of a private object, the object falls out of the picture.

This criticism of private languages is accompanied by a positive account of how feelings get their meaning and how the meaning is related to their expression. Basically, the positive amount runs like this: children first react to their environment in a natural way, with 'primitive reactions' (*RPP*, I, 313), e.g. they will cry when feelings hungry. In the *RPP* Wittgenstein asks: "How do you know, then, that the experience that you have is the one we call 'pain'?". And his answer is: "That is different - I know that, because my spontaneous behaviour in certain situations is what is called the expression of pain" (*RPP*, I, 304). Here I presuppose that these remarks on pain can be generalized to at least the 'coarser' emotions. Gradually, we learn rules and are able to transform the natural expressions of emotion into more 'civilized' and conventional responses when necessary. E.g., we will not cry immediately, but say instead: 'I feel sad, because this or that happened'. Our reaction is called the expression of an emotion, but it would be wrong to regard it as an external sign of an internal event: this whole terminology is inadequate, although it is true that we sometimes try to hide our feelings or thoughts from others. As is well-known, however, people frequently fail to do this adequately, with the result that others know their feelings as well as, or even better than, they themselves do. This is possible because the criteria for emotions are behavioral reactions. 'If someone behaves in such-and-such a way under such-and-such circumstances, we say that she is sad. (We say it of a dog too). To this extent it cannot be said that the behavior is the *cause* of the sadness; it is its symptom' (*RRP*, II, 324).
Now, in view of all this, it becomes easy to understand what Wittgenstein's criticism of James' theory of emotion consists of.

According to Wittgenstein, the relation between an emotion and its expression is not a contingent fact, a co-variation of two independent entities: it is a logical or conceptual relation. James, as much disagreeing with the traditional view as Wittgenstein, tries to correct the errors of this view by suggesting that, as a matter of contingent facts, the order of sequence is contrary to what is commonly believed. The emotion follows as it were its 'expression', and the stuff out of which emotions are made is bodily reactions. That is to say, he presents his solution in the form of an empirical hypothesis, which could essentially be subjected to experimental testing. Here it should be added that James admitted the difficulty of testing the theory in practice (cf. *PP*, II, 454). It is this self-misunderstanding in James' psychology that Wittgenstein tries to correct. He appreciates James' insight into the nature and meaning of emotions and their expression, and insight of real philosophical interest, that would eventually have the power to clear the ground for empirical psychology. However, in order to make his ideas acceptable to practitioners in the field, James thought it necessary to present his idea in the form of a testable, empirical hypothesis which would - in accordance to the principles of pragmatism -lead to some particular consequence in our future practical experience. (cf. *CER*, P. 412). This meant he had to vulgarize its philosophical contents. The result was a mongrel product, not a philosophical statement and not an empirical theory either.

In a different context, Wittgenstein mentions James' psychology in one breath with Goethe's theory of colours: "it really isn't a theory at all. Nothing can be predicted with it. It is, rather, a vague schematic outline of the sort we find in James' psychology. Nor is there any *experimentum crucis* which could decide for or against the theory" (*RC*, I, 70). And on James he comments elsewhere: 'here too James says something that sounds like a psychological statement and is not one ... it would have to be proven by the experience of individuals' (*RPP*, I, 173).

Let us sum up how Wittgenstein evaluates the James-Lange theory of emotion. He agrees with it on four main points.

1. Emotions are tied up intimately with bodily reactions, and in a sense, the reaction of the body indeed *is* the emotion.
2. James' revision of the traditional view, with its dualistic suppositions, is justified and necessary.
3. Emotions are not private phenomena, and any psychology which wishes to regard them as such is doomed to end either in subjective introspectionism or in a pseudo-objective behaviorism.
4. The object of emotion is the total situation, an event within its context.

However, he disagrees with James on the following issues:

a. To speak of a 'sequence in time' of bodily reaction and emotion is misleading. The real issue consists in the conceptual connection between the two.
b. James replaces the traditional dualism by another dualism of physiological events and the subjective perception of these events.
c. To say that emotion is made up bodily changes is making a pseudoempirical statement. Bodily changes are only the sign or criteria of what we call emotions (cf: *RPP*, I, 451/6; II, 325; *Z*, 495). 'It cannot be said that the behaviour is the *cause* of sadness: it is its symptom' (*RPP*, II, 324).
d. In the first-person case, I cannot say that I am afraid *because* I tremble, as James contends. If asked for the reason for my fear I would point to the bear, and not to my trembling hand (*RPP*, II, 24; cf *RPP*, I, 454).

What we have said about Wittgenstein's criticism so far leads to the following conclusion. James clearly recognized a conceptual problem in the foundations of late nineteenth century research into emotion. He conceived part of the right solution, but because he tried to present it as an empirical hypothesis he detracted from

his original philosophical conception. Subsequently, this vulgarization of his philosophical ideas lead to much discussion and confusion.

You will have understood that so far I am in general agreement with Wittgenstein's views. If there is any truth in his theory of emotion, or better, in his philosophy of mind, we would expect it to have exerted some influence on empirical psychology. So, it is interesting to take a look at the state of affairs in present-day psychology. Did Wittgenstein's idea exert any real influence? Are the shortcomings of the James-Lange theory that we discussed earlier indeed recognized as mistakes by theorists in the field of psychology? Textbooks are careful to explain the essentials of James-Lange to students of psychology, along with the Cannon-Bard theory and the Schachter-Singer theory. The reason for this is, that the James-Lange theory is at the roots of the newer theories that emerged during this century. And although nobody literally believes in James-Lange, many researchers in the field agree that it has never been falsified properly. The Schachter-Singer or cognitive-arousal theory, which gave rise to a lot of empirical research during the past 20 years, is basically Jamesian. It suggests that the emotions that we report to ourselves or to others result from the ways in which we interpret not only our state of arousal, but also the situation causing the arousal. It is presupposed that there is only one form of bodily arousal which only varies in intensity. Consequently, the bodily reaction in itself cannot provide us with much information about the precise nature of our emotion. This is the main difference with James, who speaks of the body as a perfect 'soundingboard' that reacts differently to every new stimulus. Cognitive arousal theory has lead to a lot of experimental studies that try to confirm the idea that people lable their emotions in concurrence with their cognitive interpretations of the context in which they occur. It seems plausible indeed that we are attributing the causes of our emotions to the environment.

To return to the question concerning Wittgenstein's influence on empirical psychology: I am afraid this influence is non-existent, at least I could not point to any idea in present-day theory or methodology that really has been inspired by his ideas. Wittgenstein's critics, I suppose, will conclude from this that his philosophy of mind is simply too outrageous to find any support among men of science. I don't think one could stage it that easy. Wittgenstein was not so excentric in his philosophical ideas as he is sometimes believed to be. With his theory of emotion he finds himself in basic agreement with many other philosophers. I mention only some thinkers of a phenomenological bent, e.g. Sartre: *'Esquisse d'une theory des emotions'* or Frijda's theory of the recognition of emotion, and German philosophers like Scheler and Plessner. Still, the answer to the question 'what is an emotion?' has not yet been found. A quotation from a recent study by Leventhal who aims at a comprehensive theory of emotion (1980) will suffice: "The one thing upon which various emotion theorists agree is, that the concept of emotion is poorly defined and research is fragmented and unintegrated" (Leventhal 1980, p. 140).

James certainly would feel sorry might he hear this, and perhaps turn himself in his grave, if he was still alive.
There are several answers possible with regard to such a sceptic statement, of which I mention only three that come directly to mind.
One could think, first of all, that the one definition that will eventually bring peace and cooperation between the various theories in psychology has not yet been found. Second, one could maintain that one's own definition is the best ever given, while other theories stubbornly fail to recognize this fact. One could also, thirdly, argue that *one* definition of emotion will never be agreed on, because the exclusion of other definitions and visions would be impossible, or at least highly improbable, on conceptual and logic grounds.

According to this last view, emotion and its definition founds a phenomenon dependent on historical and cultural notions that are changing continuously. Many workers in psychology have a problem in recognizing this possibility and admitting its reality.

I would suggest here that psychologists are still committing the same mistake as that made by James, that is to say, they only believe in facts, experimental data with immediate 'cash-value' and are fairly disinterested in theoretical conceptions.
Seen from this point of view, James' errors and alleged self-misunderstanding turn out to be a perfect understanding of the wants of his fellow scientists. Or, in the words of Peirce, it is the faculty of reasoning from rather inexact ideas, where one cannot furnish to logicians the exact forms that they are skilled in dealing with, but where, nevertheless, one comes to the right conclusions in most cases.
"That faculty makes one useful", wrote Peirce, "while I am like a miser who picks up things that *might be* useful to the right person at the right time, but which, in fact, are utterly useless to anybody else ..." (letter, june 13, 1907).

This might be read as an indication of the difference between Peirce's pragmaticism ('effects that might conceivably have practical bearings') and James' interpretation of it, the "cash-value pragmatism" that has been so much more influential.

James conceived the conceptual framework that he creates as a fresh alternative to dull classification into an empirical hypothesis. This made his theory, indeed, influential both in his times and later on in this century.

The question with which I want to conclude is: will it remain influential in the long run, and if so, for what reason? Because it forms a testable hypothesis, *or*, on the contrary, because it is one of the various possible ground-positions in the philosophy of

emotion, that will turn up again in the course of history, with different faces?

I cannot resist the temptation to quote one time more, as a response to my own question. Wittgenstein gave a partial answer in his criticism of presentday psychology: "in psychology there are experimental methods and *conceptual confusion* (...). The existence of the experimental method *makes us think* we have the means of solving the problems which trouble us: though problem and method pass each other by" (*PI*, p. 232).

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INTROSPECTION - A REAPPRAISAL

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Abstract

The case is made for cognitive-psychological research based on introspection. At the heart of the argument is the call for a basic distinction between data collection and theory construction. Introspection may be used for the former, but not for the latter. Once this segregation is maintained much of the standard criticism of introspection does not seem to hold. By way of implementation, the basic distinction is supplemented by consideration of the choice of material to be studied, of the level of the analysis, of the mode of interpretation, as well as of the epistemological status of the investigation. Throughout the discussion reference is made both to historical cases and to current research conducted by the author.

Introspection has a notorious history. No other method of psychological research has been the target of such criticism, both extensive and harsh. Boring, (1953) the famous historian of the field, might have been blatant when saying that "Introspection that does not lie does not exist", but certainly his appraisal was not unrepresentative. Indeed, throughout the century of their existence, psychology in general and cognitive psychology in particular have viewed their freeing themselves from introspection as one of the foremost steps towards becoming full-fledged scientific disciplines. In the past few decades this stance, as well as the practical ramifications it implies, have been particularly pronounced.

There have also been other voices. While not numerous, these have been definitely prominent. In surveying the methods of psychology, William James (1890) went as far as claiming that

> Introspective observation is what we have to rely on first and foremost and always. The word introspection need hardly be defined -it means, of course, the looking into our own minds and reporting what we there discover. Every one agrees that we there discover states of consciousness. The existence of such states has never been doubted ... All people unhesitantly believe that they feel themselves thinking, and that they distinguish the mental state as an inward activity or passion, for all the objects with which it may cognitively deal. I regard this belief as the most fundamental of all the postulates of Psychology, and shall discard all curious inquiries about its certainty as too metaphysical ...

Following these comments, James offered his, by now, classical, "investigation of the mind from within", namely, his study of the stream of thought (1890, chapter IX).

James was not the first. In 1879 Galton published his "Psychometric experiments" (see Crovitz, 1970) in which he "desired to show how whole strata of mental operations that have lapsed out of ordinary consciousness admit of being dragged into light, recorded and treated statistically, and how the obscurity that attends the initial steps of our thoughts can thus be pierced and dissipated. He proclaimed showing "measurably the rate at which associations sprung up, their character, the date of their first formation, their tendency to recurrence, and their relative precedence". In his famous walk Galton "scrutinised with attention every successive object that caught (his) eyes, and (he) allowed (his) attention to rest on it until one or two thoughts has arisen through direct association with that object; then (he) took very brief mental note of them, and passed on to the next object". The repeated walks eventually produced a compendium of 75 words and ideas associated with them. "The results" collected in the course of "a most repugnant and laborious work", were deemed by the ardent introspectionist "to be as trustworthy as any other statistical series that has been collected with equal care". Ironically, Galton might have been bogged down by his adamancy on methodology and statistics. In his

experiments he created, in fact, the method of free association and even came to the appreciation of its power in eliciting vast samples of seemingly bygone incidents from the course of one's entire life, with a persistent bias on early childhood experiences. Galton failed, however, to draw one step further and to consolidate his insightful self-observations into a theory. Such an enterprise was later taken up by Freud.

The method of introspection clearly plays a crucial role in both the practice of psychoanalysis and in its theoretical development. Clinically, most of the material for the analytic work is furnished by means of this method. Dreams and free associations could simply not be documented in any other fashion. Historically, this process was also crucial in the very construction of the psychoanalytic theory; it is well-known that many of Freud's psychological insights are the fruits of his own laborious self-introspection.

James reflected, but actually he did not conduct systematic cognitive investigation. Galton was daring is his pilot studies, but he did not develop it, and Freud's clinical work was primarily concerned with the affective. Only with the Würzburgers was the introspective investigation taken as the cornerstone of a fully-flegded scientific paradigm. Külpe, Mayer, Marbe and Orth are names which directly rise in the psychologist's mind when the introspective method is mentioned. I guess the common feeling evoked by the work of these early investigators is coloured by romantic enthusiasm, relentless inquisitiveness and scrupulous toil yet, all told, the research program is considered a dead end failure, fruitless even in the juvenile days of psychology. As pointed out at the time by Wundt (see Humphrey, 1963) and by other psychologists since, introspection is subjective, its findings are of limited reliability, they do not allow independent checks and they are not replicable. Arguments of this kind, which have been marshalled against the Würzburgers, have come to be taken as arguments against introspection in general. In fact, the downfall of that school has been since regarded as demonstrative that the criticism is, indeed, decisive and that the issue is closed.

The present paper sets itself to put the issue of introspection in a new perspective. It is guided by the belief that even if the arguments raised against the Würzburgers are valid, they need not hold as principled arguments against introspection in general. The method of introspection is clearly problematic, yet this does not necessarily imply that it should never be used. Rather, the very criticism suggests that the employment of introspection should be guided and constrained. For such an employment to be appropriate, the limits of the methods should be appreciated, and guidelines, proper for its use should be defined. The examination of these is the subject matter of the present paper.

By way of substantiating the distinction between the Würzburgian and the principles use of introspection, let us consider the introspective practices of the members of this school. Marbe (1901), for example, asked his subjects (amongst them prominent psychologists) to observe themselves as they executed simple cognitive tasks, and to record the exact processing that was involved. One such task was the addition of 8 to 7, and it elicited the following reports. (The following is my free translation from the original, not otherwise translated, German): "The picture of the number 15 appears attached to the interrogation mark", "the inner pronounciation of the word 'fifteen' appears after a short pause", "15 appears without any association", "no picture of the number form". Reports of this kind were taken as conclusive and demonstrative, and the nature of mental representation and cognitive processes was taken as immediately determined by them. Specifically, the reports constituted the proofs that people think in words, in pictures, or in imageless forms. Indeed, it was exactly on this basis that the existence of the *Bewusstseinslage*, i.e. the imageless thought, was established. The discovery of this mode of thinking was taken by the Würzburgers to be their most significant psychological discovery (for a review, see Humphrey, 1963).

The use of introspection just described may be best characterized as *direct*. As exemplified by Marbe's account, the introspectionist psychologist is interested in the workings of the human

mind, and he wants to construct a theoretical model of its dynamics. In order to accomplish this goal, he sets himself to look inside. He is attentive, and he tries to be as accurate as possible. Hopefully, the care would prove worthwhile and actual mental structures and processes would be observed, detected and deciphered. Isn't careful looking the essence of science? Alas, this is the Achilles heel of the entire enterprise. One cannot simply turn one's head inside, so to speak, look and settle psychological issues. These remarks, note, are not specific to introspection. In no science can theoretical issues be solved by the direct observation of facts. As pointed out by Feyerabend (1978) even telescopic observations are not immune from the vagaries of the inspector who interprets them. In general, all instruments have their own rules of use, and for one to profit from an instrument one has to acknowledge these rules and to appreciate its limitations. Thus, much of the misuse of introspection resulted, to my mind, from the failure to recognize the logic of the employment of this instrument and from a consequent breaching of the limitations it imposes. It is to the examination of this topic that I now turn.

The first step of the examination is already defined by the foregoing discussion. I refer to the distinction between data and theoretical interpretation. Essential as they are, facts do not constitute science: for this theories are needed. The Würzburgers, it appears, failed to make the distinction between the two, nor did they appreciate the fact that theories are not direct reflections of data. On the one hand, as noted, the generation of theories is mediated by instruments associated with procedures of interpretation. On the other hand, as pointed out by Koffka (1912, see Mandler and Mandler, 1964) in his (not much known, but higly modernistic) critique of introspective *Denkpsychologie*, it is one thing to perceive an image, and another to postulate "image" as a theoretical entity in psychological modelling. As marked by Pylyshyn (1973) in a totally different context, an image may very well be experienced but unless it is systematically incorporated in a

cognitive account it is only an epiphenomenon. What is implied, then, is a fundamental distinction between *data collection* and *theory construction*. Surely, this distinction is not particular to introspection, but with this method it gains special significance. Indeed, it seems to me that much of the criticism marshalled against introspection should be taken not as holding against the method as such, but rather against the misuse resulting from the failure to make this distinction.

The distinction, I think, also sets guidelines for the proper use of introspection. Introspection should be used for the retrieval of data, but not for the detection of processes, let alone the construal of models, or the establishment of theories. Naturally, the data collected will be subjective, for only the one particular introspectionist could have generated them. For this very reason, such data cannot (as James noted) be generated by means of any other method. Yet, it should be clear that having generated the data, the introspectionist ceases to hold any priviledged status with respect to them. For any psychological conslusion to be drawn the products of introspection have to be open to public analysis as any other linguistic or cognitive material would be. That data collection and theory generation are distinct not only sets a restriction on the role of the introspectionist, but also imposes a limitation on the theoretician's use of the data he analyses. Just as the introspectionist is confined to the furnishing of data, the analyzer is confined to the analysis. In other words, the analyzer has to adopt a strict phenomenological approach, one which takes the data as given. Specifically, the analyzer cannot stipulate that the date is inaccurate or incomplete and therefore suggest modifications or ammendments to it. Rather, taking the data as they are, the analyzer sets himself to detect the regular patterns in the data and to offer ordered characterizations and anchor them in a theoretical framework.

Simple as it is, the division of labor just noted already offers a preliminary response to the standard criticism of introspection. The data are, indeed, objectively derived but the analysis

is not dependent on the particular introspectionist. Being conducted by other people the analysis is objective, replicable and amenable to repeated and independent checks. Yet, clearly, this solution is not sufficient. Objective and rigorous as the analysis may be, if its subject matter is capricious, what use and interest can it have? Necessary as it is, our distinction is, indeed, not sufficient. It has to be ammended by constraints on the types of material to be investigated, on the appropriate level of their analysis, and on the scope of their interpretation. These *material, analytic* and *interpretative* considerations, as well as the *epistemological* ones that will follow them, are now to be presented. By way of illustration, and in order to concretize the discussion, the presentation will center around a particular case, drawn from my own empirical research.

First, the material considerations. To counter the subjective character of introspection we would like the material investigated to be one that minimizes individual differences. Experimental-psychological experience of the past two decades suggest that variance is smaller, even insignificant, in tasks which are highly automated. Such tasks involve little, if any, problem-solving, and are carried out without the individual being aware of the procedures involved in their execution. Such tasks are likely to reflect the natural working of the mind, and not the strategies and heuristics developed by individuals in response to different contextual demands. The tasks employed by the Würzburgers were not of this kind. They were solicited by an external agent and they were often quite complicated. As a case which does meet the criteria noted above I would like to present *thought sequences* (Shanon 1983), that is the series of verbal-like expressions which freely pass in one's head. These series, of which (*) is an example are characterized by their having experientially distinct beginnings and ends, and of their being composed of a rather small number of discrete steps:

(*) 0. A day earlier the thinker had tried to remember a friend's name and couldn't. That same day friends from Haifa came to visit and someone said "You've got it cold in Jerusalem". The sequence starts with a recollection of

the utterance:

1. 'You've got it cold in Jerusalem'
2. And in Tel Aviv?
3. In Tel Aviv - Gabi
4. "Tip of the tongue" in an interesting phenomenon
5. This is "TOTT"
6. No, "TOT"
7. "TOP"
8. The concept of "iceberg" and its "top"
9. Freud: the conscious is like the top part and the unconscious consists of the bottom part.

The particular example of thought sequence marks another advantage in the study of naturally occuring, automated introspective material. As noted in (*), thought-sequences typically reveal themselves in introspection as data, not as processes. This state of affairs further accentuates the segregation advocated here. Processes are less neutral than data (data may, of course, not be neutral either, but without data one cannot get off the ground at all) and more varied. The variation has already been noted above: it is due to psychological processing being heuristic, hence sensitive to contextual variations. The neutrality is due to the fact that the report of processes involves reflection and self-observation. Moreover, the definition of processes presupposes a conceptual framework, hence hypotheses (even if only implicit) regarding the workings of the minds. For these reasons, processes seem to call for more involvement of the introspectionist than data do. Processes involve in a sense some analysis on the part of the introspectionist himself, whereas data minimize the role of the introspectionist and pass the bulk of the analysis to the observer. (For empirical evidence indicating that people's reports of data are more accurate than their reports of processes (see Nisbett and Wilson, 1977; Ericsson and Simon, 1980).

In concluding the material considerations let me note that these are not only qualitative but also quantitative. Clearly, the analysis is to be based on a corpus of data furnished by different

people. While each piece of data in itself provides only a limited basis for analysis, the corpus as a totality can reveal systematic patterns. In this manner the analysis made and the interpretations drawn become less dependent on the contribution of the individual introspectionist. The study of thought sequence confirms these observations. Having amassed a corpus of several hundreds of sequences, a state of stability has been reached in which the description of the structural patterns in the corpus is not affected by the addition of new data. In other words, the increase in the number of tokens ceases to increase the variety of types. Such a state clearly allows for objective analysis which is amenable to repeated, independent check.

The contrast between token and type brings us to the considerations which concern the appropriate level of analysis. The heart of the argument is that this level cannot be that of the atomic expressions which comprise the sequence. The reason bears on the individual variance already noted above. Being particular descriptions and assertions, the atomic expressions reflect contents pertaining to the introspectionist's individual experiences. Furthermore, being the direct products of introspections, these expressions are more likely to be affected by the procedure of report itself. Moreover, the atomic expressions do not allow for proper check and evaluation. Such check would require a comparison of the overt expressions with covert structures, the underlying thoughts that are presumably hidden in the cognizer's mind. The presently advocated methodology perspective, however, does not allow such a comparison to be made. The division between data and analysis coupled with the phenomenological approach exclude reference to covert structures. For us, the only persmissible comparisons are between entities which are overt. Consequently, it is not single thought expressions, but rather parts thereof which define the smallest unit of our analysis. This shift of level, note, also sets a shift in the type of questions to be found in the analysis. Whereas the questions found in conjunction with single expressions

are typically contentual, the ones found in conjunction with the relations defining higher-ordered entities may also be structural. Structures are less particular than content; they are not associated with the introspectionist's idiosyncratic experiences, nor are they likely to be consciously appreciated by him. For all these reasons, structures are less affected by the process of data collection and they are more amenable to theoretical formulation. Specifically, abstract and not context-dependent structures can appear in conjunction with different contents. As a consequence they permit repeated checks, hence objective evaluations and more general conclusions.

The last remarks draw us to the interpretative considerations. The shift from content to structure entails a shift from an extrinsic evaluation to one which is intrinsic. The two types of evaluation are associated with two basic theories of truth: that of correspondence and that of coherence. As pointed out by the standard critiques of introspection, and as stipulated by the present methodological restrictions, data collected by means of this method cannot be evaluated on the basis of correspondence. The standard conclusion is that data allow for no objective evaluation whatsoever. The above mentioned analytical considerations, however, readily suggest evaluation on the basis of coherence. Such an evaluation will consist of the definitions of regular patterns which together define a compact well-ordered system. The study of a large corpus of thought sequences suggests that the structural relations between coupled thought expressions do, in fact, define such a system. Specifically, while there is no principled restriction on what thoughts may pass in one's mind, the relationship between adjacent thought expressions do seem to be constrained. It appears that the number of such possible relationships is small and that together these relationships define a formalizable coherent system (for details, see Shanon, 1983).

Replacing correspondence evaluations by coherence ones is a move with significant epistemological remifications. It implies that the question whether thought sequences, or any other introspec-

tive reports (dreams, for example, cf. Wittgenstein, 1953, Malcolm, 1962) correspond to "real" psychological states loses much of its relevance. Whether such correspondence actually holds or not will determine neither the merit of the analysis nor its interest. If systematic coherence is found, the data and the analysis are surely of significance. This last appraisal, note, is not specific to introspective psychological studies. Following Kant (1953), western philosophy exhibits a progressive trend, which dominates the role of extrinsic considerations; in current philosophy of science the role of intrinsic considerations is clearly dominant (the methodology adopted in transformational-generative linguistics is a paradigmatic example, see Chomsky, 1965, as well as Soames and Perlmutter, 1979). Yet, in the case of introspection the epistemological considerationas are of special significance. Introspection is unique in that it is not only an instrument for the collection of data. Unlike any other tool of observation, introspection is itself an object of inquiry in the field of investigation in which it is being employed. This, while the telescope or the microscope are mere technical instruments, introspection is, in fact, a genuine cognitive phenomenon. Consequently, even if the products of introspection are nor the direct reflections of underlying thoughts, they are still manifestations of the workings of the mind. Thus, to the allegation that these might not constitute a reliable documentation of thoughts proper, we can retort that we simply do not care. Nothing would be detracted from the interest of an investigation if, instead of being characterized as the study of thought, it would be characterized as the study of introspection. Practically, the epistemological considerations noted bear no concrete ramifications which are not already implied by the analytic and interpretative considerations.

Yet, philosophically, the entire issue of introspection is placed by these considerations in a new light. The restrictions on the use of introspection now can be viewed not as mere methodological constraints implied by the shortcomings of this methods, but rather as guidelines of scientific conduct which altogether free one from

the problems traditionally associated with the method of introspection.

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LOGICAL SEMANTICS AS A RESEARCH TOOL FOR THE HISTORY OF PSYCHOLOGY

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Summary

The relevance and nature of logical semantics as a tool for research in the history of psychology is illustrated by clearing up some semantical confusion about the interpretation of the universe of discourse that behavioral and phenomenological languages of psychology seem to imply. A formal explication is proposed. It is suggested that the universe of discourse D_b of behavioral theories basically consists of events or propositions, while the universe of discourse D_p of phenomenological and related theories consists of a semantically different set of so-called 'meaningful events'. The meaning of 'meaningful events' is explained by making use of Hintikka's semantics of modal logic and propositional attitudes. Criteria for (dis-)similarity of elements of D_b and D_p are stated, which have different bearings on the notion of objectivity in both groups of psychological languages. Although the way D_b and D_p are divided into subsets by further terminological stipulations is by and large the same in behavioral and phenomenological psychology, D_b and D_p are definitely different sets; which might explain some of the terminological confusion on the basis of apparent understanding.

1. Introduction

Particularly over the last decades, history of psychology as a scientific discipline has developed a great variety of tools that enable us to develop an objective perspective on the growth and changes of psychological theories. Just to mention one of them, historiometric tools are helpful to get some exact, though possibly incomplete information about the direction that psychological thinking and writing is taking.

Another variety of tools that are relevant to the historiography of psychology is provided by the application of linguistics to

psychological language. In particular logical semantics, as a linguistic discipline, that focusses on the relationship between language and that which language speaks about, has become more and more relevant to what could be called 'theoretical psychology'. So what we would like to do is, to show in what way logical semantics in an invaluable tool for carrying out research that has psychological ways of thinking or writing as its object. Basically, logical semantics, as developed by logicians like Carnap (1956, 1968), Hintikka (1969), Montague (1969) and many others, can be applied to compare and clarify the meaning of different psychological terminologies, whether these terminologies are 'old' or 'new', strange or familiar.

In order to illustrate the way logical semantical research works, we will show its application to a couple of key terms of such different language as the languages of behavioral and phenomenological psychology.

2. The relevance of semantical research for psychology

The reason for looking into the meaning of psychological terminology is an old one; first of all language, natural, scientific or formal, is the medium in which the results of scientific investigations are formulated and by which the systematic experience of the investigator is made accessible to the present and future (scientific) community. Second, there are, especially in the social sciences, a great variety of theories and languages that seem to pertain to more or less the same universe of discourse. Although they are often related, no explicit rules of translation exist between the different languages which speak about social or psychological reality. Inasmuch as a scientific language has been emancipated from the vagueness of natural language, the nonexistence of rules of translation between them is a consequence of its *raison d'être*, which creates not so much a scientific but a social problem between layman and scientist. But between the languages (and the theories formulated in them) which are considered to be scientific or precise, the nonexistence of rules which give information like

'term X of language L_i means the same as term Y in language L_j' bars the understanding between scientists. As a consequence the results of empirical investigations, when formulated in L_i do not always 'spread' to L_j. Or, in the opposite case, if the identity of meaning (synonymy) of a term 'X' that occurs in L_i as well as in L_j, is assumed at face value - although the semantical relationships between 'X' and other terms in L_i differ from the relationships of 'X' with other terms of L_j - then the results of an investigation formulated in L_i often do spread to L_j, but wrongly. Now, if one wants to do something about this problematic situation, which in our opnion is one of the main stumbling blocks for the progress of the social sciences and the development of their theories, the obvious approach is semantical analysis of the terms which are most common in present theories. For, if we would have semantical models of the language L_i and L_j at our disposal, we would be in a position to formulate rules of translation on the basis of the isomorphy (or homomorphy) of the models, if such rules exist at all.

As empirical theories in particular are meant to refer to some domain of inidividuals, a referential theory of meaning, according to which the concept of meaning is explained by the notion of extension, is taken as our methodological basis. What we will try to do is to give some sketchy hints for the construction of a few very simple models, using the semantics of propositional and modal logic as our building blocks.

Because the semantical analysis of empirical terms of psychology is still a rather unexplored area of theoretical research, we will narrow down the scope of our investigation to a problem area that is basic to all theory construction:

a) the selection of the domain D about which a theory is intended to speak; and
b) the choice of criteria according to which two elements in D are considered to be 'the same' or 'similar';
c) and explanation of basic terms that are related to the choice or interpretation of D.

To be able to deal with this area of analysis we need some logical equipment. Expecially the semantics of modal logic appears to be of great use with respect to the interpretation of the domain of psychological theories. As the semantics of model logic and propositional attitudes are well known, we will not go into the technical explanation of its concepts here (vide Hintikka, 1969).

3. The domain (universe of discourse) of psychological theories

Psychological theories might be divided into two groups.

3.1. Phenomenalistic theories

Theories belonging to this group pertain to sense data, i.e. to events that impinge on the sensory apparatus of the organism. Or as a psychologist would qualify the phenomenalistic group: behavioristic or stimulus-response-theories. The domain D of behavioristic theories may easily be identified as a set containing stimulus events and response events. The question about the choice of a criterion for similarity of elements in D, whether stimuli or responses is usually (see Skinner 1938, Estes 1955, Bezembinder 1970) dealt with in a very interesting and paradoxical way: similarity of stimulus events s_i and s_j is determined and defined in terms of similarity of response events $r(s_i)$ and $r(s_j)$, which are elicited by the stimuli s_i and s_j. The similarity of two particular responses to two stimuli gives us the information *in what sense* the stimuli are 'similar'. This semantic dependency of the terms 'stimulus' and 'response', implied by this criterion, suggests a decision rule like the following. If both s_i and s_j are (materially) implied by the same response r_x of an organism x, then and only then are the stimuli for x or in symbolic notation:

(1) $s_i \underset{x}{=} s_j$ if and only if $(r_x \quad s_i) \equiv (r_x \quad s_j)$

Here the sign '$\underset{x}{=}$' has the meaning 'similar to x'. Put into another, more colloquial form: two (stimulus) events are similar if and only if each of them is a necessary condition for the same (response)

event. Phenomenalistic theories can be seen as theories about events, i.e. states of affairs or propositions, and the similarity of events can be explained as a relationship of implication between s and r seen ans propositions. Of course (1) is a rather crude explanation, which has a couple of shortcomings: first of all the notion of causal relationship between s and r is obviously not the same as the notion of material implication. Second, the relationship between s and r is usually conceptualized in terms of probability. On the other hand the notion that s is likely to elicit r goes back to the idea of s as a necessary but possibly not sufficient condition for the occurrence of r. Also (1) brings out the old problem of behaviorism whether it is conceptually possible that two 'different' stimuli bring about the 'same' response, given the semantical dependency between s and r. However this may be, here we will be particularly interested in the role of variable x (see under 3.3), or as it is called in behaviorism, the 'organism x' which is supposed to be the material substratum of the responses r_x.

3.2 Phenomenological theories

The phenomenological group of theories will be defined broadly here as containing not only phenomenalastic statements (in the form of propositions about events), but also 'geisteswissenschaftliche' theories and more modern hybrids like existential and humanistic psychologies.

The theories in this group seem to have in common that they all speak about reality as something that has 'meaning' to us. In these theories reality appears more or less like a partner who is speaking to us in a very personal way. Often the metaphor of a dialogue is used, but even then it is not always clear whether the concept of a dialogue is really used as a metaphor or as a descriptive term per se. Of course the concept of meaning and the related notion of a 'meaningful reality' has nothing to do with another concept of meaning, on which the semantical method itself is based,

and which is explained by means of the notions of 'truth condition' and 'sentence'. In phenomenological and related theories it is perceived reality, not just language which 'conveys meaning' to us. And whatever the concept of reality may mean, one thing at least is clear, reality does not have truth conditions. The concept of meaning as it functions in the second group of theories is closely related to the concept of 'intentionality'. In fact, it is a recurrent theme in the work of Edmund Husserl (1963), and his followers, that the meaning of the experienced world is given and constituted by the intentionality of the experiencer. Not only does the particular intentionality, which relates subject to object, determine the meaning of the object for the subject, but there is also an ontological implication: in phenomenology the existence of an intention itself amounts to the existence of an intended object, and vice versa. Taking 'intentionality' to be a relation I between subject x and object y, the meaning M of an object y for a subject x could be explained as the inverse relation of I:

$$(2) \quad I(x,y) = M(y,x)$$

Because of the ontological implication, the elements in the domain D of phenomenological theories exist only as 'meaningful objects'. Therefore, it is only for syntactical reasons that a phenomenologist, in our conception, speaks of 'y' and 'its meaning', for it is unnecessary to distinguish between the meaning of y and y as an element of the phenomenological domain. Likewise there is a tendency in phenomenological writings to identify the subject x with its intentionality as such.

However that may be, we can interpret the elements of D as meaningful objects, and the difference and similarity of two elements y_i and y_j in D can be explained as a difference or similarity of intentions towards y_i and y_j. If we want to make this criterion of similarity operative, a clarification of the logical form of the concept of intentionality is necessary. It looks like the logical form of the modal notion of 'propositional attitude', as it is semantically defined by Hintikka (1969), is a reasonable candidate.

If we take the phenomenological claim seriously, to with, that there is nothing 'outside' the relationship of intentionality, then at least all those propositional attitudes, for which a translation from a propositional 'that construction' into a 'direct-object construction' is possible, must, from the phenomenological point of view, be equivalent to intentionality. And inasmuch as 'intentionality' refers to the psychological faculty of awareness or perception, there is also an argument for the modal logician to equate the logical form of 'intentionality' with propositional attitudes. For 'direct-object' constructions with perceptual terms may be reduced to the 'perceiving that' construction (Hintikka 1969, p. 164).

Therefore I would like to propose to identify the semantics of intentionality with the semantics of propositional attitudes. 'Phenomenological meaning' may than be defined along the following lines.

Let $\Pi A_x p(y)$ stand for the conjunction of all propositional attitudes A of person x towards the proposition p(y). The proposition p(y) is a proposition about y. So 'y' is meant here as an index which relates p(y) to the object y, which is mentioned when $A_x p(y)$ is rewritten in the form of a direct-object construction.

We call ΠA_x the 'connotative meaning of p(y) for x'.
Next we identify the phenomenological meaning of y for x with the connotative meaning of p(y) for x.

(3.1) y is phenomenologically meaningful for x if and only if p(y) has connotative meaning for x; p(y) has connotative meaning for y if and only if $\Phi_{\Pi A}(x,\mu) - \{\mu\} \neq \emptyset$[1].

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1) The expression '$\Phi_{\Pi A}(x,\mu)$' refers to the set of possible worlds that are compatible with the compound attitude ΠA that x holds towards p(y) in the actual world μ.

The important phenomenological expression 'phenomenon y is meaningless' we define thus:

(3.2) y is phenomenologically meaningless for x if only if p(y) has no connotative meaning for x; p(y) has no connotative meaning for x in μ if and only if $\Phi_{\Pi A}(x,\mu) = \{\mu\}$[3].

The rule according to which elements y_i and y_j are phenomenologically similar is then stated as follows:

(4) $y_i \underset{x}{\equiv} Y_j$ if and only if $\Pi A_x p(y_i) \equiv \Pi B_x p(y_j)$[2]

Note the 'extensionalistic flavor' of this decision rule: all meaningless (and absurd)[3] phenomena are similar. The idea expressed by (4) seems relevant to the notion of meaning in phenomenological as well as *'geisteswissenschaftliche'* theories.

3.3 The semantical difference between phenomenalistic and phenomenological theories.

The difference between phenomenalistic theories about domains which contain elements differentiated according to rules like (1) on the one hand, and phenomenological or *geisteswissenschaftliche* theories about domains which contain elements differentiated accor-

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2) 'ΠA_x' and 'ΠB_x' stand for conjunctions of propositional attitudes of x towards $p(y_i)$ and $p(y_j)$ respecitvely; '$\Pi B_x p(y_j)$' is short for '$B_x^1 p(y_j)$ & $B_x^2 p(y_j)$ & ... $B_x^k p(y_j)$ ... & $B_x^n p(y_j)$'.

3) The existentialistic notion of 'absurdity' of y for x in μ may accordingly be related to the condition that $\phi_{\Pi A}(x,\mu) = \emptyset$, which amounts to inconsistency of the propositional attitudes involved in ΠA_x towards p(y) or to inconsistency of the argument p(y) of ΠA_x.

ding to (4) on the other hand, has its methodological consequences. These consequences are reflected by the different positions of the subject variable x in (1) and (4). In (1) the similarity is dependent on a relationship between stimulus events and response events of x. These events are observed by an *implied observer* who is or may be *logically independent* from x. In (4) the similarity is dependent on a relationship between events and attitudes of x towards these events. Moreover, the implied observer (experiencer) of the attitudes is here necessarily the same as the subject x. So the 'objectivity' of the similarity of y_i and y_j in (4) is not the same as the 'objectivity' involved in (1). In fact, the possibility of mutual independence of 'subject and object' which is maintained in (1) is broken down in (4). This, of course, might lead us to a re-examination of the methods of investigation that are used in phenomenological and *geisteswissenschaftliche* psychology: the so called method of *'Wesenschau'* and the method of *'Verstehen'* respectively. It seems that the 'Verstehende Methode' is based on a naive theory of meaning in which 'meanings' are seen as 'objective properties' of things. In the light of our interpretation of phenomenological meaning, the activity of 'Verstehen' seems to amount to the activity of recollecting or bringing back into consciousness forgotten propositonal attitudes which one has towards the state of affairs one tries to understand *(Verstehen)*. This activity boils down to a kind of selfexamination. The phenomenological method of 'Wesenschau' and its 'epoche' can be seen as a psychological method that aimes at recalling the meaning of things, while (temporarily) putting aside (=*epoche*) all those intentionalities which make situations meaningsless (3.2) or ununderstandable. When 'Wesenschau' is applied between people the method consists of a mutual process of communication, consisting of mutual acts of opening (*epoche*), which leads to 'mutual understanding', in the sense of being able to have and experience the same intentional or meaningful relationship ΠA towards the proposition involved, as the person one communicates with.

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PSYCHOLOGY AND THE REIGN OF TECHNOLOGY*

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"Den Himmel überlassen wir
den Engeln und den Spatzen"

Heinrich Heine

I *A program, a congress and a personal statement*

The *Oxford English Dictionary* tells us that a congress is an assembly or a conference for the discussion or settlement of some question *or* a periodical series of meetings of some association or society of specialists. The organizers of this conference and their American brethern usually have the latter purpose in mind. I, for my part, would like to settle one or two questions concerning the history and historiography of psychology. In my contribution to the first European meeting of *Cheiron*, I will broadly sketch a program for the reconsideration of the place of psychology in an industrialized society. In rough outline I propose to argue that from the beginning of this century on, mechanistic ways of thinking have penetrated into the heart of the science of psychology. This 'mechanization' of the mind has reversed the relationship between theoretical and applied psychology. From the First World War on, applied psychology, usually called *psychotechnics*, has pushed out theoretical and general psychology at an ever increasing speed. From the Second World War on, psychology has become a *social technology*, which together with a handful of other social technologies, contributes to the control of individual's lives.

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* Parts of this paper were earlier presented in talks at Queens College, CUNY, New York, and Swarthmore College in 1981 and during an invited lecture at the Free University, Amsterdam in 1982.

Looking back at the '50's and the '60's in the Western world, when mechanization and automation took command, I feel that one important social issue has not received enough attention. The point I have in mind is that more industrialization leads to an *increase of societal conflicts*. I will not speculate about human nature with regard to this finding. I shall not point to an old socialist saying 'desire has set us on fire'. The only thing I would like to do, is call for a new sobriety of which simple human values and the appreciation of small goods forms a part.

II Transformational Contextualism

Ideally, dear friends and colleagues, I would like to evaluate the development of 20th century psychology from the standpoint of *transformational contextualism*. Transformational contextualism conceived as a theory of the growth and spread of psychology as a science, a profession and a social technology, *and* a conception of social reality, was developed, in close cooperation, by Thom Verhave, CUNY and me, over the last ten years or so.[1)] In our view, social conflicts form the source of psychological knowledge and one way or the other, the actions of the psychologists contribute to the maintenance, intensification or the melioration of human conflict in a stratified and hierarchical society like ours.

From this vantage point then, I would like to look at the development of post World War II psychology as an intertwined transformation of social conflicts, theoretical reflections - scientific theory formation - and psychological practices, i.e. the actions of the psychologists. Thus seen, three approaches towards the historiography of psychology are combined into one model:

societal history - *Gesellschaftsgeschichte*

+

the analysis of concepts and theories - *Ideengeschichte*

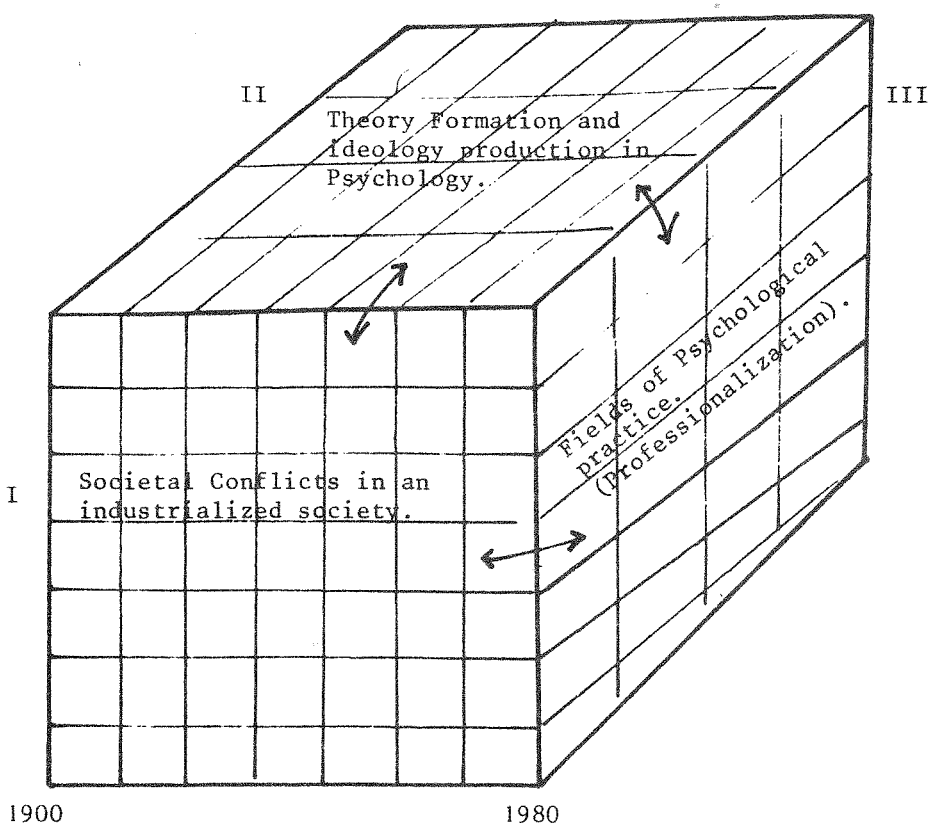
+

cultural history - *Kulturgeschichte*, history of mentality

In line with the historiographical considerations just mentioned,

Figure I

Transformational contextualism and the development of 20th.C.Psychology.



Annotation.

I = Societal conflicts in an industrialized society.

II = Theory formation and ideology production in psychology.

III = Fields of psychological practice.

↔ = Mediating links.

TC, in its analysis of the societal context of any historical complex, employs three planes, each of which may itself consist of as many subdivisions as are needed or useful.

As far as our three-dimensional model is concerned, imagine a cube like *Rubik's* (see figure). Fortunately, we need only three sides, each of which is divided into a number of small cubes. Each of the cubes of the *frontal* surface represents a distinct and specific *societal conflict*. Since "strife is the father of all things", our frontal plane indicates our a priori assumption, *viz.* that human conflict forms the source of theoretical and practical *psychological knowledge*. The top surface of our cube represents the level of theorizing in psychology and lists the appropriate aspects of the transformations taking place in psychological theories of any kind. Here, matters of ideology and legitimation, in sofar as they influence psychological theory formation, are taken into account. The lateral plane of the *TC*-cube represents the actions of the psychologists, more specifically the development of the fields of psychological practice. Thus, it is closely related to the outcome of the interacting transformations represented by the other two planes.

The top surface represents the ever-changing metaphorical, symbolical and ideological *discourse*, as this can be related to the perennial societal conflicts of the frontal surface. The lateral surface represents, in each specific field of psychological activity, the *resultants* of the interactions of the ongoing transformations represented by *all* three surfaces.[2)]

The existing histories of psychology usually do not make a distinction between the development of psychology as a science, a social technology (a so-called 'application'), and a profession (see, however, Van Hoorn & Verhave, 1977). Moreover, the way psychology has influenced 20th century Western conduct and mentality - Philip Rieff, Serge Moscovici and Christopher Lasch notwithstanding - has not been studied to a satisfactory extent. Generally speaking, the fundamental importance of the cleavages of World Wars I and II as the societal impetus of the institutionalization of the

fields of psychological practice has not been recognized. Here we may think of psychotherapy, industrial psychology and ergonomics, educational psychology, developmental psychology and the psychology of testing, which still has to be described and evaluated.[3] In this respect, the aims of *TC* are:

(i) to determine the mediating links - *Vermittlungsglieder* - of the interdependency of societal conflict, theoretical psychology and psychological practice. Karl Marx, Karl Mannheim and Max Scheler have paved the way for this approach.[4] Well understood, psychology *emerges* at the intersections of I, II and III

(ii) to search for the societal roots of theoretical and practical psychology

(iii) to stress the *temporalization* of social and mental processes such as labor and to systematically investigate the *Nacheilung* of theoretical psychology in comparison to the development and the spread of the fields of psychological practice[5]

(iv) moreover, *TC* tries to describe the variants and constancies in human experience and behavior[6]

(v) fifth and finally, then, *TC* expresses the program that psychological knowledge may lead to a qualitatively different *self-knowledge*, and that the points just mentioned may contribute to a *healthier way of life* in a post-industrial society. Health being, according to Descartes: "...sans doute le premier bien et le fondement de tous les autres biens de cette vie".[7]

III Post World War II psychology and the politics of industrialized societies

What I would like to bring out, is that psychology is a very recent phenomenon and that World War I and World War II - e.g. mechanization and automation of the production processes - form breaking-points in the transformation of the 20th-century psychology.

Why?

As everyone present in this room knows, up to the beginning of this century, there was no societal psychology to speak of. Around the time of the First World War, a handful of self-styled psychologists decided that the newly emerged scientific psychology had to become an *intervention-oriented* 'science', i.e., in my view, a *societal psychology*. This is to say that labor -, educational -, selection of personnel -and mental health problems were turned into fields of psychological practice, a development which was to bring with it all the inescapable ethical and moral problems of every applied science. More specifically speaking, a *social technology* is built up, in analogy to industrial technology as this, characteristically, blossomed forth during the 2nd Industrial Revolution. During this transitory stage of Western development, technology pushed out science to take the lead in the construction of the material world. Ever since then, science, especially pure science, *reine Wissenschaft* and her twin-sisters epistemology and philosophical anthropology, have lost ground to the engineers, civil, social and otherwise who technocratically shape and control our society. This also means that *the nature of scientific knowledge* in the social sciences has drastically changed, particularly after World War II and the emergence of the Welfare State.[8)]

During and after World War II, technology, military and otherwise, takes the lead in shaping the production processes. In the same period, psychologists came to the fore maintaining that their experimentally proved, *technical* body of knowledge could be of great help in building industrial society and the Welfare State. The technology of production should solve the problems of scarcity; we, the psychologists, will take care of people's *individual lives*, and if necessary, contribute to the social ordering. In my opnion, it is precisely this focussing on the person as an a-historical, individual being in clinical, educational and industrial psychology, which has led our science into a blind alley. Summarized in one brief sentence: the unholy alliance of psychologists with the national institutes of health, with the requirements of profit-ma-

king in industry and with the overall structure of education, have turned us into servants of power, to use Baritz's expression. I seriously doubt whether clinical psychology, the teaching machine and programmed instruction, behavior therapy and behavior modification, scientific management and the human relations movement in industrial psychology, and indeed, even psychoanalytic psychotherapy as a consolation prize for the embattled (upper) middle class, have genuinely contributed to the emancipation of the people concerned. In my opinion, the social technology as developed by scientific psychology, has rather lent itself to the oppression of people. The control of behavior and the emancipation of human beings are at odds in an economic order, which is governed by the strife after ever-increasing production. In Dutch we have a saying: "Wiens brood men eet, diens woord men preekt", which can properly be translated into English by: "Don't bite the hand that feeds you".[9)]

IV In conclusion

Dear friends and colleagues: Of course I know about the wonders psychology has worked in the lives of people. I also know about the impressive improvements wrought by our ergonomic colleagues in the outfitting of cockpits, military and otherwise. And, certainly, I do not close my eyes to the fact that at least 50 percent of all psychotherapy yields one success or the other, whatever 'success' may mean in this context. What disturbs me is the disconnection between psychology, politics and ethics. What bothers me is that psychology in the 50 odd years of its societal existence has produced much more *Herrschaftswissen* than *Bildungswissen* (to borrow Max Scheler's terms). The almost undisturbed reign of material and social technology - there is one and only one *technical* solution to every problem - has prevented us from focussing on individuals as historical, social and ethical beings, who as secular men and women want to be happy on earth.

Freud's second chapter of *Civilization and its Discontents* (1930a) deals with the anthropology of happiness. One of the prin-

cipal questions Freud puts before us is to ask how much religion, art and science can contribute to human happiness. First, however, says Freuds, we have to investigate why people want to be happy at all. Here, the principal issue seems to be: what is the goal of human life? In this connection Freud does not hesitate to maintain that:

a) the idea of a goal in life stands or falls with the system of religion

b) from a psychoanalytic point, it is simply the program of the pleasure principle which directs the goal of life

The trouble with this position is that secular and non-analytic *angehauchte* women and men would find themselves at a loss when it comes to answering the questions of life's goal and human hapiness. Freud summarizes his anthropology in *Civilization and its Discontents* in a misanthropic passage *par excellence*: "...die Absicht, dass der Mensch "glücklich" sei, ist im Plan der "Schöpfung" nicht enthalten". From a contextualistic point of view I would like to counter this by saying that we know nothing about creation's plan (either with or without parentheses). In our secular and technological world, the proper study of psychology should be the promotion of the happiness of people. Thus one could end by quoting Goethe's *Faust*: "O glücklich wer noch hoffen kann, aus diesem Meer des Irrtums aufzutauchen!"[10)]

Annotations and References

1) The first extensive treatment of what Verhave and I have later labelled "Transformational Contextualism" can be found in my *As Images Unwind*. Ancient and modern theories of visual perception. University Press, Amsterdam, 1972, pp 16-39. Most of the ideas put forward in the *Images* are of an *internalistic* nature: "Contextualism simply means trying to understand the past for the sake of the past". "Psychology ... partly belongs to the domain of art and literature and partly to the natural science field. Thus, psychology is partly a hermeneutics of the mind and partly a science of the mind". " ... I propose to

let the history of psychology proper deal with the last 75 years of psychology's development. It is my opinion that the greater part of the history before 1900 belongs to historical psychology, because psychology as it flourishes nowadays, is a very recent invention" (1972, pp. 24, 27 and 31). In 1975, I became a member of the special research unit *Technics, Technology and Society which was started by Prof. Kees Bertels. Discussions with the members of TTS* opened my eyes to the value of social and economic history, while I had already started a Marxist introduction to the history of the social sciences by teaching a course (from 1974 on) in which I used Bernal's *Science in History*, vol. 4. The fruit of these new insights was expressed in Van Hoorn & Verhave: "Socio-economic factors and the roots of American psychology: 1865-1914. An exploratory essay". In the meantime, Thom Verhave was digging into the importance of the concept of *temporalization* for understanding social history and the history of science. See Verhave & Van Hoorn: "The temporalization of ego and society during the nineteenth century. A view from the top". Drafts of both essays were finalized while I stayed at Thom's home in april 1976. The final texts were published in Rieber & Salzinger (Eds.) *The Roots of American Psychology*. Annals of the New York Academy of Sciences, vol. 291, New York, 1977. Verhave and I were engaged in lengthy historiographic discussions. Out of this came an unpublished analysis of the concept of 'transformation' by Verhave. In 1977, Sacha Bem joined the staff of the Leiden Psychological Institute, which has resulted in a fruitbearing division of labor between him and me. Sacha teaches the introductory history of psychology course, from Descartes to the beginning of the twentieth century, while I teach psychology's developments from the end of the nineteenth century to the present. From 1978 on, my *research* interests have moved away from the analysis of concepts and theories towards the societal significance of the fields of psychological practice in the 20th century. Together with Ben Vincent

and Thom Verhave, I am preparing a book about this subject, which is now in its final stage.

In the autumn of 1981, Prof. Luciano Mecacci of the Rome *CNR* Psychological Institute invited Prof. Barbara Ross and me to partake in a special workshop: "Problemi della recerca storica-critica in psicologia", supported by *CNR*. During this workshop, I presented our views in a paper entitled, "Transformational contextualism as a general model for the development of psychology". Additional information was presented with reference to psychoanalysis and behaviorism. In this paper we find the appearance of the *TC* three-dimensional "box" or "cube" as this was worked out by Thom Verhave and me. An application of this three-dimensional - societal conflict - theory formation - action - model can be found in my contribution to the *Pongratz Festschrift* (G. Bittner, Ed.): "The cultural context of psychoanalysis" (Toronto, 1983). In the latter paper I have tried to describe several mediating links (*Vermittlungsglieder*) between the socio-cultural context, psychoanalytic concepts and the emergence and spread of psychoanalytic psychotherapy. "It is *le charme discret de la bourgeoisie* (Bunuel), which as a social-psychological process constitutes the new, repressed unconscious of the 20th century. The vices and virtues and the attitudes towards sexuality of the bourgeoisie, form the start of a secular conflict psychology of every-day life". "In conclusion, the shared-life-style of patient and doctor forms a mediating link between societal processes and the emerging profession of the psychotherapist. In their turn, the radiating effects of the psychotherapeutic profession constitute a mediating link between scientific psychotherapy and the proto-professionalization of particular social groups. Thus conceived, the discreet charm of the friends and supporters of psychotherapy serves, in part, to explain the cultural significance of psychoanalysis in the twentieth century". (Pongratz *Festschrift*, 1983, pp 230-241).

A further elaboration of *TC*'s concept of temporalization can be found in Verhave & Van Hoorn: "The temporalization of the self in a technological society" in Gergen & Gergen (Eds.) *Historical Social Psychology*, Erlbaum, 1983.

2) Since Rubik's cube consists of 3x3x3 small cubes, the total number of possible transformations exceeds 4.3×10^{19}. Such a device seems sufficient to serve our goal, *viz.*, to represent the interactions and interdependencies of societal conflict, theory formation and the actions of the psychologists.

3) The emergence and spread of the fields of psychological practice are described in Van Hoorn's, Vincent's and Verhave's forthcoming book on the societal development of psychology in the 20th century, of which earlier versions were published by the Leiden Psychological Institute in 1978, 1980 and 1982.

4) See, e.g., Scheler's *Die Wissensformen und die Gesellschaft* (1926, Ges. Werke, vol. 8, Bern, 1960), which deals extensively with his sociology of knowledge. The value of Scheler's contributions to this field has hardly been touched upon. See H.J. Lieber: "Bemerkungen zur Wissenssoziologie Max Schelers" in *Max Scheler im Gegenwartsgeschehen der Philosophie* (Paul Good, Ed.). Bern, 1975, pp 225-239.

For Mannheim's position see, e.g., "Historismus" (1924), *Ideologie und Utopie* (1929) and *Wissenssoziologie* (K. Wolff, Ed.), 1964. Best known is Mannheim's *Mensch und Gesellschaft im Zeitalter des Umbaus* (1935; 1940 Engl. transl.).

5) Lovejoy's concept of temporalization plays an important role in the theoretical framework of transformational contextualism. See Verhave & Van Hoorn, 1977 and 1983 (see also note 1).

In a recently published paper, "Wundtian psychology and the psychologies in post-industrial societies" (*Revista de Historia de la Psicologia*, 1982, Vol. 3, Núm. 2, pp 115-132), I have pointed to the significance of the intrusion of newtonian, uniform and linear time into the conceptual framework of theoretical psychology. I think that such a process developed

all of a sudden at the very end of the eighteenth century and that hence from 1800 on, we can see the simultaneous rise of a general and theoretical psychology (Herbart, Lotze, Wundt) and a *differential* psychology (Mesmer, Gall/Spurzheim, Galton).

By the end of the nineteenth and the beginning of the twentieth centuries, we see the rise of the fields of psychological practice and from the time of the First World-War on, we may note an increasing *Nacheilung* of theoretical psychology in comparison to practical psychology. In this respect, the psychology of testing would be an excellent case to investigate: first came the war, then the testing, and finally its theory!

In the paper just mentioned, I have launched the idea of *accelerated Nacheilung,* which implies that after World War II, especially as a result of the unexpected growth of clinical psychology, theoretical psychology is lagging behind the developments in practical psychology at an ever-increasing speed.

6) Here *TC* explicitly makes contacts with historical psychology and historical sociology and anthropology. See H. Peeters' *Historische Gedragswetenschap* (A historical science of behavior), Boom, 1978.

7) Descartes, *Discourse*, part 6. The context of this quote is truly fascinating and of great interest to the further development of a contextual psychology. It contains the principles of Descartes' notion of praxis, his famous idea of people as *maîtres et possesseurs de la nature* and his Epicurean stance of enjoying the fruits of the earth. Moreover, Descartes links the preservation of health with the betterment of mankind and the attainment of longevity with the progress of medicine. Drawing the logical consequences from the Cartesian position, I have called for a preventive medicine, preventive architecture and a *preventive psychology* in the paper mentioned above.

8) The idea of a social technology can be found in Weber, Mannheim, Huxley, Zamiatin, Ellul, London, Marcuse, Packard,

Skinner, Sennett and many others. Obviously, theoretical psychology has hardly touched upon the ethical and moral problems inherent to psychotechnology. The reasons for this neglect are not hard to find. The almost exclusive concentration upon *the individual*, as the proper object of study in psychology, has brought about the near complete divorce of theoretical psychology and the ethical, social and moral concerns of an industrialized society. In this respect there is a grotesque *Nacheilung* between developments taking place in theoretical psychology and the real needs and deeper concerns of the people of our time.

In the summer of 1982, a group of doctoral students (S. Vermeulen, K. de Mik en B. Vincent) of the Leiden Psychological Institute has compiled a bibliography of literature on social technology as this pertains to military control, propaganda, labor, advertising, elections, welfare work and deviant behavior. This bibliography may be ordered from the secretary of the Vakgroep Theoretische Psychologie, Psychological Institute, University of Leiden, Hooigracht 15, Leiden, The Netherlands.

9) I am aware that my text indicates my disappointment about psychology's idleness in the technological wilderness. Here I would like to only shortly mention a number of friends and colleagues who have expressed similar and other feelings of dissatisfaction with psychology's course:

- Kenneth Gergen, 1973, 1976, 1979, 1980, etc.
- Russell Jacoby, 1975
- James Hillman, 1975
- Gordon Westland, 1978
- Klaus Riegel, 1978
- Seymour Sarason, 1981

10) With reference to the teneur of my presentation, the immediate context of the Faust quote seems to lead us zu den Sachen selbst:

"Wenn du, als Mann, die Wissenschaft vermehrst,
So kann dein Sohn zu höhrem Ziel gelangen...
Was man nicht weiss, das eben bracht man,
Und was man weiss, kann man nicht brauchen".

What I do not yet know is, how a psychology unaffected by whatever christian or jewish religious ideas and prescriptions, would look like. In this sense I make a plea for the construction of a secular, humanistically oriented anthropological psychology, which lies *diesseits* of good, evil and the pleasure principle.

Still, the call for a thoroughly secular psychology does not imply that there would be no room for a religiously inspired psychology. On the contrary: "Du choc des sentiments et des opinions la vérité s'élance et jaillit en rayons!" And since transformational contextualism is a relativism pur sang, by slightly twisting Frederick the Great's words, one might say: In meinem Staat kan jeder selig werden, chacun à son facon.

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MEDIEVAL-RENAISSANCE EPISTEME AND A CONCEPT OF PERSON

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Summary

This paper attempts to present the origin of a concept of person.

Since the Middle Ages, the term person has meant an individual, autonomous and reasoning being. At that time, the concept of person was associated with Divine Persons and with the concept of dignity. A human being was termed 'person' only because of connections with God.

In the Renaissance humanism the human being, his intellect and individual freedom was of key value. However, it has failed in the discovery of the basis of human autonomy and dignity, different from that of the Middle Ages. The main ideas about relations between man and the Universe, and God, basic epistemological and ontological questions and solutions are the same in the Renaissance and in the Middle Ages.

Introduction

According to Michel Foucault[1] every age is characterized by an 'episteme' a largely unconscious world view providing the basis for all forms of knowledge or social cognition, during that period. The Tartu School[2] assumed that this unconscious world view is the result of properties of a particular culture and comprises a structure of social concepts like categories of time, space, causality and human being, which together make up the cultural model of reality.

All products of a culture of any time: customs, religion, ethical language, philosophical systems, the language of formal sciences, are the objectivization of the cultural picture of reality.

These products, called social semiotic systems by the Tartu school, could be studied by social and behavioral sciences in order to reconstruct the social picture of the world.

The history of Western culture comprises several varying periods, which display an evolutional change of the picture of the world. It seems that the world views of different periods are incompatible, and the evolution of social cognition is not the gradual accumulation of knowlegde; it is rather the history of the revolutions in social cognition.
The cultural picture of the world is not entirely closed and compact, it contains the concepts of previous or next 'episteme'.
This is probably the main reason of evolution of social cognition.

The Renaissance period is according to many authors a typical example of the revolution, both in social structure and social cognition. On the other hand, other authors[3] emphasize the similarities between the Middle Ages and the Renaissance picture of the world. The solution to this problem can probably not be separated from the point of view of an author and his decision as to which revolutionary aspects of the Renaissance should be deemed most important.
To understand the present, we must understand the past. This truism has a fresh meaning in the works of Kuhn[4], Foucault[5] or Rorty[6].
It is probably necessary to study the history of ideas not in order to avoid the old mistakes, but in order to understand our modern concepts or behavior.

Contemporary fortune telling is based, in a manner of speaking, upon a rational, however very old, rule of 'universal analogy'. In this case, the deck of cards is a model of the Universe and because of this it can reveal the hidden aspects of this Universe, particulary in the past, present and future of a particular person.

The same rule of universal analogy was the base of the mnemotechnic invented in ancient Greece and developed in the Renaissance. The art of memory which had been founded on the false, from the modern point of view, assumption that the individual mind resembles the whole Universe and the act of remembering is analogical to an act of ordering things in physical space, was a sufficient and useful method allowing a large quantity of material to be remembered[7].

From the author's point of view, it would be useful to investigate historical changes of the concept of human being and other concepts connected with this, such as individuality, the individual mind, psychological unity and freedom.
This kind of investigation can help to understand our modern concept of mind, consciousness, or free will.
The ambiguity of these concepts is, perhaps, the result of the fact that they are based on an anachronistic world view. Besides this, it seems that it is not quite clear how many revolutions have happened.
This paper attempts to reconstruct the Medieval and the Renaissance concepts of human being and other concepts connected with these.
For this purpose it is necessary to analyze conception, popular in these ages, about relations between humans and the Universe and the place of an individual in society and the world.

The Concept of Individual

The concept of soul and the relations between the soul and the human body was a central problem in Medieval thinking.
Generally, there were two main approaches: one contending that the soul had been understood as a form of the body, and the other, stating that it was connected with body substance.
The soul is the main part of a human being because it is connected with God. This assertion is common in the Medieval period. But questions like: 'How strong is the connection between man and God''; and 'Can man be a thinking being without God's help?'; 'Is the soul equivalent to man?', seemed complicated for Medieval thinkers.
Many philosophers followed Plato and thought of the soul as a spiritual substance making use of a body, but which is in itself self-sufficient, and after the death of its body it could survive and live a life of its own.
Other philosophers, including Thomas Aquinas, followed Aristotle and assumed that the soul is the form of a body, it is not a separable substance[8].

It seems that Medieval philosophers had to choose between the immortality of the sould and the substantial unity of man, or, in other words, between the Platonic and Aristotelian traditions.
The assertion that the soul does not fully constitute an individual being, and an individual itself is a 'compositum' of soul and body[9]; together with the assertion that the body is the rather worthless part of man, resulted in a very ambiguous attitude towards individuality during the Medieval period.

The Renaissance humanism was above all a manifestation of individualism. The Renaissance thinkers tried to discover human truth, rather than divine truth and found secular values rather than religious values. Man, his individual freedom, and his intellect were of key value in Renaissance humanism. This humanism was an attitude rather than a coherent philosophy. The Renaissance philosophy has an eclectic character[10]; there is not a single first-rate philosopher between Ockham and Descartes.
The general conception of the Universe, the place of man in this Universe, was the same as in the Middle Ages. Man was perceived as consisting of two parts: body and soul (mind). The human body is a microcosm of the physical world, the mind is a microcosm of the invisible world. Man is at the centre of the Universe because he is between the physical and spiritual worlds, all of his properties are the result of that position.
The Renaissance attitude of exploration and activity contradicted, however, the Medieval conception of man as a passive element in a hierarchic and fundamentaly static social structure. The new economic structure of the Renaissance was favorable for an active, enterprising, ingenious and bold individual. Renaissance man was the initiator and creator of his fortunes. Humanists of this period stressed the need for a broad education. Renaissance man noticed human potentialities as the necessity of self-development. The inidividual personality could be developed by the extension of dimensions of experience; this means that the scope of human activities should be enlarged.

Self-realisation through active involvement in any human and possible activity also meant the active participation in social life.

Individualism and Human Cognition

Activity, exploration and critisicm form the early beginning of any individualisation process. Also, the empirical and experimental approach seems conformable to the attitude of exploration.
Medieval man based his ideas on authority rather than on his own experience. Medieval philosophy had a mostly systematic and rationalistic character. There were, of course, several exceptions; the best example is the philosophy of Roger Bacon, who pointed the way to Newtonian physics and the further development of positivism. Renaissance thinkers did not care about philosophical systems, they preferred the experimental approach and the usefulness of any science.

However, Platonism was popular in Renaissance philosophy as well as Aristotelianism[11]. The first was the basis for later rationalistic tendencies in philosphy, the second for later materialism and empiricism. It seems, however, that the empirical approach is more characteristic for the Renaissance period.
Materialism en empiricism were expressed in Renaissance art as well as Medieval art expressed idealistic and universalistic interests. It is commonly held that Renaissance art differs in character from the art of the Middle Ages. Renaissance man appreciated and enjoyed a secular life; his art, even that of a religious theme, has a secular and sensual character. Furthermore, the Renaissance painter like the modern painter (realist) abode by the same principles of geometric perspective. The perspective of Medieval painting[12] has a different character; the objects painted are represented from several different ponts of view and the perspective of whole painting has a dynamic character. But is is not true that the principles of geometric perspective had been discovered in the Renaissance. According to Uspenskij[13], the composition of Medieval painting had a semantic rather than geometric order. It seemed necessary for Medieval painters to paint all important things, and

because of that they did not care about geometric perspective. However, they predominantly used angular perspective when representing unimportant elements of a painting.
Simply speaking, Medieval man painted what he knew about the objects painted; Renaissance man painted what he saw from his own individual and motionless point of view. This is the reason for the different perspectives of Medieval and Renaissance painting. Anyway, the Renaissance painting reflects an individual and empirical rather than a universal and rational attitude. "Wisdom is the daugther of experience" - these words of Leonardo da Vinci were conformable to both his artistic and scientific activities.
The foundation of science and knowledge upon experience rather than reason seemed during this period the way to certain knowledge.

Renaissance man knew that his logic and thinking could be delusive. But is was to be for another epoch to discover that both empirical and rational knowledge are uncertain.
The scientific discoveries of Copernicus and Kepler were the result of this experimental approach. But also the magic and alchemy of the Renaissance were products of this same attitude.

The Idea of Individual Autonomy and Dignity

Accordong to Thomas Aquinas, man is an intellectual substance (soul) which can be united to the body as its form. Cognition is the most important human property. In Aquinas' philosophy human cognition is relatively independent of God. But according to St. Augustine, knowledge of truth is impossible without illumination by divine light[14].
The connection between human intellect and superindividual divine substance (God) is closer according to the system of Averroes.
Averroes followed Artistotle and distinguished between active and passive intellect. The intellect of the human soul is entirely passive and has a potential for knowledge actualized by the active Intellect (or agent intellect). This agent intellect is, according to Averroes, outside the human soul; it is a superindividual divine substance[15]. The comparison of the philosophical systems of Aver-

roes and St. Augustine, on the one hand, and the system of Aquinas, on the other, reveals an evolution in the Medieval mind: Man, or rather his intellect, was gradually understood as more independent of God and more individual.

The most developed stage of this process is represented by the ideas of Duns Scotus. For him every human is an individual, a unique being, and deepest loneliness is a human property. Because of his loneliness a man is a person, that is, an individual autonomous and reasoning being. Human love, human autonomy spring from man's loneliness and individuality[16]. The philosophical system of Duns Scotus is a polemic with the tradition of Averroes, which was more typical for the Middle Ages, in which the most important element of a man, his reasonable soul, is a part of the universal (superindividual) intellect.

Medieval thinking contains many contradictions. On the one hand, man should be an autonomous reasoning being capable of choosing between good and evil; on the other hand, man and his thinking depends on God's illumination, and his most important and valuable part - the reasoning soul - could be a part of a divine intellect. However, the Medieval period seems to be an important step in the complicated history of thinking in Western culture.

The main discovery of the cultural tradition, which was born in the Mediterranean area, is the concept of 'person'. The elaboration of this concept took several centuries in the history of this culture. The term person (lat. persona) meant a mask, then a role in religious ceremonies (Etruscian civilization), next a legal entity (a person in the legal sense, Rome), and finally it became the synonym of the true nature or essence of man, his spirit, his self, or his consciousness[17].

In Medieval Christian philosophy and religion the term person referred to Divine Persons. Less frequently this term was used in description of a human being (St. Thomas Aquinas. Sum. theol. I, 29, 3-4)[18].

To the Medieval mind, due to his soul, a human being is of value, but he is of value only because of his connection with God. In the

Medieval personalism God is the only source of human dignity[19]. Every human being is a person - "an individual substance considered as possessing a certain native dignity of its own"[20].
In the Renaissance philosophy, as in that of the Middle Ages, to be a person meant to be reasoning and independent, to be able to direct oneself and to be of value for oneself and for others.
The Renaissance thinkers tried to found secular values; according to them, human dignity depends on human intellect[21] and the special position of man in the Universe (the Center of Universe).

The modern concept of person (which has been elaborated in post-Kantian philosophy) is associated with human individuality and subjectivity, human consciousness; in other words, with the psychological aspect of a human being.

Medieval-Renaissance Episteme and the Next Development of Social cognition

The problem of autonomy, individuality, human dignity, the central problems of Renaissance humanism were noticed and investigated in the Middle Ages. For Medieval thinkers, human dignity is the reflection of God's magnificence, human wisdom is possible because of its connection with God's supreme wisdom, and human autonomy (and dignity) is possible because of God's will. The eclectic Renaissance philosophy was looking more for human than divine truth, but in the end it was impossible for this epoch to discover a basis of individual autonomy and dignity which would be different from that of the Middle Ages. In both epochs individual autonomy was due to something beyond the individual.

The surface structure of Renaissance thinking may appear very different from medieval thinking. But the depth structure, the main ideas about relations between man and the Universe, primarily unconscious epistemological and ontological basic questions and solutions, are the same in both epochs.
Up to Descartes' days, the soul had been often described as a special subtle kind of matter, and thought was understood as a movement of mind-atoms[22].

According to René Descartes, man's nature is basically separable into two kinds of reality: the mental and the physical. Matter is extended, while soul or mind is not extended, it is a rational not a spatial reality. The dualism of Descartes is a conception beyond that of the Medieval-Renaissance episteme. Descartes, thus opened the new era in philosophy, made the revolution in thinking which consists in the "invention of the mind"[23].

Descartes' dualism is also the beginning of a mind-body problem, which according to some authors (e.g. Eccles[24]) seems to have remained unsolved up to now. Despite the fact that according to other authors (e.g. Ryle, Armstrong[25]) the mind-body problem is not a real philosophical problem, this idea still exists in our ordinary thinking.

The dualism of Descartes and the opposition of spiritual and physical realities was the basis of the concept of subjectivity. Subjectivity seems to be the main category of post-Kantian philosophy[26] and ordinary thinking of the XIX and XX centuries. The literature of Romanticism expecially celebrates the subjective experiences of the individual. In this period the individual attains value in and of himself, and the Romantic artist views himself as a creator equal to God.

In ancient Greece and in the Middle Ages an artist was considered as a craftsman rather than a creator. This was due to the underlying notion of beauty. Unlike in later periods, beauty was considered to exist objectively and thus, the artist's role consisted in its imitation or discovery. This has changed in the Romantic and, expecially, modern notion of art, where it is viewed as a matter of creation.

Romanticisim does not found key humanistic values (like: truth, right, beauty) on any universal or divine order. Since Romanticism and Kantian philosophy an individual is of value only because of himself.

One can say that the Medieval-Renaissance period is an epoch of elaboration of the conception of man as a person. Man as a person though, was not conceivable (could not be comprehensible), especially for Medieval man, without the idea of God.

Modern humanism has its origin in late Medieval and Renaissance humanism but it is founded on a different picture of the world.

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HISTORICAL PSYCHOLOGY
THEORETICAL AND METHODOLOGICAL ASPECTS
A CHALLENGE

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1. *Introduction*

Modern behavioral sciences study human behavior mainly within the dimension of their own time. Human behavior is studied in molecular time units, or in wider time units of short or medium duration. The borders of one's own time, therefore, are hardly ever crossed. Shortly after its beginning, psychology defined its task in such a way as to make it impossible to discern to what extent human behavior is included in a historical context. Modeling itself after the natural sciences and a positive methodology, psychology gave preference to the study of human behavior, preferably divided into subcomponents, as detached from its historical surroundings. Originally finding its material and object primarily in history, retreated more and more to the field of actuality. The complexity of the present gradually became too much of a challenge to trouble about the past. For this reason, research of human behavior from deep in the past, including the research of behavior forms of long duration and changes of behavior in the long term, was and still is generally disregarded.

Naturally, this limited perspective had consequences with regard to the view of the object that one studies, human behavior, and the terms, theories and methods that one desings and employs to describe and investigate the object. First of all, an indentation is normally made in the time levels in which people are included, and the wide stream of their history is narrowed to the here and now. This distracts attention from the historicity of people, the continuance and functioning of their old behavioral patterns, the historicity of behavioral determinants, and the processes and

factors which cause these changes or constants and with which they are connected. Above all, this onesided accentuation removes any possibility of discerning the interaction and mutual dependence between the present conditions and the more permanent structures or organizational principles.

2. *A-historical views and concepts*

People are hardly aware, of the historical character of their existence. They primarily perceive themselves as acting out of a permanent identity which remains constant in all circumstances. The opinion that people do not change, or at least do not 'essentially' change, can also be found in psychology. One finds this in classical psychology, where psychological processes are seen as a manifestation of an intellectual inner life, or as a natural function of brain tissue. In both cases, the conception is of an a-historical kind (Luria, 1971). Another example of an a-historical view in psychology is to be found in the personality concepts which belong to the so-called trait theories. In these theories, personality is seen as a structured cluster of traits, or inner behavioral positions, which determine certain forms of behavior in divergent situations, independent of external condtions. The situation is left out of consideration, due to the concept that many situations are functionally equivalent against the background of a certain trait-structure. By these theories, the individual is truly lifted above his situation (Hettema 1967: 618-641). Petrification of internal processes and functions, as well as their substantiation towards traits, also leads, therefore, to a lack of appreciation and a denial of the historicity of behavioral determinants and to the opinion that situations are characterized by rigidity and invariability. Generally, an a-historical attitude in the behavioral sciences appears by the use of static concepts, by substantiation and reification of processes, relations, and concepts, by the reducing of complex phenomena to simple metaphors and by generalizing specific regularities to general laws. Some behavioral variabilities are obviously so permanent (especially biological-psycho-

logical processes) that they seem to justify the search for regularity. This search for regularity has presented some general propositions from which, by manipulation of conditions, a variety of behavioral forms can be derived (Schlenker, 1974; Homans, 1967). However, the problem with human beings is that they are not only regular and explainable creatures, but also primarily social- and cultural-historical beings, characterized by continuous new adaptations, by new information, by new world-3-products (Popper), and by new social surroundings and ecological settings. Realization of the historicity of human behavior led Gergen to believe that social psychology is "primarily a historical inquiry", and that it, therefore, cannot detect general principles of human interaction. Social psychology is limited to facts that are largely non-repeatable; to surmount its own historical limits is not possible. According to Gergen, the observed regularities of human behavior (and, therefore, the most important principles) are firmly bounded to historical circumstances. To support this opinion, Gergen, among others, refers to: the different variables, which were indicators for political activism during the onset and the conclusion of the Vietnam war, to the fact that Festinger's theory of social comparison is based on the assumptions that people wish to evaluate precisely, and that they compare themselves with others (assumptions which do not always have validity), to the theory of cognitive dissonance, which emanates from the supposition that people cannot stand contradiction although it is obviously possible in other cultures (and other cultural periods), and to reinforcers of human behavior which do not remain stable (i.e., social approval and applause are not of equal value in all historical periods). In addition to these historical factors, one can add the social theories which reach the common consciousness and, therefore, neutralize each other, as well as the prophecies that fullfill or destroy themselves due to the fact that knowledge of the law leads to actions stated in the preconditions or to actions known to be countersuggestive. (Gergen, 1973: 309-320; Th. de Boer 1975: 756). Gergen concludes that because of these changes of behavior, social

psychology cannot be a nomothetical science, but only a sort of historiography.

Empirical relations change in every field. In psychology, J.W. Atkinson suggested that when a substantial relation is found between personality variables, it describes only "the model personality of a particular society at a particular time in history" (1974, 408). class differences observed in the 1950's were sometimes just the reverse of what had been observed in the 1930's (Bronfenbrenner, 1958). Each construct validity is temporary. With new times, the items carry new implications. Lee J. Cronbach once said: "Generalizations decay. At one time a conclusion describes the existing variance, and ultimately it is valid only as history. The half-life on an empirical proposition may be great or small. The more open a system, the shorter the half-life of relations within it are likely to be. We cannot store up generalizations and constructs for ultimate assembly into a network. It is as if we needed a gross of dry cells before we had half the battery completed. So it is with the potency of our generalization" (Cronbach, 1975: 123).

By reflections on their own results, psychologists are forced, at present, to reformulate the institutional rules of their own language game, to redivide the 'reality domain', and to make new internal rules.

4. Timelevels of behavior - a qualitative distinction

People stand in time in multiple ways at each moment of their history. They are part of time in different ways. In other words, differences in the durability of behavioral aspects constitute different time levels. For an analysis and elaboration of the historical partition of behavioral aspects, conceptual distinction and nomenclature of these time levels is inevitable. A well-known classification in historiography was made by the French historian, Fernand Braudel. This classification distinguishes between the time span of short duration, the conjuncture of medium duration, and the long duration of structure (Braudel, 1958: 725-753). This distinc-

tion is of the molar kind, but it can sufficiently illustrate the heuristic importance of time levels.

The time span of short duration is comprised of the daily events. The range of most research in modern behavioral sciences is not larger than one of those events or one of a series of loose events.

Conjuncture is a time span of medium duration; of decades, of a quarter of a century, or, maximally, of half a century. With its rythm, repetition, and regularity, it is the timelevel that sciences such as economy and sociology are concerned with: the span of time that shows price curves, demographic progressions, developments in wage scales, production, and traffic. In as far as duration is concerned, it is also the source of the so-called longitudinal studies in psychology.

Above the time level of cycles and intercycles, stands the time level of long duration, of *'la tendance séculaire'*, the time of structures, of organizations, of connections between quite steady elements of social generations. According to Braudel, the *"cadres mentaux"* are imprisoned in long duration.

The time level of long duration is essential for behavior, and, therefore, is equally important for behavioral science. It provides the structural framework for research of human behavior, manifesting itself in the time tempi of shorter duration (molecular time units, events, conjunctures). These time tempi have to be fitted into more comprehensive and stable frameworks, in the *"Wirkungszusammenhang"* of structures. Those structures precede, carry and surround the elements and conjunctures, and often continue to exist when those elements and conjunctures have passed. More importantly, they are the immanent partners to events and conjunctures. There is, however, the opposite as well: structures cannnot be separated from the events and conjunctures, and are, in fact, influenced by them.

For the analysis and elaboration of the history of human behavior this means that, in negative terms, one should not get entangled in methodological and theoretical pseudo dilemmas, and,

in positive terms, that there has to be a fluent change-over between narrative, descriptive and analysing methods; between reducing and totalizing schemes. Hermeneutic investigation of people and their behavior can only be a coherent, segmented procedure. Naturally, that also means that it has to be shown what is structural in human behavior and what is conjunctural or determined by events.

The classification of behavioral aspects by time levels and the establishing of those aspects on a time axis has, although not always explicitly, been tested several times in the history of psychology.

For Freud, the most hidden element was also the most permanent one. The 'id' kept its strength despite the various manipulations by the 'ego'. For Jung, the durability was identical to that of the archetypes; they were the prototypes of the residues of the experience of all earlier generations. Others (Rokeach, 1960; Williams, 1971; Hermans, 1973; Popper, 1972) associated longer durability with valuation, values, institutions and organizations rather than with attitudes and cognitions.

5. *Psychology in 3 dimensions*

The historicity of human behavior and the different time levels should be a sufficient reason for formulating new rules and redividing domains, tasks and methods in psychology.

Recognizing that people are changeable does not yet mean that the search for regularity is pointless. That is not at all the case with characteristic behavioral aspects, physiological-psychological processes, and language structures. A structural psychology should operate within these permanent behavioral conditions and forms of behavior. Methodologically, this can be done by means of scientific (biological, biochemical, or physiological) methods, by means of ethological observation and formal mathematical analyses, and/or structuralistic (linguistic) research. The search for laws, however, can only be conditionally formulated even in structural research. Only if certain earlier conditions are realized, certain

consequences can follow: should these conditions not exist, the results fail to appear (Popper, 1972: 337-338). This means that it is not laws, which are relative or temporary, but rather the conditions under which they are valid. The historicity is, therefore, not in the law, but in the circumstances (de Boer 1975: 750).

A conjunctural psychology should study human behavior occuring in a medium term of duration. It is mainly directed at social and cultural aspects. Here, too, there are several methodological possibilities: time series analysis, longitudinal research, cohort-analysis, "figurative" research (Elias), and the "orthodox" historical description. It is situated between other historical-social sciences and the so-called "mental history". Conjunctural psychology also can not avoid the search for laws and regularities. A continuous radical change of action patterns is logically possible, but, empirically, rather an exception. More than a structural psychology, conjunctural psychology would have to take a "shrinking application field" into account (de Boer, 1980).

Contemporaneous and event-directed psychology has to look at behavior as it manifests itself here and now. Methodologically, this psychology should be mainly guided by what Cronbach has called 'the aspiration to assess local events accurately and develop explanatory concepts'. This can occur by the use of experimental research, field research, treatment, action research, and description. Here, every generalization (whether or not distilled out of the other two psychologies) is a working hypothesis, not a conclusion. Therefore, it is the task of contemporaneous psychology to pin down the contemporary facts (Cronbach, 1975). The great challenge, however, would be to attune these levels to one another. Empirical results of their interaction would appear to be the best guarantee "to know the human being as he is".

6. *A frame of reasoning for the study of long-term human behavior*
The evolutionary frame work

6.1. A metaphysical theory

The theory of evolution is not a scientific theory since it

can not be refuted. Even if we found three species of bacteria on Mars tomorrow with a genetic outfit similar to that of three terrestrial species, Darwinism would not be refuted (Popper, 1976, 1973). And yet, according to Popper, this theory remains invaluable. Without this theory, our knowledge would not have progressed as it has since Darwin. Although the theory of evolution is not a scientific theory in the strict sense of the word (Popper calls it a metaphysical theory) it seems able to throw light on practical and concrete research (e.g. the adjustment of bacteria to penicillin). To date, the theory of evolution is the only theory that suggests the existence of an adjustment mechanism and makes it possible to study this mechanism in detail. In addition, this theory can uphold criticism and can be improved.

6.2. *The variants of the 'third world' and the 'third way'*

The theory of evolution (Darwin and Lamarck) and psychology have met several times throughout their mutual histories. Evolutional mechanisms of variation, selection, and retention can be recognized in the behavioral model, especially in operant conditioning. One should also think of Tolman and Brunswik who respectively, investigated, the processes of learning and perception. Lamarckian views were asserted in theories about the dangers of 'social mixing' (Van Hoorn and Verhave, 1977).

After a temporary decline, the epistomological climate changed again in the 1950's and 1960's in favor of evolutionary thinking. Parallels were drawn between biological and cultural evolution (Kluckhohn and Rapoport, 1956: 6-19; Campbell, 1975; 1103-1126), and differences such as between of the exogenetical and the exosomatical conveyance (Medawar 1975: 105-155; 1977: 13-18), as well as in cumulation and speed (Tinbergen, 1976: 1977) were pointed out.

Popper, as well as Piaget and Moscovici pleaded that two variants of evolutionary thinking - the 'third world' and the 'third way' - are of fundamental importance for the research of human behavior of a long duration. More than any other theoretician, Popper not only applied evolutionary thinking to the scien-

ces, but to everything that people do, make, or made. He applied it to theoretical systems, problems, problematic situations, critical arguments (all of these inside as well as outside of science), books, libraries, houses, tools, art, and the descriptive and argumenting functions of language. What he calls the 'third world' next to the (first) physical world and the (second) world of subjective consciousness is, although created by men and reacting on men, greatly autonomous. It creates its own domain and generates new facts and problems, but at the same time, creates new refutations. This world is also real; it is not fiction. The atom-theory works through technologies that are representative of the second world (the second world is always intermediate between the other two worlds) and acts upon the a-organizal and organizal world in a radical manner. This third world is, contrary to other third worlds (e.g. the one of Plato), variable and changeable. According to Popper, people's struggle for existence takes place mainly in this world. The third world is a massive block with guiding mechanisms such as traditions and institutions which provide the necessary stabilization of achievement. According to Popper, the evolutionary process does not at all lead to chaos. His interpretation of the Darwinian theory of evolution suggests that the mechanisms of natural selection simulate what could be the effect of 'the Creator's plan', or what could be the goal of mankind. The continuous series of trials and errors, the confrontations with ever upcoming problems makes it, for instance, possible for a complicated organ such as the eye (possibly resulting from a long series of accidental happenings) to appear as though created according to a well-considered plan. Analogous processes appear throughout the history of societies and human behaviors. With this 'third world', we nearly reach the 'third way', a *tertium quid* between exclusive endogenic and exclusive exogenic dynamism.

Popper thinks - together with Alister Hardy, Schrödinger, and Waddington - that evolution shows 'orthogenetic trends', meaning that it shows successions of changes which go in the same direction. Popper explains this orthogenesis through the operation of

internal as well as external selection mechanisms. With the assumption of different sorts of genes:

- a-genes which mainly control anatomy
- b-genes which mainly control behavior, and which are divided into:
  p-genes which control preferences or purposes, and
  S-genes which control skills,

he has an instrument with which interactions between environment, genetic structure, and behavioral forms can be described. Changes in the environment can cause new problems and lead to the adaptation of new preferences. Those manifest themselves first in the form of investigating, tentative behaviors. If they are successful, changes are brought about in behavior. If they are successful, changes are brought about in the s-structure, which in turn change the a-structure:

P → S → A

There is a process of feedback evident in every phase, taking care of a continuous interaction between the different structures (Popper, 1976: 167-180; 1973). Similar ideas were formulated with a stronger emphasis on behavior as a factor in evolution by Jean Piaget, and with an emphasis on social behavior by Serge Moscovici. The adaptation of people to their environment has always been a fundamental problem for Piaget. Mankind controls all knowledge with respect to his world: he can think, he can use terms, and he can discover relations which, in turn, enable him to control his environment. According to Piaget, the relationship between thinking and reality is an unique example of the biological relation between an organism and its environment. Thinkings, therefore, serves a function in adaptation. Piaget believes that this adaptation demonstrates the aspects of assimiliation and accomodation. In these processes, the available structures of schemes play a guiding role. The structures are not the product of accidental processes or of an absolute preformation: these structures structure themselves in a process that Goldschmidt called the 'phenocopy', a process whereby

changes in the phenotype precede changes in the genotype, and whereby the phenotype is copied by the genotype. It is, therefore, a process by which behavior acts as the motor of evolution (Piaget, 1976).

The process of adaptation, as formulated by Piaget, can be appropriated bot only to the level of reflexes, instinct, and higher cognitive functions, but also to the levels of organisms, individuals, groups of individuals and their products such as sciences, organizations, institutions, societies and cultures. A series of conditions, however, have to be fulfilled before specific completions and concrete applications are possible:

- Each level of organization has its own structure or scheme; this structure or scheme assimilates and accomodates to the possibilites present at that moment.
- Each level stands in time in its own specific manner (or constitutes time in its own manner). Each level must, therefore, be classified by a time category to determine the extent of durability and the working power of its scheme.
- The lower levels serve as preconditions for the rise and existence of higer levels.
- The phenomena and processes of a lower level are indispensable to the complete understanding of phenomena and processes of a higher level; they do not, however, explain what is specifically characteristic of that higher level.
- The specific character of a phenomenon at a higher level that which distinguishes it from the phenomena of lower levels) is only explained by recalling its own preceding events and regularities.
- The higher level influences the lower levels; e.g. collective cultural values give orientation to individual persons (Fortmann, 1971: 164-165; Piaget, 1974: 231-253).

An action scheme, therefore, exists at each behavioral level. These schemes originate from earlier schemes which, through successive differentiations, go back as reflexes and movements. In Popper's terminology, these schemes are forms of background knowledge

which play a part in the formation of problems and their solutions; at the higher levels, values, institutions and traditions accomplish these functions.

It is my opinion that the theory of evolution offers the best chances for connecting the historical long term to the biological phylogeny, and for researching the interaction between species-typical and individual-typical aspects of human behavior (Vossen 1967: 69-86) as well as cultural-typical aspects (Duijker, 1976). It also provides an opportunity to replace the dichotomies between nature and culture or between human and animal behavior and the suppositions that are connected with them (i.e., 'the specific difference' or 'the dominant reality') with new postulates such as 'the transformation of totalities' and 'the complementarity' (Moscovici, 1972: 30, 42; 1974: 236. 238-274).

6.4. A new synthesis: system and evolution

The new positive interest in evolution by system-theorists (e.g. Parsons after 1953, and Luhmann, 1972) and the concurrent association of systems to evolutionary principles, have opened up new theoretical perspectives. The fundamental realization that systems are open and differentiated from the environment is also 'historically produced' because, as Luhmann syas, with differentiation a *"momenthafte, Punkt für Punkt korrelierende Erhaltung der Differenz" is excluded; "Es kann nicht mehr alles gleichzeitig geschehen"*.

The broadening of concepts to thinking in terms of systems and their relativeness to evolution is the consequence of a sharpened realization of how complicated and differentiated our society and our behavior have become. System-theorists have increasingly realized that human society was, and still is, subject to change. A system-theory must be universal if it is to have validity. Its pronouncement should be valid for any and all societies ever in existance.

The theory of evolution, however, also needs system-theory.

Without a fundamental difference between an organism or a social-cultural unit and the environment, evolution is not possible. Moreover, evolution is based on differentiations within the mechanisms of variation, selection and stabilization. These functions must be divided up in different vectors, and their coordination must be regulated. A good understanding of the nature of systems and the terms and methods of system-theory is inevitable if one is to analyse these processes better (Luhmann, 1975; 154-168).
In addition to those already mentioned, there are other reasons for looking at the two theories conjointly. The long term wherein phenomena manifest themselves, the metaphysical research program and, what Popper calls 'the situational logic' of Darwinism, are arguments in favor of evolution theory: if one accepts a world of limited constancy, a world with specific conditions and creatures of limited variability living in it, then a situation is given in which the idea of trial and error elimination can only be called logically necessary. To be able to reduce the complex reality by manageable and representative models by explaining continuities or hierarchic connections with outher (sub)systems, and by stringent arrangement of prediction and testing of propositions are arguments in favor of systems-theory. By coupling the theory of evolution with the systems-theory, it becomes easier to understand why the genetic equipment is not sufficient for adaptation to the constructed third world. When the feedback between people and their environment stops and their psycho-social or cultural systems become closed systems, when the processing of new information ceases, people alienate themselves and their societies become 'abstract societies' which can no longer show the adaptive power necessary in critical moments. Due to this conjunction between evolution and systems, and their collective merging into cybernetics, a larger framework of information-theoretical views can be offered.

7. *Historical psychology and the history of psychology*

7.1.

Historians of science no longer present the history of their disci-

pline as a development from lower to higher, from simple to intricate, or as a pedagogical didactic introduction to the treatment of contemporary problems. But the Kuhnian practitioners of the history of psychology overlook one thing (which is not important in the natural sciences that serve as the model for Kuhn); namely, that human behavior changes in the course of history, that is, that the *object* of their science changes and not only the *view* on that object. Some examples might help to illustrate this point.

7.2.

Psychological development is a process of qualitative changes in functioning, relative to the world and oneself. This process is closely related to the biologically determined physiological maturation and the socially determined phasing of age, but it is not identical to it. The psychological development occurs within certain dimensions such as sexual behavior, social behavior, cognitive processes, and moral behavior. There are certain phase theories within the psychological theory of development that have been established for those different dimensions. Their implicit assumption made by these classifications is that the subsequent phases imply the former ones; that the characteristics of the subsequent phases also belong to adult behavior. The occurrence of characteristics in the former phases determines childlike or adolescent behavior. With that, however, contemporary views on growth to adulthood determines the view on growth to adulthood in former days. It is impossible, however, to compare the phases of life in this abusive manner. Analysis of early-medieval practices show, for example, that the behavior of people was guided by external authorities and the considerations of reward and punishment; it was hardly guided by internalized norms or supposed intentions by others. If Kohlberg's moral phases were applied to this example in an anachronistic way, the conclusion should be that these people were not adult and were still in a earlier phase of life.

The degree of internalization, *Selbstzwang*, or selfcontrol has, in itself, nothing to do with adultness but has almost every-

thing to do with the social-cultural matrix to which people belong. It was an extremely 'intelligent' adaptation that made people, 1500 to 1000 years ago, guide themselves to external forms and norms of authority (Radding, 1978: 577-597).

In the dynamical process of psychological development, the environmental factors (in addition to the individual nature) determine which behavior will be actualized and demonstrated. People from a certain cultural period or a certain social class show conformity in forms of behavior because they have internalized the same roles, symbols, and values; or they react in the same way because they have reached about the same point of development in each of the sectors in human growth. On the basis of demands, education or schooling, role patterns, and other behaviors, modal profiles of children, boys and girls, young people, men and women from certain historical periods and social settings can be constructed with the assistance of these starting points. (Keniston, 1971: 329-346). It can be deducted from late medieval letters and instructions written to future merchants that they should have a good memory for figures, should handle situations rationally, should always be alert to profit. These people were forced to attune their social behavior to this. The Puritans, in particular, made moral demands upon their children, thus creating a different profile of development with a strong awareness of guilt, a suppressed inner life and instinctive life, and a developed consciousness. In every phase of their life, they were forced to make the choice between good and evil; the child was still completely unable to do that, the young people were a bit more able, but they, even more than the adult, were threatened by commotion and unbalance. The process of attribution in this case is morally determined: phases in the course of life are placed on both ends of the axis (pure-spoilt, good-evil), and the chances that existed for moral strength (Greven, 1977, 1970; Demos, 1970; Stannard, 1977; Davies, 1977; Levy, 1978). Other demands were required of the artisans' children during development than those required of the farmers' children or aristocrats' children. It is only these different modal

profiles of development (which have to be constructed), and not the massive concepts of child, adolescent, or adult that are comparable through the ages. By the differentiations made between age phases, physical maturation, and psychological dimensions, it is also possible to more clearly distinguish the influences of the biosocial-historical matrix on psychological development.

7.3.

Social and cultural conditions play an important role in the beginning, the dispersion, and the disappearance of psychological disorders. According to Elias (1969), one of the determinants from this so-called extra-discursive sphere lies in the process of civilization. Ever since the Medieval times, western society has become more and more differentiated, integrated and complex. These changes manifested themselves within interwoven areas; i.e., ways of production, forms of government, education and science. Sociologically and psychologically these changes can be seen as an increase of mutual dependence between people, and, therefore, also as an increase of social control over spontaneous impulses and emotional expressions; the development of the 'them' perspective, and the rational weighing of goals and means. The history of mental diseases is included in this wicker-work of the dependences at issue. The incarceration of the 'lunatics' in the 17th century was made possible by the greater power of the state, the sense for social order, and the new ethics of labor. It was also made possible by the increased feelings of distress that resulted in people with deviant behavior being 'put away'. As such, the history of mental diseases is a symptom of the general process of civilization, of the 'stashing them backstage'. It shows parallels with the disappearance of public executions, which shifted to the use of separation and isolation for forms of punishment; the evolvement of the more closed family (in contrast with the previous open family); the development of architecture so designed that it became possible to seclude oneself. Even death, mourning and burying of the dead became more private. The increasing control of affect also became

discernible in the history of mental diseases. The control of affect, the social constraint for it, and the internal and external conflicts caused by it have, therefore, especially manifested themselves as the cause of disturbed behavior and in its forms of expression.

8. Natural science, once again a model

In the natural sciences, it became obvious that the acknowledgement of movement and change in nature was a necessary condition for their development. Classical Physics studies physical phenomena as separate units against a background of unchangeable coordinates of time and space. In the pursuit of locating the unchangeable behind the changeable, there were conservation for materials, impulse and energy: every real causal explanation meant a conversion to identity. Modern physics, on the contrary, is directed towards establishing a sequence of continuously moving fields which can only arbitrarily be analysed in separated components (Dijksterhuis, 1975: 7). This change in the conception of reality has been, according to Einstein, the most profound and fruitful change in physics (Einstein, 1934). For behavioral sciences, the recognition of human behavioral changes in a continuously moving field of social, biological, and physical factors will also be necessary. Mankind can only become a complete object of scientific research when scientists recognize that "movement is an essential property of his being not something that has to be accounted for separately" (Kelly, 1971: 296).

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EMINENT AUTHORS AND WORKING GROUPS IN AMERICAN PSYCHOLOGY. TWO APPRAOCHES TRHOUGH BIBLIOMETRIC ANALYSIS OF CITATIONS IN PSYCHOLOGICAL JOURNALS

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The image of a science always includes several dimensions. At least it its possible to differentiate in it between theoretical and conceptual dimensions, and social and material ones. The problem lies in finding a unifying principle for all these facets (Ziman, 1968).

We have suggested, as an integrative model, to consider science itself as an organization. As much, it would aim at the discovery of laws governing different kinds of natural phenomena through the specialized labour of many people, working in groups under the direction of some leading scientists, and mantaining formal and informal communications, supported by society that benefits from its results (Carpintero, 1981).

When we want to obtain an historical image of psychology as a scientific enterprise, we can assess the value and importance of different authors and their theoretical contributions measuring their impact and weight in scientific community, specially through their presence in scientific networks (journals, proceedings, conferences, books). In doing so, the quantitative analysis of scientific literature becomes an important tool for the task of the historian and bibliometric methodology can be employed fruitfully (Carpintero & Peiro, 1981; Garfield, 1979). As Garfield proposed, "bibliometry can be defined as the quantification of the bibliographical information capable of being analyzed" (Garfield et al.

1978). This methodology is not without limitations, but gives useful insights and an objectively based knowledge of historical influences and interactions, as it works on some apparent features of informations and communications (Endler, 1978; Carpintero & Peiro, 1981).

In what follows, we offer two applications of that methodology to the history of the psychology in the States. In both cases we have analyzed psychological journals with a common general background. They belong to the same country, the same epoch, and the same scientific community, apart from their differential particularities. A research program enabled us to elaborate complete bibliographic files and citation indexes for some journals from their foundation till 1945 (Carpintero & Peiro, 1978, 1979). For our present work we will take into account data coming from *American Journal of Psychology (A.J.P.)*, *The Psychological Review (P.R.)* and *Psychological Bulletin (P.B.)*. From each original article appearing in them, we have recorded not only title, and authors signing it, but also every citation included in it. (In the case of laboratory communications, presented through as well-known professor, we took also this as an author - or a signature - of the paper). So, it has become possible to evaluate frequency of productive work of an author (through the frequency of appearance of his name among the collected signatures), and also to measure his impact (through the number of evoked citations). Now we turn to our data about eminent authors and their works.

Eminent authors in two journals (American Journal of Psychology, and the Psychological Review)

A historical approach to the development of a science includes, as well as many other factors, the detection of its leaders, and of those works that have contributed the most to present theoretical paradigm. A knowledge of those authors that have made the biggest theoretical contributions is also needed. These leading scientists are persons with academic and social recognition and power, frequently invited by well-known universities and institutions, whose works are supported with private and statal funds;

they normally have many collaborators, and frequently their researches are awarded by learned societies (Cole and Cole, 1973; Cole, 1979; Zuckerman, 1972).

In the detection of these important authors many ways have been employed. Among them, citation analysis tries to evaluate their eminence through the volume of references evoked, in the communication network (books, periodical journals, symposia, and so on) by every author and every work, in the frame of a scientific community (Margolis, 1967; Garfield, 1979).

We have tried to establish, by this method, the most eminent authors - that is, the most cited ones, in *A.J.P.* and *P.R.*, from their birth till 1945. Both journals seemed to us well suited for our purpose; both are interested in broad theoretical questions of scientific psychology, and for many years they have maintained a sort of competition between them. It is noteworthy that *A.J.P.*, founded in 1887 by G.S. Hall, and bought in 1920 by K.M. Dallenbach, has been published as a private journal for the whole period analyzed; at the same time, *P.R.*, founded by J.M. Baldwin and J.McK. Cattell in 1894, became an A.P.A. journal in 1925, a difference in ownership not to be dismissed as meaningless as has been found recently (Gotor, 1982). In any case, they seem to provide us with a good sample of the information, papers and knowledge produced by psychologists in the United States till the middle of our century.

We have taken into account all the citations that appear in original contributions and papers published by *A.J.P. and P.R.* Table I offers the main features of the data we have employed here. It appears that both journals have a very similar citation policy, and the existing differences seem to come from the different annual volume of papers edited by each journal. The average number of citations per article are pretty much the same (10.80 in *A.J.P.*, 10.58 in *P.R.*), and the average citations per author cited, for the whole period, are very similar (3.53 citations per author in *A.J.P.*, in 59 years; 4.31 citations in *P.R.* in 52 years). (That means an average of 0.05 (in *A.J.P.*) and 0.08 (in *P.R.*) citations author-year).

TABLE I.-INFORMATION EMPLOYED IN A CITATION ANALYSIS OF AJP (1887-1945) AND PR (1894-1945)

	AJP	PR		AJP	PR
NUMBER OF YEARS ANALYZED::::::::::::	59	52	DIFFERENT CITED AUTHORS::::::::::	6635	3532
NUMBER OF ARTICLES ANALYZED::::::::::::	2198	1571	CITATIONS TO ANONIMOUS AUTHORS::::	306	828
NUMBER OF CITATIONS:	23754	16062	AVERAGE CITATIONS PER CITED AUTHORS::::::::::::::::::::	3.53	4.31
AVERAGE CITATIONS FOR ARTICLE:::::::::	10.80	10.58	NUMBER OF CITATIONS TO THE 20 MOST CITED AUTHORS::::::::::::	3215	2467
			PERCENT OF TOTAL CITATIONS CORRESPONDING TO THE 20 MOST CITED AUTHORS::::::::::::::::::::	13.50	15.35

TABLE II.- THE MOST CITED AUTHORS IN AJP (1887-1945) AND PR (1894-1945)

a) Common Authors:

	AJP				PR			
	(A)	(B)	(C)	(D)	(a)	(B)	(C)	(D)
WUNDT, W	593	1826	Germany	27	125	-	-	-
TITCHENER, E.	547	1871	G.Britain	27	186	-	-	-
JAMES, W.	156	1841	USA	27	168	-	-	-
BORING, E.G.	153	1886	USA	-	96	-	-	-
KOHLER, W.	118	1886	Germany	27	106	-	-	-
KOFFKA, K.	107	1886	Germany	27	101	-	-	-
THORNDIKE, E.	91	1871	USA	27	191	-	-	-

b) Differential Authors:

WASHBURN, W.	261	1871	USA	23	TOLMAN,E.	180	1886	USA	27
BENTLEY, I.	137	1871	USA	21	WATSON,J.B.	161	1871	USA	27
HALL, G.S.	129	1841	USA	27	HULL,C.L.	151	1886	USA	27
FERNBERGER,S.	110	1886	USA	18	McDOUGALL,W.	150	1871	G.Britain	27
DALLENBACH,K.	108	1886	USA	-	BALDWIN,J.M.	125	1856	USA	25
EBBINGHAUS,H.	105	1856	Germany	27	LASHLEY,K.	120	1886	USA	27
STUMPF,K.	97	1841	Germany	27	LEWIN,K.	106	1886	Germany	26
BINET,A.	94	1856	France	27	WOODWORTH,W.	102	1871	USA	27
HELMHOLTZ,H.	90	1826	Germany	27	DODGE,R.	90	1871	USA	20
CALKINS,M.W.	82	1856	USA	20	SPEARMAN,C.	84	1856	G.Britain	27
WARD,J.	81	1841	G.Britain	24	CARR,H.	75	1871	USA	23
FREUD,S.	78	1856	Austria	27	DEWEY,J.	75	1856	USA	27
KÜLPE,O.	78	1856	Germany	27	DUNLAP,K.	75	1871	USA	22

Total citations:	3215 (13.50%)	2467 (15.35%)
Average citations per author.....	160.75	123.35
Average citations per author in 1 year:	2.7	2.4

A) Citation obtained; (B) Supposed generation; (C) Country of origin; (D) Rank in the Anning-Boring-Watson's ranking.

TABLE III.- FREQUENCY DISTRIBUTION OF THE MOST CITED AUTHORS (AJP AND PR) ACCORDING TO THEIR COUNTRY OF ORIGIN, SUPPOSED GENERATION AND EMMINENCE RANK (ABW)

a) Country of origin	AJP	PR	b) Generation	AJP	PR	c) Emminence	AJP	PR
Germany	7	4	1826	2	1	Rank 27	13	14
Austria	1	0	1841	4	1	Rank 26	0	1
Great Britain	2	3	1856	5	3	Rank 25	0	1
France	1	0	1871	4	8	Rank 24	1	0
USA	9	13	1886	5	7	Rank 23	1	1
						Rank 22	0	1
						Rank 21	1	0
						Rank 20	1	1
						Rank 18	1	0
						Rank 17	0	0
						Average Rank	25.39	26

We have established (Table II) the most cited authors in *A.J.P.* and *P.R.* till 1945. All are placed well above the average level of citations, as they received at least one citation per year. At the same time, these 20 authors, represent, for each journal, more than 10 per cent of all the citations collected, because they represent less than a 1 per cent (0.3 per cent in *A.J.P.*; 0.5 per cent in *P.R.*) of cited authors.

It is possible to group these authors in different ways. They are common names in both lists; nevertheless, each journal also contains some differential ans specific authors.

The common authors seem to represent a scientific core for the American Psychology. Here we find the structuralist tradition, with W. Wundt and E.B. Titchener; the Gestalt school, with K. Koffka and W. Köhler. All of them are foreign people. There are also three american-born persons: W. James, E.L. Thorndike and E.G. Boring, they perhaps could be taken as representatives of an open-minded approach to different problems and different schools of psychology. The case of Boring is particularly interesting; as an "eclectic psychologist" (see Murchison, 1930), he was able to integrate historical and experimental ways of research, psychophysics and psychoanalysis, the structuralism ot Titchener at Cornell and the operationalism of Feigl at Harvard, "moving - as it was said (Watson & Campbell, 1963) - from a narrow focus on particular problems to a focus upon the science of psychology as a whole".

It is also noteworthy the similarity of the impact obtained by these authors in both journals; the exceptional amount of citations of Wundt's and Titchener's works in *A.J.P.* are, in part, due to the fact that the journal offered their complete bibliographies (for Wundt, see Titchener & Geissler, 1908, 1909, 1910, 1911; Titchener & Foster, 1912, 1913, 1914; Titchener, 1921; Titchener & Feldman, 1922; for Titchener see Boring, 1927; Dallenbach, 1928).

There are also differential aspects, besides the common ones. First of all, there seems to be the differences in leading theoretical interests. Hence *A.J.P.* seems to offer well-known names in experimental psychology, centered around Wundt; many of them focu-

sed upon the study of consciousness, from such different points of view, as structuralism, psychoanalysis and Gestalt; *P.R.*, on the other hand, includes the main representatives of psychological schools that occupied the scientific field in the '30s: functionalism (Dewey, Carr, Baldwin), hormic psychology (McDougall), dynamic psychology (Woodworth), behaviorism (Watson, Tolman, Hull, Lashley, as well as others as Dunlap and Dodge, not very far placed), factorial correlational psychology (Spearman), field theory (Lewin), plus the core schools (structuralism and Gestalt) found to be common.

When we consider the most cited authors from the point of view of their native countries, it appears to be some differential trends in both journals (Tables II and III). More than a half of the names included in the *A.J.P.* list are european (55 per cent), and 8 (40 per cent) belong to German-speaking tradition; *P.R.*, on its part, includes only 35 per cent of european-born psychologists, and seems to pay more attention to national authors.

The most cited authors in both journals could also be compared in their levels of eminence, as measured by the importance given to them in such a scale as the Annin-Borning-Watson's ranking (Annin, Boring, Watson, 1968). In the *A.J.P.* list we find 18 authors that also appear in that ranking, and 13 are included in the highest level (65 per cent of all the eminent listed); if averaged their ranks, we obtain a mean of 25.22 per author. In the case of *P.R.*, the list includes 19 authors also appearing in the ranking - the only missing in both lists is E.G. Boring, not entering as evaluated author in the ranking by self-evident reasons -; 14 authors (that means 70 per cent of the names) belong to the highest level; the average rank is equal to 26. So it could be said that similar levels of eminence have been taken into account by both journals (or by the authors publishing in them and accepted by editorial committees), but perhaps *P.R.* seems to pay a little more attention than *A.J.P.* to the psychologists considered most eminent by the Annin-Boring-Watson's ranking, and closer to that way of evaluating psychology than *A.J.P.* does.

We also considered all these psychologists in a generational perspective (Tables II and III). Given their birth years, we grouped them into generations, taken every generation as the group of persons that have born into a span of fifteen years, as a rough measure suggested by the theoretical work of Ortega and Marias (Ortega, 1959; Marias, 1970; Jansen, 1975). These generations will be designated here by its central year. So, all the authors very frequently cited in both journals belong to five generations: that of 1826, of 1841, of 1856, of 1871 and of 1886. Taken both lists as a whole, it is possible to see the growth of modern psychology reflected in them (Table III); more than a half of the authors belong to the two younger generations (1871 and and 1886). If we consider at the same time the generation and the native country of each author, there seems to be a greater importance of European psychologists in the older generations (1826, 1941 and 1856) and a greater weight of American psychologists in the younger ones (1871 and 1886). This points to a change of the gravitational center in modern psychology from the old to the new continent, detected in different ways (Perez-Delgado, Peiro & Carpintero, 1981, Tortosa, Carpintero & Peiro, 1981; Ben-David & Collins, 1966; Littman, 1979).

When we consider the distribution of citations according to the generational level of cited authors, we also find different trends of citation in *A.J.P.* and *P.R.* articles. In *P.R.*, younger generations (1871, 1866) include 15 out of 20 names (75 per cent); the same generations in *A.J.P.*, add upp 9 authors (that means 45 per cent of its list). So, it could be said that *A.J.P.* seems to pay more attention to older eminent people than *P.R.* does.

To summarize, *A.J.P.* and *P.R.* seem to differ in their scientific policies, as they offer differential echoes to the leading authors of classical and modern psychology.

Eminent works in A.J.P. and P.R.

A complementary view can be taken from the study of most cited works in both journals. Generally speaking, very high levels of

TABLE IV THE MOST CITED WORKS IN AMERICAN JOURNAL OF PSYCHOLOGY (1885-1945) AND IN PSYCHOLOGICAL REVIEW (1894-1945).

AMERICAN JOURNAL OF PSYCHOLOGY	PSYCHOLOGICAL REVIEW
B E N T L E Y, I.M. THE FIELD OF PSYCHOLOGY.-13-1924.	B A L D W I N, J.M. MENTAL DEVELOPMENT IN THE CHILD AND THE RACE-25-1895.
B O R I N G, E.G. URBAN'S TABLES AND METHOD OF CONSULTANT STIMULI (AJP),1917)-21-1917.	J A M E S, W. PRINCIPLES OF PSYCHOLOGY-82-1929 PSYCHOLOGY: BRIEFER COURSE-27-1895.
C A L K I N S, M.W. FIRST BOOK IN PSYCHOLOGY-20-1909.	K Ü H L E R, W. GESTALT PSYCHOLOGY-26-1929.
E B B I N G H A U S, H. GRUNDZUGE DER PSYCHOLOGIE-36-1897. UBER DAS GEDÄCHTNIS-30-1885.	K O F F K A, K. THE GROWTH OF THE MIND-31-1924. PRINCIPLES OF GESTALT PSYCHOLOGY-26-1935.
H E L M H O L T Z, H. HANDBUCH DER PSYCHOLOGISCHEN OPTIK-58-1856-1869. DIE LEHRE VON DEN TONEMPFINDUNG-20-1863.	L A S H L E Y, K.S. BRAIN MECHANISMS AND INTELLIGENCE-22-1929.
J A M E S, W. PRINCIPLES OF PSYCHOLOGY-111-1890.	L E W I N, K. A DYNAMIC THEORY OF PERSONALITY-27-1935. PRINCIPLES OF TOPOLOGICAL PSYCHOLOGY-22-1936.
K O F K A, K. PRINCIPLES OF GESTALT PSYCHOLOGY-18-1935. PERCEPTION: AN INTRODUCTION TO THE GESTALT THEORY (P.B. 1922)-17-1922.	Mc D O U G A L L, W. OUTLINE OF PSYCHOLOGY-29-1923. INTRODUCTION TO SOCIAL PSYCHOLOGY-26-1908.
K Ü H L E R, W. GESTALT PSYCHOLOGY: AN INTRODUCTION TO NEW CONCEPTS IN MODERN PSYCHOLOGY-19-1929.	T H O R N D I K E, E.L. EDUCATIONAL PSYCHOLOGY-40-1913-1923. ANIMAL INTELLIGENCE-25-1911.
K Ü L P E, O. GRUNDISS DER PSYCHOLOGIE-52-1893.	T I T C H E N E R, E.B. A TEXTBOOK OF PSYCHOLOGY-46-1909. EXPERIMENTAL PSYCHOLOGY-26-1901-1905.
S T U M P F, K. TON PSYCHOLOGIE-41-1883-1890.	T O L M A N, E.Ch. PURPOSIVE BEHAVIOR IN ANIMALS AND MEN-44-1932.
T I T C H E N E R, E.B. EXPERIMENTAL PSYCHOLOGY: A MANUAL OF LABORATORY PRACTICE-97-1901-1905. A TEXTBOOK OF PSYCHOLOGY-73-1909. LECTURES OF THE ELEMENTARY PSYCHOLOGY OF FEELING AND ATTENTION-33-1908. LECTURES OF THE EXPERIMENTAL PSYCHOLOGY OF THOUGHT PROCESSES-30-1909. BEGINNER'S PSYCHOLOGY-24-1915.	W A T S O N, J.B. PSYCHOLOGY FROM THE STANDPOINT OF A BEHAVIORIST-49-1919. BEHAVIOR: AN INTRODUCTION TO COMPARATIVE PSYCHOLOGY-31-1914.
W A S H B U R N, M.F. THE ANIMAL MIND-17-1908.	W O O D W O R T H, R.S. PSYCHOLOGY: A STUDY OF MENTAL LIFE-39-1921.
W U N D T, W. GRUNDZUGE DER PHYSIOLOGISCHEN PSYCHOLOGIE-128-1874. VORLESUNGEN UBER DIE MENSCHEN UND TIERSEELE-21-1863.	W U N D T, W. PRINCIPLES OF PSYCHOLOGICAL PSYCHOLOGY-59-1893.

See First and Second Note

citation for works are parallel to those that are found for authors. Exceptions are found in two cases: when we deal with the great impact of authors of a solitary work, (the case of exceptional work), or when the sustained activity of some scientists gets a feeble but continuous rate of citation that totalizes an important amount.

In our present study, some interesting features are detected about scientific policies orienting citing behavior. It is easy to see that both lists (Table IV) contain a large amount of books, and only two journal papers are included in them both in *A.J.P.* list (a fact that fits well with the dominant experimental character of this journal, and the greater attention paid to journal articles than to books in experimental research). It is also noteworhty that, in both lists, all the cited items are works written in English or German, and most of them are broad expositions of theoretical value, not specialized monographies; the most frequent word appearing is "psychology"; and many works are text-books.

Although it is not the case that every eminent author has a correlative "eminent" work, it is true that the most cited works have been written by the most cited authors.

There also seems to be a core of works, common to both journals; they present the theoretical ideas of structuralism and Gestalt. included are two text-books of Titchener, the physiological psychology of Wundt, and two general expositions of Gestalt theory, namely those from Köhler and Koffka. There must also be added the classical work of James, *Principles of Psychology,* the work that occupies the first place in a global consideration of the citations of both journals.

When both journals are considered from the standpoint of the eminent works they have cited most, there also appears to be an important difference between them. *A.J.P.* pays more attention to duch experimental works as those of Ebbinghaus, Stumpf, Helmholtz or Kulpe; whereas *P.R.* seems to cover a broader scope of theoretical views by the citations to the main presentations of several schools and tendencies.

A.J.P. and *P.R.* seem to have paid different attention to various historical stages of psychology; *A.J.P.* list includes 11 works out of 20 belonging to the present century, whereas *P.R.* citations 16 out of 20 for the same period - that means 55 per cent against 80 per cent -. If distributed through decades, in *A.J.P.* the mode pertains to 1901-1910 decade, (frequency equal to 5), whereas in *P.R.* the mode is placed in the 1921-1930 decade (frequency equal to 5).

In conclusion, our citation analysis of *A.J.P. and P.R.* articles has shown interesting trends in the development of American psychology. Two different scientific policies in American psychology have been detected, governing the two oldest journals (*A.J.P. and P.R.*). One journal appears more interested in German experimental research and oriented to older authors and older works; the other, *P.R.*, interested basically in the presentation of a broad scope of different theoretical points of view that were dominating the "era of schools" of American Psychology.

But, at the same time, we have detected the existence of a common nucleus of ideas in both journals coming from structuralism, functionalism and Gestalt, that seem to lay the basis for the American psychological tradition.

A more complete view will probably be gained when other journals will be analyzed with this methodology, and new data could be added to our present information.

Working groups in two journals (American Journal of Psychology and Psychological Bulletin).

One of the main features of modern science lies in its collaborative way of obtaining knowledge. This collaboration implies the possibility of combining pieces of research, as a result of the general acceptance of some methodological principles by a wide scientific community. As in other organizations, modern science offers in itself a division of labour, with specialization and a plurality of functions in the research. The sociology of science has detected an increasing growth of collaborative groups in science in our century (Merton, 1973).

TABLE V: COLLABORATIVE GROUPS ("INVISBLE COLLEGES") ESTABLISHED BY JOINED AUTHORSHIP IN *A.J.P. (1887-1945) and in P.B.* (1904-1945)

AMERICAN JOURNAL OF PSYCHOLOGY

1	2	3	4
1	598	598	874
2	97	194	216
3	24	72	110
4	10	40	69
5	9	45	92
6	3	18	46
7	-	-	-
8	1	8	30
13	-	-	-
19	1	19	35
20	1	20	32
375	1	375	690
TOTAL	745	1398	2194(3)

PSYCHOLOGUCAL BULLETIN

1	2	3	4
1	434	434	1104
2	47	94	136
3	13	39	90
4	4	16	35
5	2	10	12
6	2	12	20
7	1	7	14
8	-	-	-
13	1	13	45
TOTAL	504	625	1456

NOTE: Number of authors in each group (1); Number groups (2); Total number of authors (3); Number of articles for each class of groups (4)

A deep study of collaborative groups in modern psychology is needed. It could be admitted as a general hypothesis, that the growing collaboration in psychology paralleled an increasing degree of "naturalization" of its epistemological status as a science.

In the present occasion we try to offer a sketch of collaborative work in american psychology, through the analysis of the articles published in two well-known journals - the *American Journal of Psychology (A.J.P.)* and *The Psychological Bulletin (P.B.)*, from the first numbers till the end of World War II (1945).

In our study, we differentiate working groups joining all those authors together that have joined their signatures when publishing some article. In doing so, all the names that are bound, are, taken as members of the same group, or, as sometimes it has been called (Price and Beaver, 1966; Peiro, 1981), as an "invisible college" defined through collaborative work and joined authorship (joined signatures) of papers.

The hypothesis is that those authors who do their research and publish the results together are closely connected and become indirectly related to those who collaborated with their own collaborators. In this way we may group many authors in a complex unity, whose real meaning has to be ascertained in the context of the history of our science - in the context of American psychology, in our case.

In our study, we have taken into account all the articles with two or more signatures appearing in both journals for the studied period. Every new author that produced a collaborative work with a former member of one "invisible college" has been included in the same group. Applying this methodology, we obtained some working groups with a lasting presence and influence in each journal, as well as many others of more limited scope and shorter existence (Table V).

More than half of the authors in *A.J.P.* (791 authors, that means 57 per cent) are included in the 147 groups that have been found (with an average of 5.38 members in each group). The biggest "invisible college" joins 375 members (Appendix I) (nearly one

third of all the authors) and explains also one third of the whole volume of published articles (690 papers). The following group adds up to 19 authors, with 35 articles (nearly a 2 per cent).

The former includes many important psychologists that headed different sub-groups, all around the leading figure of E.B. Titchener. It is easy to understand that this group represents the main intellectual line of the journal. It covers the whole analyzed period and includes nearly all the editors of *A.J.P.*, with the exception of G.S. Hall and J.W. Baird. Many of the productive authors obtained their Ph.D. degree at Cornell University, under Titchener, as E.G. Boring, M.F. Washburn, K.M. Dallenbach, M.I. Bentley, and W.P. Pillsbury. It also includes H.P. Weld, close collaborator of Titchener in the laboratory of Cornell, and E.C. Sanford, head of a subgroup centered at Clark University and close friend of Titchener (Boring, 1950; Ross, 1972).

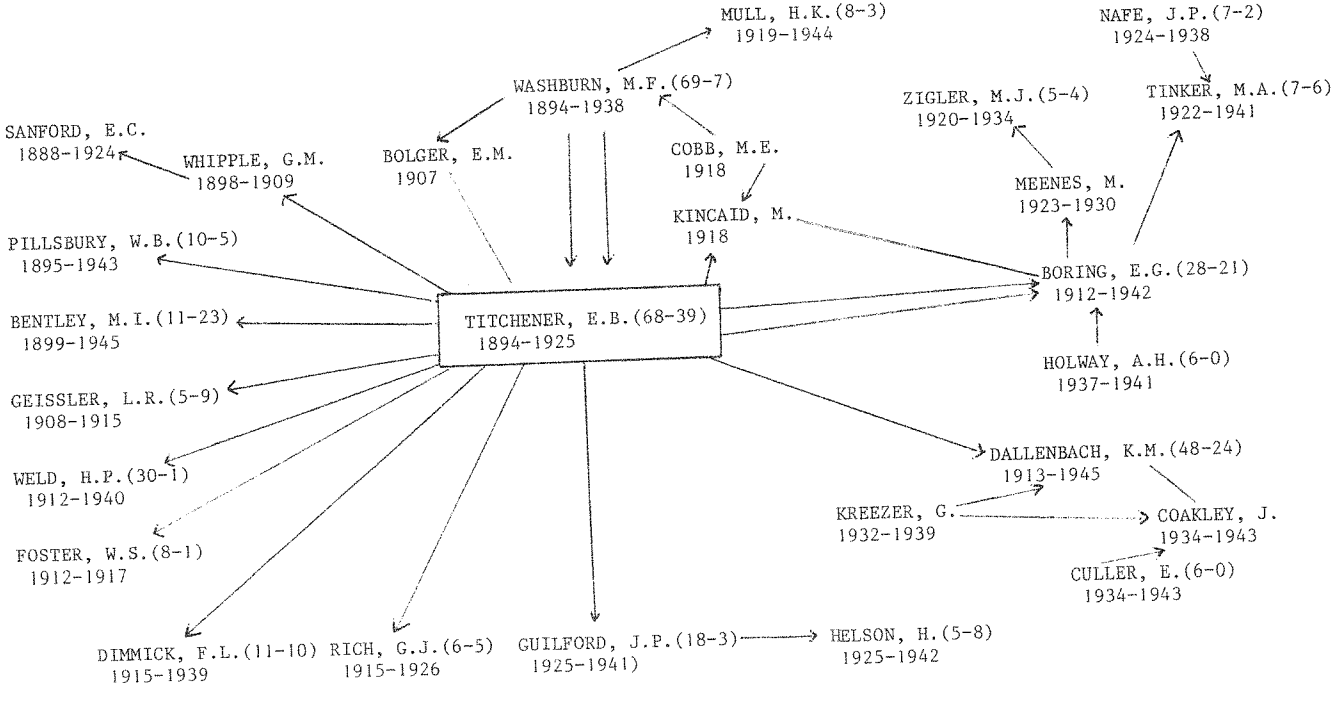
Cornell University, with Titchener, and Vassar College, with Washburn, appear to be the institutional core of this "invisible college" and also of the *A.J.P.* itself. The group aimed at the progress of scientific psychology, paying special attention to the laboratory research, and this was also the program and goals of *A.J.P.* (Hall, 1887, 1895; Titchener, 1921; Dallenbach, Washburn, Bentley and Boring, 1926).

The main subject matters studied by this group are basic psychological processes, from an experimental point of view, and research methodology.

As the group had an active life in the journal for more than half a century, there does not seem to be a small difference between its early members and the latter ones. At the beginning, M.F. Washburn, E.B. Titchener and E.C. Sanford worked on the dominant questions of those days, namely sensation, perception and feeling processes. After that Pillsbury, Boring, Dallenbach, among others, added supplementary interest on memory and learning questions. As Titchener passed away, Guilford, Thorndike and other well-known members paid more attention to research in educational and intelligence areas. As a whole unity, this "invisible college" seems to

GRAPHIC 1 "Invisible College" of E.B. TITCHENER (American Journal of Psychology, 1887-1945)

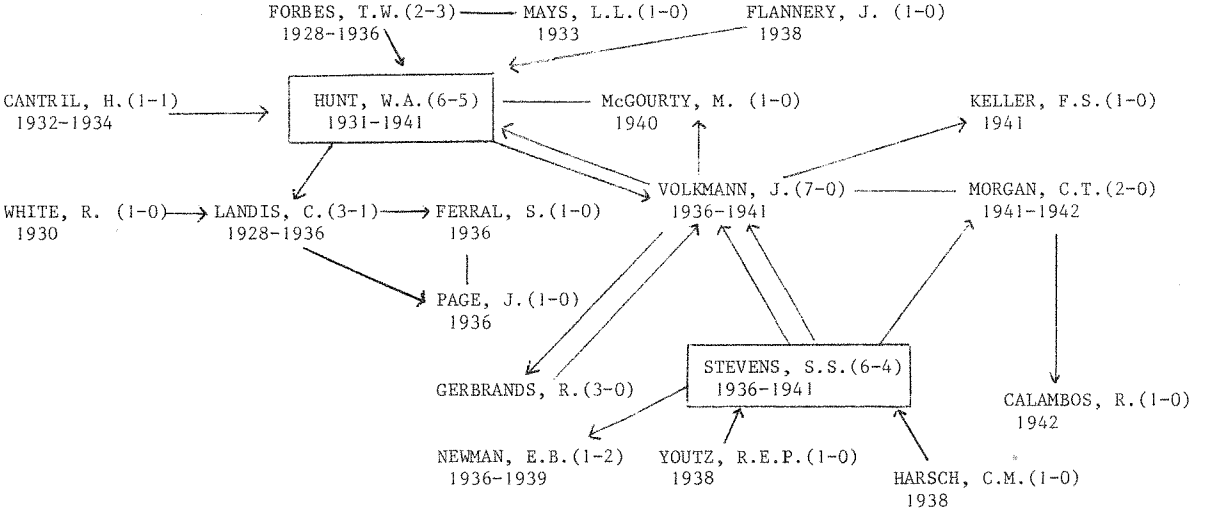
Numer of authors : 375
Number of articles : 690
Years of permanence : 1888-1945



See Forth Note

GRAPHIC 2 "Invisible College" of W.A. HUNT and S.S. STEVENS (American Journal of Psychology, 1887-1945)

Number of authors : 19
Number of articles : 35
Years of permanence : 1928-1942



focus upon human psychology with an experimental background not far from the German tradition.

The second productive group we want to consider here includes 19 authors and had contributed tot *A.J.P.* with 35 articles (1.6 per cent of all the articles). It groups many important psychologists that worked in the Harvard laboratories in the '30s. By those years the operationalist psychology was at its best in Harvard, by the efforts of S.S. Stevens, influenced by Feigl and supported by Boring. There are two subgroups, headed by Stevens and Hunt, respectively; both of them seem to be connected by the work of J. Volkmann. The Stevens subgroup worked on psychophysics and scaling theory, and the Hunt subgroup in psychophysiology and emotion; Volkmann seems to have been interested in emotion and affection, as well as in scaling methodology. The whole group worked on human psychology with psychometric methods.

Working groups in the psychological bulletin

This journal shows a very different picture. It offers a short number of articles written in collaboration. For the whole period they represent only 24.2 per cent of the published articles, and the amount of articles with only one signature equals to 75.8 per cent.

Only one third of the authors (191, that means 30.6 per cent) show joint signatures, and there is a large number (434, or 69,4 per cent) without collaborative work in the journal.

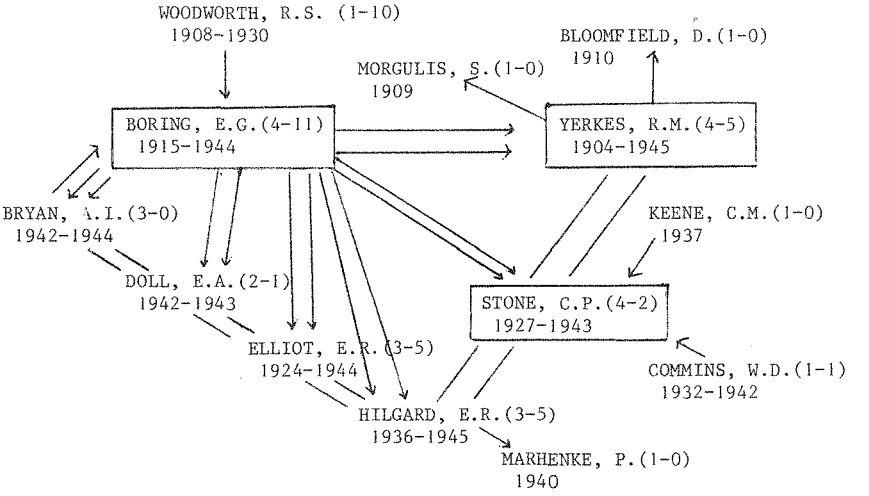
It is also noteworthy the large number of authors with only one or two articles in the journal; they appear to be "transient authors" in Cole and Cole terminology, with sporadic presence in these pages.

Among the groups of authors detected in *P.B.*, the two biggest "invisible colleges" appear to be centered round E.G. Boring and A.T. Poffenberger respectively. Let us consider their main features.

Around the leading figure of E.G. Boring there appear to be 13 authors, that have contributed with 45 articles, but only 9 papers have two or more signature. The permanence of the group in the

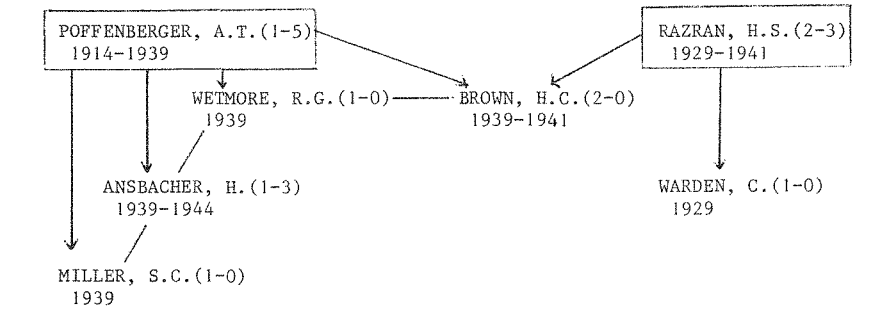
GRAPHIC 3 GROUP OF E.G. BORING (Psychological Bulletin)

Number of authors : 13
Number of articles : 45
Years permanence in PB : 1904-1945
Number commun articles : 9



GRAPHIC 4 GROUP OF A.T.POFFENBERGER y G.H.S. RAZRAN (Psychological Bulletin)

Number of authors : 7
Number of articles : 14
Years permanence in P.B. : 1914-1944



journal covers the whole period; it takes 42 years. It includes well-known psychologists as Yerkes, Woodworth, Stone, Hilgard and Boring himself.

The unity of this group is not deep rooted. It seems to include four minor nuclei, put together as a result of collaborative work in the World War II. One of them, around Yerkes, has focused on animal psychology, with the presence of D. Bloomfield and S. Morgulis (this one, with Yerkes, offering in *P.B.* the first account of pavlovian methodology for American readers in 1909). The second one, around C.P. Stone, seems interested in psychophysiology, and includes W.D. Commins and C.H. Keene; its central years are in the '30s. The third nucleus contains the names of E.R. Hilgard and P. Marhenke, working on learning; and the latest one, is based in a report at the IX International Congress of Psychology, in New Haven (U.S.A.), jointly signed by R.S. Woodworth and E.G. Boring. All these groups became integrated and in a larger structure, with new names as those of Bryan, Doll and Elliot, when a subcommittee on survey and planning for psychology, created on the occasion of the II World War, published two reports on military psychology in the pages of *P.B.* in 1924. Those reports were signed by all these authors - Boring, Yerkes, Stone, Hilgard, Bryan, Doll and Elliot -. Without the reports this "College" would not exist at all.

The second group to be considered here includes two shorter clusters centered around A.T. Poffenberger and G.H. Razran. They are linked by H.C. Brown who collaborated with Poffenberger in 1939 (working on an index of "The Psychological Index"), and with Razran in 1941 (on military psychology). Here we are also dealing with an artifactual "invisible college", of very problematic unity, without thematic coherence.

The obtained results show us interesting differences between the two journals according to the structure of their collaborative groups of authors and its theoretical meaning.

The "invisible colleges" of *A.J.P.* present internal coherence based in theoretical grounds, with frequent master-pupil connections, common research facilities or institutions of educational

nature, and with an important amount of collaborative work. They seem to be some kind of psychological school, guided by a leading researcher, placed in a laboratory center, and doing experimental research along a common line.

At the other hand, the groups of *P.B.* seem of a very different nature. They have only an artifactual unity, not a theoretical one; they include only few members with a small amount of collaborative work, and their final structures seem to depend on social grounds.

The journals differ also in the amount of collaborative articles and the number of groups of authors detected.

By far, the ultimate reason of all the founded differences could be places in the various nature of both journals.

A.J.P. has been all the time a journal dedicated to experimental psychology, publishing original research done in laboratories. *P.B.*, on its part, focused on reports and reviews, giving critical accounts of psychological literature and research carried out by others, and was also interested in social and institutional facts and news.

The crucial point seems here to be placed in the deep difference between both kinds of scientific literature, the "research article" and the "review article", that are produced in two dissimilar ways with particular conditions in each case.

Concluding remarks

At the end of our present exploration of three specialized journals through bibliometric methodology, it appears that the image of a science, when established through scientific literature, offers characteristic features, modulated by the literary genus of analyzed works (journal article, bibliographical revision), the dominant orientation of periodical publications including theoretical bias and particular interests of editorial groups. The variable 'journal' has not to be forgotten, as it explains an important amount of 'variance' in scientific communication.

Existing differences had been shown between three journals,

A.J.P., *P.R.* and *P.B.*, differences that include differential theoretical influences, specific networks of collaborative work through "invisible colleges", and idiosyncratic response to eminent works and classic authors.

Moreover, bibliotmetric methodology, when integrated in a larger comprehensive understanding of data, permits to differentiate between different kinds of results, such as real "invisible colleges" against the episodic ones, or normal rate of citation against simple bibliographies.

We have detected, trhough quantitative methodology, well-known theoretical differences inside the psychological community of the United States, that very early benefited from the existence of various communication channels for the ideas and a rich institutional background supporting differences.

It would be possible to go further in this sort of research, by taking into account important changes in time, corresponding to real developments and variations of a science in a historical frame. We tried here only to offer the flavour of that kind of approach.

APPENDIX I

ALFABETICAL LIST OF 375 AUTHORS INCLUDED IN THE "INVISIBLE COLLEGE" OF E.B. TITCHENER (American Jourbal of Psychology 1.887-1.945).

ABRAHAMS, H. (1, 0, 1937); ADAMS, A.L., (1, 0, 1932); ADES, H.W. (2, 0, 1939-1943); ADLER, H. (1, 0, 1914); ALSPACH, E.M. (2, 0, 1916-1917); ALSTON, J.H. (1, 0, 1920); ALSTYNE, D. Van (1, 0, 1922); ALVORD, E.A. (1, 0, 1907); ALLEN, E.C. (1, 0, 1936); ALLEN, L.K. (1, 0, 1938); ANDREWS, G.A. (1, 0, 1900); ARNETT, L.D. (1, 2, 1904-1905); ARONOFF, S. (1, 0, 1936); ATHERTON, M.V. (1, 0, 1912); ATWATER, M.J. (1, 0, 1924); BACON, M.M. (1, 0, 1914); BAILY, H.W. (1, 0, 1945); BARBITT, M. (1, 0, 1915); BARNES, F.F. (1, 0, 1908); BARNES, S.B. (1, 0, 1905); BARNHOLT, S.E. (1, 0, 1911); BASSETT, M.F. (1, 0, 1919); BAUM, H. (1, 0, 1919); BAXTER, M.F. (1, 1, 1917-1927); BELL, A. (1, 0, 1906); BENTLEY, I.M. (11, 23, 1899-1915); BERGSTROM, J.A. (2, 3, 1893-1907); BICKNELL, N. (1, 0, 1935); BIRCH, L.G. (1, 0, 1897); BOLGER, E.M. (1, 0, 1907); BOLTON, T.L. (2, 3, 1891-1903); BOOTH, M.E. (1, 0, 1926); BORING, E.G. (28, 21, 1912-1942); BOSWELL, F.P. (1, 0, 1916); BOWMAN, A.M. (1, 0, 1920); BROGDEN, W.J. (3, 4, 1935-1943); BROWN, A.J. (1, 0, 1916); BROWN, D.E. (1, 0, 1913); BROWNING, M. (1, 0, 1913); BRUES, A.M. (1, 0, 1927); BURGERT, R.H. (1, 0, 1936); BURKE, R.S. (1, 0, 1924); BURNETT, N.C. (2, 0, 1927-1928); BURNS, M. (1, 0, 1933); BURR, E.T. (1, 0, 1913); CALKINS, M.W. (4, 7, 1893-1927); CARNES, M. (1, 0, 1916); CATTELL, J. (1, 0, 1918); CLARK, D. (1, 0, 1911); CLARK, H. (1, 1, 1913-1916); COAKLEY, J. (3, 0, 1934-1943); COBB, M.E. (1, 0, 1918); COGGINS, K. (1, 0, 1942); COLEGROVE, F.W. (2, 1, 1899); CONKLIN, V. (1, 0, 1925); COCK, B. (1, 0, 1930); COOK, C. (1, 0, 1937); COCK, H.D. (1, 0, 1899); COTZIN, M. (2, 0, 1941-1944); COWDRIK, M. (2, 0, 1917-1919); CRANE, R.L. (1, 0, 1917); CRAWFORD, D. (1, 0, 1911); CULLER, E. (6, 0, 1934-1943); CURTIS, H.S. (1, 0, 1900); CURTIS, J.N. (1, 2, 1915-1917); CHAMBERLAIN, E. (1, 0, 1909); CHAPIN, M.W. (1, 0, 1912); CHICHIZOLA, T.L. (1, 0, 1931); DALLENBACH, K.M. (48, 24, 1913-1945); DALLENBACH, J.W. (1, 0, 1943); DANTELS, A.H. (1, 1, 1893-1895); DANZIG, E.R. (1, 0, 1940); DARLINGTON, L. (1, 0, 1898); DEAN, P.R. (1, 0, 1909); DEWEY, D. (1, 0, 1924); DEYO, D. (1, 0, 1924); DICHMAN, B. (1, 0, 1944); DIGGS, E. (1, 0, 1942); DIMMICH, F.L. (11, 10, 1915-1939); DODGE, A. (1, 0, 1934); DODGE, R. (3, 3, 1912-1931); DON, V.J. (1, 0, 1924); DONALDSON, H.H. (1, 6, 1888-1892); DONOVAN, M.E. (1, 0, 1913); DRESSLAR, F.B. (1, 4, 1892-1903); DRURY, M.B. (1, 1, 1931-1933); EARHART, R.H. (1, 0, 1933); EBERSBACH, R. (1, 0, 1930); EDES, B. (1, 0, 1936); EDWARDS, A.L. (1, 0, 1939); EKDAHL, A.G. (1, 0, 1934); ELLIOT, M. (1, 1, 1922-1924); ENGLISH, H.B. (4, 5, 1921-1939); ERB, M.R. (1, 1, 1937-1939); ERNEST, J.L. (1, 0, 1924); EWART, E. (1, 0, 1940); FATSON, C. (1, 0, 1934); FARNSWORTH, P.A. (4, 4, 1925-1938); FEHRER, E.V. (1, 1, 1932-1935); FELDMAN, S. (1, 5, 1922-1944); FERRALL, S.C. (1, 0, 1930); FIELD, R. (1, 0, 1923); FINCH, G. (2, 0, 1934-1935); FINEMAN, A.E. (1, 0, 1924); FOSTER, W.S. (8, 1, 1912-1917); FREDERICKSEN, N.D. (1, 0, 1934); FREIBERG, A.D. (1, 2, 1929-1937); FRIEDLINE, C.L. (1, 1, 1918-1929); GALLOWAY, C.E. (1, 0, 1904); GARD, W.L. (1, 0, 1907); GARVER, L.N. (1, 0, 1915); GATES, E.J. (2, 0, 1915); GEISSLER, S.R.

(5, 9, 1908-1915); GLANG, E. (1, 0, 1925); GIBSON, L. (1, 0, 1919); GILL, N.F. (1, 0, 1926); GILLETTE, A. (1, 0, 1933); GINSBERG, D. (1, 0, 1924); GLANVILLE, A.D. (1, 1, 1929-1933); GLASCOCK, J. (1, 0, 1918); GLEASON, J.M. (1, 1, 1915-1919); GLIKSMAN, E. (1, 0, 1926); GOLDMAN, N. (1, 0, 1932); GOODELL, M.S. (1, 0, 1911); GRAHAM, C.H. (4, 0, 1930-1937); GRASSI, J.R. (1, 0, 1942); GRAVDS, K.B. (1, 0, 1919); GRITMAN, W.B. (1, 0, 1929); GROSE, S.L. (1, 0, 1921); GROSS, N. (1, 0, 1943); GRUBBS, W.H. (1, 0, 1932); GUILFORD, J.P. (18, 3, 1925-1941); GUNDLACH, R.H. (2, 2, 1930-1935); HACKMAN, R.B. (1, 0, 1936); HAIGHT, B. (1, 0, 1921); HALLAM, F.N. (1, 0, 1896); HAMLIN, A.J. (1, 1, 1895); HANGER, E. (1, 0, 1942); HARDING, L. (1, 0, 1925); HARTMAN, T. (1, 0, 1919); HATT, E. (2, 0, 1923); HEATH, E. (1, 0, 1919); HELSON, H. (5, 8, 1925-1942); HENSLEY, R. (1, 0, 1942); HERRINGTON, F.A. (1, 0, 1907); HEYWOOD, A. (1, 0, 1905); HICKS, J. (1, 0, 1908); HILL, A.B. (1, 0, 1894); HOAG, R. (1, 0, 1908); HOISINGTON, L.B. (3, 2, 1917-1924); HOLT, C.N. (1, 0, 1929); HOLT, E.D. (2, 0, 1923); HOLWAY, A.H. (6, 0, 1937-1941); HOPSON, L. (1, 0, 1917); HOUSTON, H.E. (2, 0, 1907-1908); HOWE, H.C. (1, 0, 1894); HOWELL, A. (1, 0, 1917); HUBBARD, M.R. (2, 0, 1939); HUDGES, E. (1, 0, 1930); HURVICH, L.M. (2, 0, 1937-1938); HYDE, W.F. (1, 1, 1926-1929); IVES, M. (1, 0, 1925); JACOB, E. (1, 0, 1930); JACKSON, H. (1, 0, 1930); JENKINS, J.G. (3, 2, 1924-1933); JOHNSON, C. (1, 0, 1944); JONES, M.G. (1, 0, 1926); KEELER, K. (1, 0, 1929); KELLY, E.L. (1, 0, 1934); KEPLER, H. (1, 0, 1927); KILLEN, B. (1, 0, 1904); KINCAID, M. (2, 0, 1918); KNOX, H.W. (1, 0, 1894); KRAKAUER, D. (2, 0, 1937); KREEZER, G. (2, 4, 1932-1939); KRYTER, K.D. (1, 0, 1943); KUNKEL, F.M. (2, 0, 1919); LACEY, B.C. (1, 0, 1941); LACEY, J.I. (2, 0, 1939-1941); LASKI, E. (2, 0, 1916-1918); LAY, W. (1, 0, 1909); LEACH, H.M. (1, 0, 1910); LEAROYD, M.W. (1, 0, 1895); LEUBA, J.H. (2, 7, 1893-1917); LEVINE, H.A. (1, 1, 1936-1942); LEVINE, J. (1, 0, 1937); LINDEMANN, J.A. (1, 0, 1908); LINDNER, R.M. (1, 1, 1938-1939); LINDSAY, C. (1, 0, 1942); LIPMAN, E.A. (2, 0, 1938-1942); LITCHFIELD, M. (1, 0, 1919); LOWENSTEIN, E. (2, 0, 1930-1937); LOWY, K. (1, 0, 1943); LUCE, A. (1, 0, 1917); McBROOM, N. (1, 0, 1927); McDONALD. M.T. (1, 0, 1922); McKENZIE, M. (1, 0, 1930); McLEAN, K.G. (1, 0, 1934); McMICHAEL, G. (1, 0, 1933); McNEIN, M. (1, 0, 1909); McOUBREY, C. (1, 0, 1931); MACK, M. (1, 0, 1926); MAJOR, D.R. (1, 1, 1895-1898); MALIAY, M. (1, 0, 1931); MANNING, P. (1, 0, 1934); MANRO, H.M. (1, 0, 1908); MARKS, D. (1, 0, 1924); MARRILL, G. (1, 0, 1942); MARSHALL, B.H. (1, 0, 1934); MAY, S. (1, 0, 1917); MEADS, L.G. (1, 0, 1915); MEENES, M.A. (2, 1, 1923-1930); MILES, C.C. (3, 0, 1895-1932); MILES, W.R. (3, 4, 1928-1939); MILLER, D. (1, 0, 1930); MISUMI, I. (1, 0, 1931); MODELD, J.D. (1, 0, 1915); MOEDER, W.D. (1, 0, 1933); MOESSNER, L.R. (1, 0, 1924); MOGENSEN, M.F. (1, 0, 1926); MONTAGUE, M. (1, 0, 1918); MOORE, E.M. (1, 0, 1934); MORGAN, E. (1, 0, 1919); MOULD, M. (1, 0, 1915); MOYER, F.E. (1, 0, 1897); MUCKENHOUPT, L. (1, 0, 1906); MULL, H.K. (8, 3, 1919-1944); MULLER, E.F. (1, 0, 1922); NAFE, J.P. (7, 2, 1924-1938); NAYLOR, A. (1, 0, 1931); NEAL, E. (1, 0, 1926);

NEFF, W.S. (1, 2, 1936-1937); NEW, K.B. (1, 0, 1929); NOH, L.J. (1, 0, 1930); NORRIS, E.L. (1, 0, 1911); NORTHUP, K.M. (1, 0, 1926); PARK, D.G. (1, 0, 1931); PARRISH, C.J. (1, 0, 1895); PARRISH, C.S. (1, 0, 1897); PARSHALL, F.M. (1, 0, 1929); PARTRIDGE, G. E. (1, 1, 1900); PATERSON, D.G. (3, 0, 1930-1933); PATERSON, E. (1, 0, 1929); PHINNEY, E.D. (1, 0, 1896); PILLSBURY, W.B. (10, 5, 1895-1943); PISEK, F. (1, 0, 1925); POLLACK, M. (1, 0, 1925); POTTER, H.M. (1, 0, 1914); POWELL, N.J. (3, 0, 1937-1942); POWELSON, I. (1, 0, 1913); POYNTER, W.F. (1, 0, 1931); PRATT, C.E. (2, 3, 1920-1933); PRESTON, B. (1, 0, 1944); PRITCHARD, W. (1, 0, 1927); QUACKENBUSH, N. (1, 0, 1913); RAUSH, H.L. (1, 0, 1943); RAVESSLER, R.L. (1, 0, 1943); REAGAN, C. (1, 0, 1934); REGENSBURG, J. (1, 0, 1921); REIMER, I. (1, 0, 1927); REYNOLDS, M.M. (1, 0, 1918); RICH, G.J. (6, 5, 1915-1926); RING, C.C. (1, 0, 1930); RIZZOLO, A. (1, 0, 1924); ROBERTS, D. (1, 0, 1930); ROBINS, H. (1, 0, 1915); ROBLEE, L. (1, 0, 1912); ROESE, K. (1, 0, 1916); ROFF, M. (1, 0, 1938); ROOD, E.A. (1, 0, 1914); ROWE, L. (1, 0, 1908); ROWLEY, J. (1, 0, 1926); RUBIN, B.R. (1, 0, 1924); RUCKMICK, C.A. (4, 16, 1912-1932); RUDERMAN, S. (1, 0, 1924); RUDISILL, E.S. (1, 1, 1924-1925); RUNDQUIST, E.A. (1, 0, 1933); RUSSO, S. (1, 0, 1939); SABINE, G.H. (1, 0, 1905); SANDERS, R.W. (1, 0, 1929); SANFORD, E.C. (20, 23, 1888-1924); SAUDERS, W.H. (1, 0, 1907); SAUNDERS, M. (1, 0, 1944); SCAHILL, H.G. (1, 0, 1925); SCHAUB, A. de V. (1, 2, 1909-1911); SCOTT, R. (1, 0, 1934); SCRIPTURE, E.W. (1, 3, 1891-1894); SEARLE, H.E. (1, 0, 1907); SECOR, W.B. (1, 0, 1900); SEVERENCE, E. (1, 0, 1907); SHAAD, D.J. (1, 0, 1931); SHAW, A.M. (1, 0, 1908); SHEARER, L.C. (1, 0, 1916); SHELLEY, M. (1, 0, 1926); SHUEY, A.M. (1, 1, 1924-1926); SHURRAGER, P.S. (1, 0, 1939); SIMONS, H. (1, 0, 1925); SLAUGHTER, J.W. (1, 3, 1900-1902); SLIGH, G. (1, 0, 1930); SMITH, D.L. (1, 0, 1933); SMITH, F.E. (1, 0, 1924); SMITH, D. (1, 0, 1915); SMITH, J.S. (1, 0, 1915); SMITH, M.K. (1, 0, 1907); SPENCE, W. (1, 0, 1933); STEPANOVA, V. (1, 0, 1923); STEVENS, H.C. (1, 3, 1902-1905); STEVENSON, T.J. (1, 0, 1908); STEWARD, C.C. (1, 0, 1900); STEWART, C. (1, 0, 1930); STOCKER, S. (1, 0, 1926); STONE, L.J. (2, 0, 1934-1936); STONE, S.A. (1, 0, 1926); STRAUSS, H.H. (1, 0, 1919); STROH, M. (1, 0, 1908); STRONG, C. (1, 0, 1921); SUCHMAN, E.A. (1, 0, 1938); SUPA, M. (2, 0, 1941-1944); TALBOT, E.B. (1, 1, 1897-1898); TAYLOR, A.H. (1, 0, 1910); TAYLOR, D.W. (2, 1, 1942-1943); TAYLOR, M.L. (1, 0, 1895); TAYLOR, R.W. (1, 0, 1901); TERMAN, L.M. (1, 2, 1905-1929); THALMAN, W.A. (1, 2, 1921-1941); THORNDIKE, E.L. (3, 8, 1908-1935); THORNE, F.C. (1, 1, 1933); THUMA, B.D. (3, 0, 1927-1942); THURNSTONE, E. (1, 0, 1934); TINKER, M.A. (7, 6, 1922-1941); TITCHENER, E.B. (68, 39, 1894-1925); TOMLINSON, D. (1, 0, 1925); TRAVIS, R.C. (2, 4, 1925-1945); TREADWELL, L. (1, 0, 1915); TRIPLETT, N. (1, 4, 1898-1902); TUTTLE, R. (1, 0, 1914); TWISS, A.G. (1, 0, 1911); UHLMAN, R.F. (1, 0, 1919); VARON, E.J. (1, 0, 1933); VOGELER, R. (2, 0, 1931-1932); VORTRIEDE, H.A. (1, 0, 1905); WAGONER, K.S. (5, 0, 1937-1938); WALKER, A. (1, 0, 1919); WALLACE, R.F. (1, 0, 1933); WALTON, J.E. (1, 1, 1933-1937); WARD, K. (1, 0, 1928); WARNE, C.J. (1, 0, 1919); WASHBURN, M.F. (69, 7, 1894-1938); WATANABE, R. (3, 0, 1894-

1895); WATKINS, P. (1, 0, 1900); WEBER, C.O. (2, 5, 1925-1941); WEED, S. (1, 0, 1896); WELD, H.P. (30, 1, 1912-1940); WEMPLE, L. (1, 0, 1930); WERNER, S. (4, 2, 1935-1942); WEST, J. (1, 0, 1924); WESTON, S.B. (1, 0, 1926); WHEELER, H.S. (1, 0, 1906); WHIPPLE, G.M. (2, 7, 1898-1909); WHITE, A.M. (1, 0, 1932); WHITE, S.D. (1, 0, 1917); WILKE, M. (1, 0, 1930); WILSON, D. (1, 0, 1924); WILSON, T. (1, 0, 1934); WILLIAMS, H.D. (1, 0, 1918); WILLIAMS, M. (1, 0, 1914); WINFIELD, M. (2, 0, 1919-1921); WINTER, C. (1, 0, 1926); WOLF, E. (1, 0, 1923); WOODRUFE, L. (1, 0, 1944); WOODS, (1, 0, 1915); WRIGHT, C. (1, 0, 1938); YAMADA, K. (1, 0, 1917); YOUNG, C.W. (1, 0, 1941); ZIGLER, M.J. (5, 4, 1920-1934).

See Fifth Note.

Notes

(1) The works underlined have been cited in the two journals.

(2) The numbers to the left of works denoted, the first the totality of citations, the second the year of publication.

(3) Not included here are 4 anonymous papers detected in the journal.

(4) All the authors with 5 or more articles are represented here. Also included are those authors who served as connecting bonds of different groups. Each arrow goes from a first author to every second one; second authors are connected by mere straight lines. In brackets, the number of articles in collaboration, followed by the total number of articles published in the journal. Also indicated is the year of presence in the journal.

(5) Each name is followed, in brackets, by an indication of the number of articles published in collaborative way, the number of articles published as single author, and the years corresponding to his presence in the journal.

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BIBLIOMETRIC METHODS AND THE INTELLECTUAL HISTORY OF SCIENTIFIC SPECIALTIES: CITATION TRACING AND THE HISTORY OF PSYCHOLINGUISTICS.

Marc De Mey
University of Ghent

The life cycle of a paradigm

In some applications of the paradigm-concept, the difference between implicit knowledge and expectations on the one hand and explicit and established knowledge on the other, is accounted for in terms of a trade off relation maintained over the paradigm life-cyle. In its initial phase the paradigm is a promising source of ideas evoking possible investigations and generating expectations about their outcome. As the suggested research is gradually realized and solid data are assembled, the knowledge involved gradually becomes more articulated and the share of programmatic principles diminishes. Finally, the suggestive power of the paradigm seems used up and the scientific community is left with a more or less stabilized body of knowledge which can be added to the store of established science or classified as a dead end. The specialty enters what Yellin (1972) calls the "post developed state". Several models for the development of specialties distinguish between four stages for a paradigm life-cycles.Goffman (1971) introduces a sequence of stage 1: insufficient and unordered information; stage 2: insufficient but ordered information, stage 3: sufficient but unordered information and stage 4: sufficient and ordered information. Crane (1972) couples the four stages of knowledge development to four developmental stages for scientific communication. In stage 1, when the paradigm appears, there is no developed social organization, in stage 2 when normal science flourishes, invisible colleges appear, in stage 3 with major problems solved and anomalies turning up, social splitting occurs; in stage 4 with the paradigm exhausted, the number of participants decreases. A clear link is suggeted between cognitive "states" and characteristics accessible to socio-

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(This paper is based on materials reported at the OECD-conference on Science and Technology Indicators, 1980).

	Stage 1	Stage 2	Stage 3	Stage 4
cognitive content	paradigm formulated	normal science constructive applications	-diminishing productivity -increasing number of anomalies	-exhaustion
methodological orientation	-originality -philosophical programmatic	-verification -productivity -non-philosophical	-consistency	-apologetic -philosophical controversy
literature	-innovative document(s) -preprintes	-papers	textbooks domain specific journals	-journal -bibliographies
social structure	-none	invisible college	formal groups and societies	residual groups
institutional forms	-informal	small symposia	congress and formal meetings	institutionalisation (univ-department).

Figure 1: Characteristics of the life-cycle of scientific specialties in relation to the various stages "superimposed" on the logistic growth curve (see Crane, 1972, p. 172).

metric analysis. In Figure 1, we have added an augmented table of stage-characteristics to Crane's stage-segmentation of the logistic growth curve for specialties. Manifestly, through those various stages the nature of the expectations and accumulated knowledge changes and one wonders whether bibliometric indicators could be sensitive to such changes and provide a lead to their study.

In an effort to check the accuracy of the phase-characteristics on a specific case of specialty development, we made some analyses on the diffusion of innovations paradigm. Rogers & Shoemaker (1971) contains a bibliography which covers the development of the area from its very beginning and which distinguishes between empirical studies and publications that do not report empirical results. Furthermore, they provide for a detailed list of hypotheses which express the diffusion paradigm, a listing of the bibliographic items which either support or do not support the hypothesis studied. This should allow us to verify whether indeed non empirical or programmatic studies are to be retrieved in the earlier phases and whether the negative anomalous findings constitute a substantial portion of the studies reported in the later phases.

Figure 2 and 3 show the distribution of items respectively split up in *empirical* versus *non-empirical* and *supporting* versus *non-supporting*.

With respect to empirical versus non-empirical, it is clear that the non-empirical literature seems to develop as an almost constant proportion of the whole literature, roughly one third, rather than as a category mainly to be found in the earlier stages.

With respect to supporting versus non-supporting studies, the proportion of non-supporting literature seems to increase but it is not the category that dominates the later stages.

There are several possibilities to explain why the diffusion paradigm as registered in the 1971-bibliography should not follow the paradigm lifecycle. One could argue that the specialty is

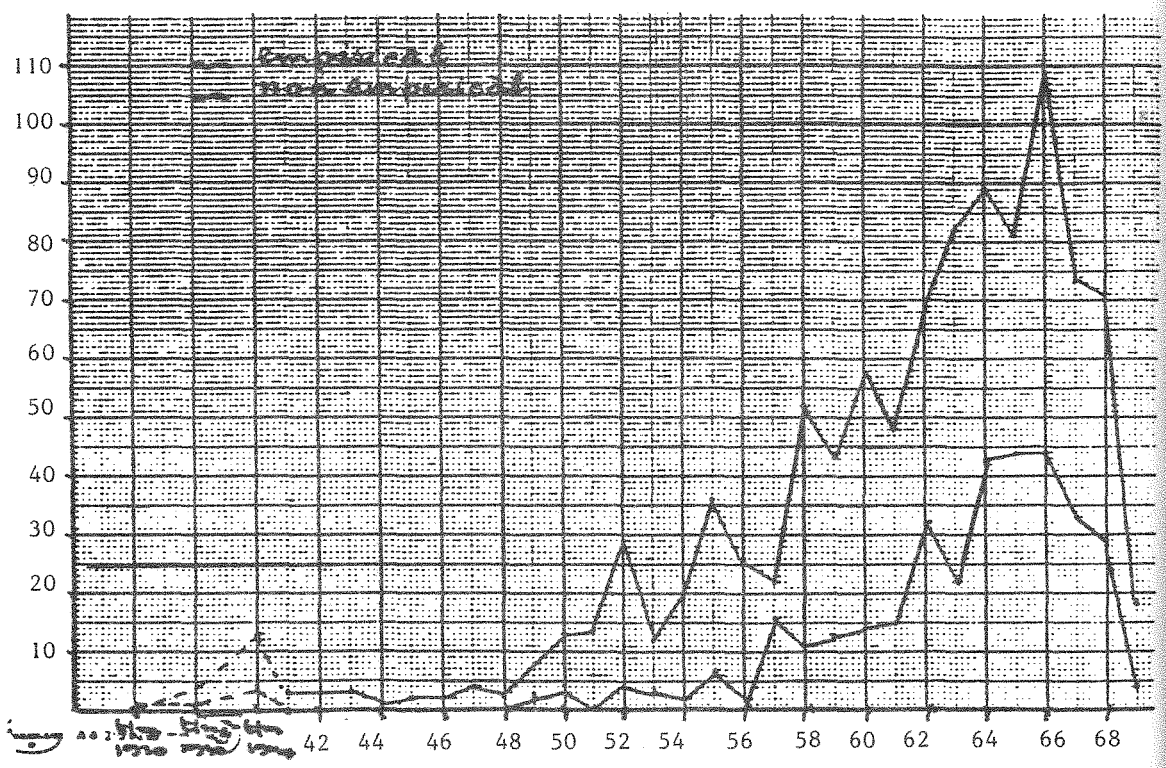

Figure 2: Distribution of empirical versus non-empirical items in the bibliograp on diffusion of innovations (Rogers & Shoemaker, 1971).

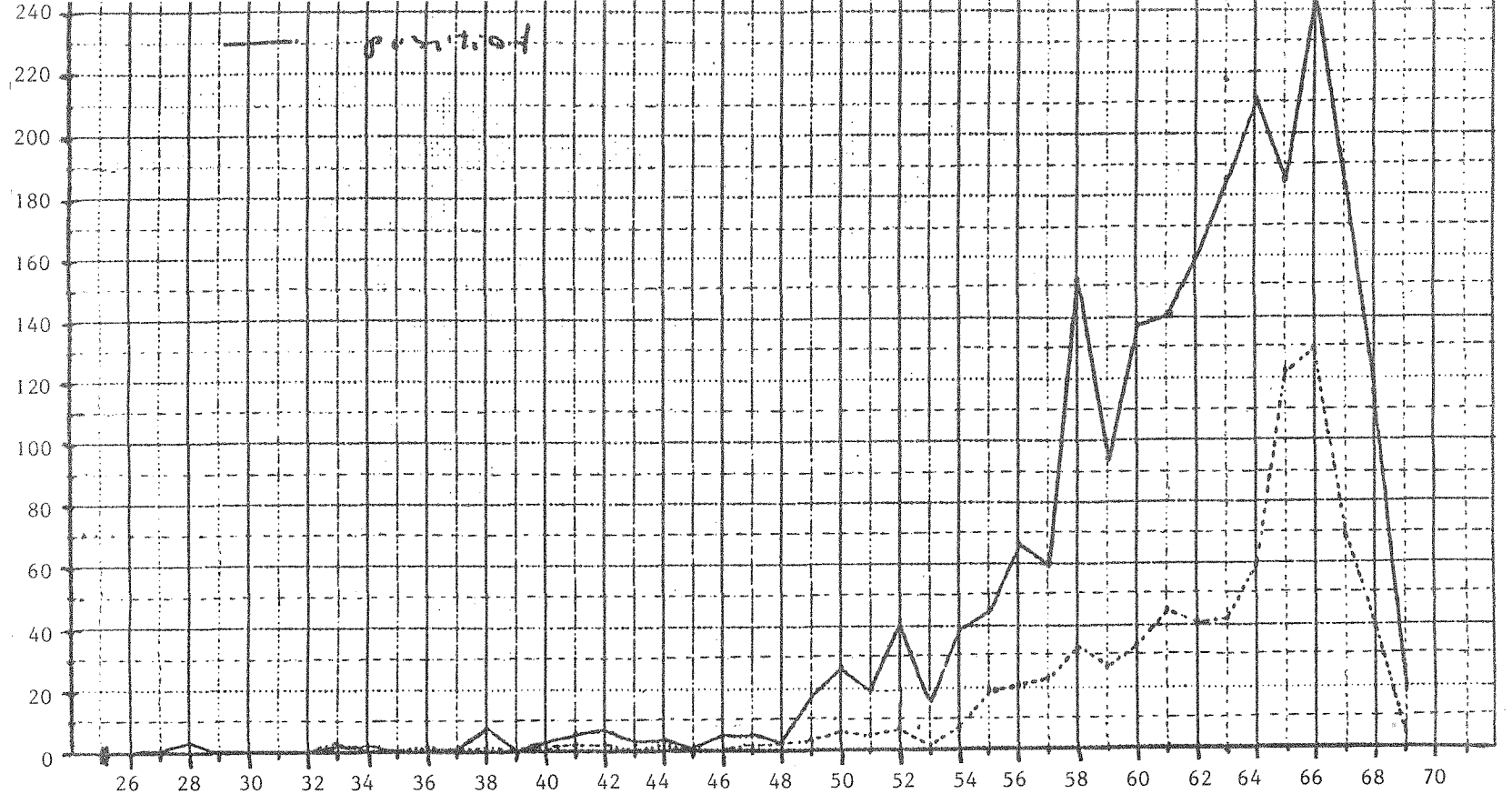

Figure 3: Distribution of the supporting versus non-supporting items in the bibliography on diffusion of innovations (Rogers & Shoemaker, 1971).

still expanding and hardly in the third phase since new domains of application are still added. An alternative formulation of this argument would be that the 1971-bibliography covers a whole sequence of related and partly overlapping paradigms which have gone or go through the life-cycle on their own.

I first applied in rural sociology with the diffusion of hybrid corn, then medical sociology with the adoption of new drugs by physicians, then marketing ... etc. Conflating this whole literature into one bibliography results in a mixing up of late literature in one sub-paradigm with early literature of another. Therefore, the classification in Rogers and Shoemaker might not be an optimal sample for tracing the development of one specialty. In order to understand the organizing and suggestive forces of a paradigm one would need to locate the real seminal papers or works and study the details of their impact on the specialty community. For such an analysis, we have found a more suitable domain in a case where there is one well defined author who is unambiguously considered the intellectual leader of a field which he revolutionized by publishing a seminal monograph: Noam Chomsky and his *Syntactic Structures* of 1957.

While the diffusion paradigm promoted by E.M. Rogers followed a rather smooth diffusion pattern, Chomsky's innovative ideas in linguistics and psycholinguistics caused much turmoil and became highly controversial. If there is a sequence of *inspiration, application, criticism,* and *rejection* in the cognitive life-cycle of paradigms, it should be particularly apparent in the reception of his ideas.

The case of the Chomskyan revolution.

In the list of social science documents cited at least 200 times from 1972 to 1974 (Garfield, 1979, p. 143), Chomsky's *Aspects of the Theory of Syntax* (1965). "scores" 382 citations. For a monograph which is not on methods, this is quite high and an undeniable indicator of influence.

We will not investigate whether Chomsky's movement qualifies as a genuine Kuhnian revolution or whether Kuhn needs to be revised

on the basis of the history of that movement. The revolution impact of his publications, in particular *Syntactic Structures* (1957), seems widely accepted. The statement of purpose for the new journal *Linguistic Inquiry* published for the first time in 1970 starts out with the statement "With the publication of Noam Chomsky's *Syntactic Structures* in 1957, the field of Linguistics began to undergo certain radical changes". The first sentence of Smith & Wilson's *Modern Linguistics* reads: "The publication of Noam Chomsky's *Syntactic Structures*, in 1957, marked the start of a revolution in linguistics" (p. 9). But while Katz and Bever (1977) indicate that "the transformationalist revolution in linguistics" which denotes Chomsky's innovation "fits Thomas Kuhn's (1962) account of scientific revolutions" (p. 11), Winston (1976) feels justified to claim the "failure of the revolution to conform to the Kuhnian account" (p. 30). For our purposes it is sufficient to notice that their is a major development in science induced by N. Chomsky and that it is manifested in a great number of references to his work. The question is whether we can trace the cognitive dynamics of that movement by means of these references and in particular, whether they provide a hint on the inspirational qualities of the innovation.

Among the hallmarks of Chomsky's doctrine is the *competence-performance* distinction. The Chomsky oriented linguist is interested in a theory of language that is independent of the psychological and social processes that are actually involved in producing or understanding language. The emphasis is on grammar, considered as a set of principles or rules by which acceptable strings of language elements can be characterized (the preferred term is "generated", from there: "generative grammar"). Grammar is reduced to syntax which occupies a central position. A syntactically generated deep structure "sentence", later on "base string", serves as a pivotal unit for both semantic interpretation and surface implementation of a sentence. The "surface structure" is the actual form a sentence takes. It is derived from deep structures (earlier version) or base strings (later version) by means of "transformations", hence "transformational grammar". The base string or deep structure is

derived with or generated by phrase-structure rules or recursive rewriting-rules. This technical apparatus which is succesfully applied to specific grammatical problems (such as active-passive and auxillary verbs) is coupled to a philosophical position of rationalism emphasizing innateness of language capacity in terms of linguistic universals. It is important to notice that Chomsky's system contains both specific technical devices such as various types of rules and philosophical positions which are, while being general, nevertheless articulated. In principle, he can be cited for either of them and the one category of his contributions does not entail the other.

The reference in Chomsky-citations.

In a review of a book that he calls "the definitive defence of the relevance of Chomsky's work to psycholinguistics" (Fodor, Bever, Garett, 1974) Johnson-Laird points out that "the book probably marks the end of an era" (p. 264). According to that view, it would not seem inappropriate to go through fifteen to twenty years of references to Chomsky's seminal works in the hope of finding successively preponderantly positive references (first general than specific) and then preponderantly negative references (first critizing specific technical problems, then rejecting the whole paradigm). But according to monographs as Smith & Wilson's (1979): "the effects of that revolution (Chomskyan) are still worked out" (p. 9). They would apparently not consider it appropriate to apply "post-developmental" characteristics to Chomskyan linguistics, all the more because Chomsky is still very active and he recently published another major work *Rules and Representations* (1980). What we have mentioned with respect to diffusion-of-innovation-studies applies equally well to Chomsky. While he might be considered outmoded in one area, another specialty might only recently have hit upon his relevance and the latter might introduce his concepts as a potential solution while the former has reached the stage of rejection.

In a classificiation of journals according to the number of Chomsky-references the *Journal of Verbal Learning and Verbal Behavior* (JVLVB) ranks probably first. (SCI does not contain *Linguage, Lingua, Linguistics* and other highly relevant linguistic journals which makes exact comparison difficult). The journal came into being in 1962 after a rather long gestation period reported in Cofer (1978) "Origins of the Journal of Verbal Learning and Verbal Behavior". Before their big impact upon linguistics, Chomsky's concepts affected psycholinguistics and the journal became a major medium in its expansion.

Because of its central position in the first wave of the Chomskyan revolution, we decided to do a detailed analysis on the Chomsky-citations in the JVLVB, in particular the references to *Syntactic Structures* (1957) and *Aspects of a Theory of Syntax* (1965). Since these are the highly cited monographs and since they can be considered as representing the official texts of the doctrine, we could expect that the pattern of references to those works over time would reflect the cognitive life-cycle of the Chomsky-paradigm. There are 171 references to Chomsky in the JVLVB from 1962 (first volume) to the 1979 volume, 44 to the 1957-monograph, 66 to the 1965-book and 61 to other publications of Chomsky. Figure 4 gives the distribution of these references over the eighteen volumes which have been analysed.

We analyzed the 110 references to the two monographs in detail and classified them with a classification scheme comparable to Chubin and Moitra's (1975). Their classification scheme distingsuishes between

- affirmative references, further subdivided into:
  - basic
  - subsidiary
  - additional
  - perfunctory
- negative references
  - partial
  - total

VOLUME JVLVB	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
References to	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79
SYNTACTIC STRUCTURES (57)	-	4	2	8	3	5	2	5	3	4	3	-	-	2	1	1	1	-
ASPECTS OF A THEORY OF SYNTAX (65)				-	3	6	8	8	8	5	10	10	2	3	-	2	-	1
OTHER CHOMSKY PUBLICATIONS	-	2	1	5	8	5	6	4	5	4	2	3	5	3	3	-	2	3
TOTAL NUMBER OF REFERENCES TO CHOMSKY	-	6	3	13	14	16	16	17	16	13	15	13	7	8	4	3	3	4

Figure 4 Distribution of references to N. Chomsky in Journal of Verbal Learning and Verbal Behavior, Vol.1 to Vol. 18. Total number of references is 171.

In our interpretation, these categories link to stage-characteristics of the paradigm life-cycle roughly as follows:

- *stage one*: establisment and recognition of the paradigm: basic affirmative references expected to it;
    - *stage two*: application and utilisation: subsidiary affirmative references;
    - *stage three*: indiciation of anomalies turn up:
        - additional affirmative references coupled to remedying suggestions for improvement of the paradigm;
        - partial negative references poiting to serious and possibly irremediable trouble;
- *stage four*: rejection of the paradigm: total negative reference.

Perfunctory affirmative references do not fit the scheme because they acknowledge familiarity with the cited reference without expressing a definite commitment to or against the position it represents.

The results are represented in Figures 5 and 6.

Before we attempt to see whether they provide any insight on the cognitive dynamics of a specialty, it should be emphasized that content analysis of citations, at least in this case of N. Chomsky is rather ambiguous.

Our results are comparable to other classifications of citations in that we notice a substantial number of perfunctory citations. This number would be higher if we would have restricted our analysis to the local context of the citation. Many citations require an analysis of the line of argument of the whole paper in order to be properly understood. Gilbert's (1977) warning with respect to the relevance of context is highly appropriate. One cannot avoid taking into account context when analyzing content and as we have argued elsewhere (De Mey, 1982) context is a glibly notion. Paraphrasing Wittgenstein, one could say "to understand one citation is to understand a whole field" (vs. Wittgenstein "to understand one sentence is to understand a whole language").

Volume / JVLVB / Type of reference	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Total
	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	
BASIC														1					1
SUBSIDIARY		4	2	2	1	1			1										11
ADDITIONAL				4	2	1	1	1	1		1			1					12
PARTLY NEGATIVE						2		1		2									5
TOTALY NEGATIVE																			
PERFUNCTORY				1			1	1		2	2				1	1	1		10
NOT CLASSIFIED				1		1		2	1										5

Figure 5: Classification of Reference to Syntactic Structures (1957)

Volume / JVLVB	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Total
Type of reference	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	
BASIC																			
SUBSIDIARY					2	3		2	3	1	3	2							16
ADDITIONAL						2	4	2	2		6	5		2					23
PARTLY NEGATIVE						1				1		1				1			4
TOTALLY NEGATIVE																			
PERFUNCTORY					1		2	2	3	2	1	1	2	1		1		1	17
NOT CLASSIFIED							2	2		1		1							6

Figure 6: Classification of References to Aspects of a Theory of Syntax (1965)

Nevertheless, the classified citations seem to confirm the life-cycle model of specialites, although rather weakly and only for stages two and three. The bulk of the citations links indeed to constructive or critical use of the cited monographs. The prominent feature of these citations is that citing authors take great caution not to express too strong a commitment or too strong a rejection. The general line of argument is: "given the evidence I have assembled, there is something to say for Chomsky's notion of --------" (subsidiary citations) or "there is something to say against Chomsky's use of ------- and some correction or alternative needed" (additional citations). Furthermore, citations which identify generic concepts of the cited autor are rare. Most authors cite a technical concept or a particular position of the cited author and avoid attempts to come to a global grasp of his contribution. Surprisingly, the rare exceptions come rather late in the specialty life-cycle and not at the beginning where we expected them.

In a 1976-paper of Smith & Baker, we find a general characterisation of Chomsky's endeavor in the following terms: " ... The reason for this claim derives from a characteristic argument that pervades much of generative grammar: If we attempt to write rules, so the argument runs, with reference only to overt linguistic forms, we can succeed in producing only a very complex system with many irregularities, and we gain little insight into the general principles that might govern linguistic structures: however, if we allow ourselves to assume the existence of underlying linguistic forms that are not directly observable, then a significant simplication of the rule system is possible, and we stand a better chance of discovering general linguistic principles" (p. 267). Manifestly, it is this two-component-model with "deep structure" and "surface structure" which inspires the first generations of Chomsky-users contributing many of the subsidiary citations. However, their citations do not identify that generic notion in Chomsky. Rather uncritically, they take the two-component-model for granted and apply it straightaway in its technical details. A citation which

aims at identifying a generic aspect of Chomsky (1957) does so (in 1975!) in blaming current literature for still adhering to associationism while acceptance of Chomsky would mean its rejection.

If, in general, generic aspect of frequently cited text, would be revealed only relatively late in the life-cycle of paradigms, the analysis of citation contexts would not be very instrumental in discovering the suggestive and guiding role of these popular documents. One is indeed surprised to find how heterogeneous the citation contexts are which focus upon the same book. Small (1978) suggested a study of citations in chronological sequence in order to follow the narrowing of meaning which occurs with respect to such documents.

For the series of citations we have analyzed, it is far from clear how this occurs. The first generation of citing authors does not justify its citing on an identification of the generic aspects of the text. The relevance and importance of the cited text is apparently taken for granted. The second and third generation of citing authors engange in detailed and rather fragmented analysis. A few fourth generation citing authors expresses a global view on the contribution, grasping a glimp of the generic concepts which we consider to be underneath the suggestive power of paradigms. The majority of the fourth generation authors however plays safe by restricting to perfunctory citations. If only few citing authors acknowledge rather late in the life-cycle the generic value of the highly cited publication, what then drives the majority of citing authors?

The absence of explicit citations in the first stage of the paradigm life-cycle is not incompatible with the augmented Crane-model. To the contrary, informal communication definitely dominates. Therefore, in order to see how a cluster of high citing authors forms around a highly cited document, it might be required that we pay more attention to the informal communication process of the first generation of highly citing authors. In the case of Chomsky, it cannot be sheer coincidence that almost all these authors were related to Harvard's Center for Cognitive Studies or

M.I.T. The "negotiation of meaning" that went on there before it became reflected in high citation frequencies should be crucial in our understanding of nascent popularity. We need to know the cognitive basis of popularity in order to understand what indicators based on popularity measures (including co-citation networks) mean.

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TECHNOLOGY AND HISTORY OF PSYCHOLOGY

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Summary

In general, histories of psychology present psychological investigation as if it were exclusively scientific, and do not take into account the existence of differentiated psychotechnological investigation with its own epistemological status. Science and technology in psychology are closely related but they form two different activities and have related in different ways through history. Psychological historiography should study this matter in more depth.

The Problem

When the history of psychology or contemporary psychology are presented from the academic standpoint, they are conceived as an almost exclusively scientific discipline. Generally, historians of psychology fail to recognize the *technological research* which founds the technics of professional psychologists and attempts to optimize it. They fail to recognize it, at least, as a specifically differentiated body of research. In some way, they confine themselves to considering technological research and technics under the fallacious label of "applied psychology". By this approach, research is valid, legitimate and worthy of consideration in scientific psychology, to the extent that scientific-psychology methods and theories are applied to the resolution of socially relevant problems.

This attitude is present in the main historical presentations of psychology (Boring, Carpintero, Hehlmann, Marx and Hillix, Misiak and Sexton, Murphy, Lundin, Wolman, etc.). There is no preliminary discussion in them of the respective "status" of science and technology and their relationship, and no differentiation between scientific and technological theories. Without this, everything is reduced to the systematic presentation of some psy-

chologists' work (Allport, Bruner, Guthrie, Murray, Piaget, Pavlov, Rotter, Skinner, Vygotsky, Wertheimer, ...) with some later indications about the clinical or educational "application" of their ideas as if the application to practical problems would happen without mediating research whatsoever. This attitude, while failing to recognize technology, reduces research to scientific research without recognizing technology, and is also reflected in the treatment of differential-correlational psychology in the historical presentation of psychology.

We believe that the reconstruction of the history of psychology demands a restatement of this attitude. To reduce psychology to a *science* in the strict sense, would mean leaving out of this history a large part of the research activity of psychology. To understand psychology technics as a simple "application" of scientific psychology would be tantamount to depriving the history of psychology of its meaning by denaturalizing the specificity of the technological research. Reconstructing the history of psychology implies a critical reflection on the nature of science and technology as different research activities with mutual and complex relationships.

Need and Urgency to Confront the Problem

Given the increasing technological orientation of psychological research, this reflection becomes more urgent and necessary. This orientation may be understood in the light of the increasingly known number of professional psychologists. It is evident that academic work and research receive *retroactively* the impact of problems and to the technics of professionals and come out of the immanent internal dynamics of scientific research itself.

There are many objective indicators of this *technological orientation*. Thus Tortosa (1980) has studied quantitatively the evolution of *psychological topics* of research using *Psychological Abstracts* from 1927 to 1978 as a source. Tortosa verifies the increasingly applied and professionalized character of psychologi-

cal publications since 1948. Pascual (1980) has also used *Psychological Abstracts* to study the most productive psychologists and the predominant topics of their publications between 1969 and 1976; his findings point in the same direction as those of Tortosa. The prevalence and omnipresence of the labels related to behaviour modification and/or behaviour therapy in an increasing number of manifestations, such as journals, books, scientific meetings, ... is another important indicator of this trend (cf. Carpintero and Peiró, 1980; Peiró and Carpintero, 1981).

Science, Technology and Technics

Herrmann (1979) distinguishes three types of psychological activities that we consider of interest to the historian of psychology. They are the following:

a) Psychology as a set of psychological activities of a *non-researching* nature which, in a different way (e.g. applying a test, modifying smoking habits), use psychological knowledge for practical purposes (e.g. diagnosing, improving behaviour) and do not *directly* attempt to increase psychological knowledge, even if the technics used have developed within academic psychological research.

b) Psychology as a set of psychological activities of a *basic-scientific* nature. Within this "pure" science two basic types may be differentiated: the "domain" type of research (Shapere, 1964) where we start from the problematization of a *thematic domain* (e.g. semantic memory, language acquisition, etc.), and *explanation* or *explanatory tools* are looked for; and the paradigmatic or quasi-paradigmatic type of research where we start from a methodological, ontological and theoretical nucleus of principles and ways are sought to articulate and apply them to progressively larger areas of a scientific discipline.

c) Psychology as a set of psychological activities of a technological (non-basic scientific) research nature, but in principle just as innovating, genuine and authentic as basic-scientific research and using the same methodological principles as this.

Traditional academic psychology of the differential-psychometric type and significant psychological fields like educational or clinical psychology would belong to this technological activity. The purpose of this activity is to obtain rational rules and criteria which may guide the practice of "professionals" and improve and optimize their technical skills. The existence of this psychological technology as a field of research with its own goals makes it inadequate to consider the activity of clinical or education psychologists as simply "applied" psychology. The technics and procedures professionals use in their technical activity are *mediated* by an activity which is genuine research. This constructs theories and models as well, if only the rationality of its decisions is based on their own *values* and *criteria* (usefulness, efficiency, possibility of application to concrete situations, etc.) and which are not the same as those of scientists (audacity, contrastability, novelty, conceptual precision, heuristic fertility, etc.). Besides, the problems it attempts to solve come from professional practice and refer to material, social and psychological needs.

This technological activity, generally performed by scientifically trained people, makes use of scientific procedures and normally *develops* its own models and theories - especially at present - upon the *theoretical knowledge* that basic research provides. In simpler terms, we could say that from laws it formulates *rules*. Not forgetting, nevertheless, that it selects the laws according to its own objectives and that sometimes technology itself contributes with theoretic ideas to the basic research (let us consider for example the influence of Thorndike's "educational" psychology on the basic resarch of neo-behaviourism).

History of the Relationship between Science, Technology and Technics

It will be wise, however, to consider the relationship possible between science, technology and technics from a historical perspective. Kuhn (1977), after stressing that science and technology had always been separate until Bacon proclaimed their union, and so they remained, despite the proclaims in the Baconian tradi-

tion, pointed out three types of relationships: one going back to antiquity, another to the middle of the 18th century and the last one to the end of the 19th century.

The *first*, which may occur at present only in the social and behavioural sciences, lies in the influence of already existing techniques, whatever their origin, on the sciences. The new sciences of the 18th century (e.g. chemistry, magnetism, etc.) and the thermodynamics of the 19th century are typical examples of it. In psychology it would be the tradition of animal magnetism and other educational and clinical techniques. In general, science has benefited from this and improved its knowlegde of nature and contributed to explaining and understanding technology itself, but has not necessarily improved in its efficacy.

A *second* kind of interaction initiated in the middle of the 18th century is the increasing use of *scientific methods* and of *scientists* in technology and practical trades. They are conscious attempts by scientists to apply scientific methods to social needs; they might or might not be scientifically relevant. By this approach initiated in the 18th century, theories and discoveries meant hardly any changes of the techniques, though no doubt, the understanding and nature of them improved. Psychometric psychology and most of the so-called "applied" psychology up to 20 years ago may be integrated in this kind of relationship wihtout having worn out yet.

The third kind of relationship emerged at the end of the 19th century. It consists of a technology whose procedures and products derive from theoretical and empircal results from existing scientific research; its development being dependent on research through forces scientifically formed. This phase started with Swiss and German coloring industries towards the end of the 19th century and radically changed the production and distribution of energy, medicine, war, etc. It constituted "big science" and its omnipresence and importance conceals the differences between science en technology. Some recent developments in intervening, educational and clinical psychology should be interpreted from this point of view.

An Approach to the History of Psychology

In any case, in the light of these systematic and historical considerations we shall outline what could be an attempt to integrate psychological technology in the history of psychology.

Psychology as a technological and scientific discipline and as a profession acquired its identity from psychology as a *science*. Yet, it had already had then a long *past* scattered through philosophy, multiple techniques (educational, clinical, commercial) and other activities (literary, etc.). To think that psychology absorbed such a variety of knowlegde immediately, then, or even otoday, is an illusion. Soon though, some scientific psychologists became interested in practical problems (Ebbinghaus, Scott, Hall, Münsterberg, etc.), with their science and scientific optimism in the Baconian fashion. But here we should distinguish, first of all, the case of some professionals with a stricts scientific formation who were interested in techniques already in existence. Such is the case of Freud who, as a physician, is interested in hypnosis and from the technical use of hypnosis develops a technology and little by little a science, both being closely related in his work. It may be perhaps for this reason that the technique of hypnosis itself does not improve much, for being very close, although it became more understandable and justifiable and science and technology benefited considerably. For years, psychoanalytic technology was the only technology in the therapeutic field.

Secondly, we have the case of scientific psychologists who approach practical problems with their scientific instruments (Binet, Meumann, American functionalists, etc.); they believed psychology could, as a science, provide instruments to solve practical problems. But all they really did, was to apply their *scientific-psychological methods*. Through the *psychometric* tradition they obtained unquestionable success, which today is still continuing. What it could not do was to found a technology based on the theoretical results of their research. Scientific psychologists (Judd, Thorndike, Meumann, Stern, Claparède, etc.) did indeed try to improve their educational, clinical or industrial techniques,

and even though they contributed with language, concepts and some "experiments", which helped to understand certain old techniques, they did not succeed in improving them noticeably. The modesty of their achievements has been made to stand out many times.

Since the late 50s, the situation has changed radically. The third form of interaction indicated by Kuhn where technology makes use not only of the methods of science but also of the results, has come into sight. Today, we can speak of psychology as a real weft of programmes of scientific and technological research. In the last 25 years, with the orks of Wolpe, Skinner, Eysenk, Mowrer, etc., academic psychologists were faced with the interventionist demands of professionals who did not agree with simple diagnostic tasks, and in the light of the achievement and results of neo-behaviourist acquisition, decided to work out, without interruption, techniques with which to confront practical problems through the work of theorists of acquisition. Moreover, in the last decade or so, this technology has tried to base itself on the most recent but still immature cognitive psychology. In this regard, cognitive psychology is running the risk of working under the increasing pressure of technological demands and not following the rythm, that its own immanent dynamics as a scientific investigation should impose. Here we touch on a subject towards which the psychology historian, with the perspective that his own discipline offers him should contribute some elements of reflection on practical research in psychology. This research must be scientific and technological and be in both cases related, yet aware of the different objectives. The lack of reflection on the differentiated nature of these objectives - the technological and the scientific - may lead to a loss of identity in psychological research itself.

In any case, we think that the psychology historian must not be alien to the issue here discussed. He has to think about it in his praxis and be productive in his historical reconstructions. Our reflections may help to work out a research programme about the place of technology in the history of psychology.

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CONTEXT OF DISCOVERY AND CONTEXTUAL HISTORY OF PSYCHOLOGY

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For some time philosophers of science have been pointing out the importance of the so-called 'context of discovery', a stage in the development of science abused for long in the received view of the philosophy of science. According to this received view with its theory-oriented bias, the task of the philosophy of science was to test the scientific products (theories) by following a number of methodological rules. These rules did not apply to the discovery of theories and since methodology was supposed to furnish criteria for rationality, discovery was considered irrational and was kept out of doors.

Since Hanson and Kuhn, however, philosophers of science took an interest in science dynamics and particularly in the generation stage. Initially discovery was still described in terms of 'Gestalt switch', 'flash of insight', 'spontaneous creativity', etc. Thus, it would appear that the philosophers reconciled themselves with the irrationality of the discovery stage.

However, along with the exposure of the shortcomings of the old positivistic-popperian methodology, the discussion moved towards the concept of rationality. It was questioned whether rationality could be identified with 'logicality'; whether rationality was a matter of logical argument. With that, the odium of irrationality which rested on discovery was rolled off and the c.o.d. became a honest topic for many philosophers of science. Recently one of a growing number of "Friends of Discovery" even wrote about those matters in terms of a break-through (Nickles in Nickles, 1980b).

In this paper I would like to show that, hidden behind this intensified interest in scientific discovery, there may still be found a considerable amount of positivism/scientism . '(Context of) discovery' is a conception too narrow for the history of science. Furthermore, I hope to show that a so-called 'contextual' history of psychology can be of interest, not only for a small number of specialists, but for others as well.

Why is 'discovery' not the best concept for the history of science? In the first place 'discovery' suggests a too hard-boiled realism. Scientific realism is much debated nowadays in philosophy of science. Since some of the assumptions of positivism are refuted and no longer guarantee a stronghold for certainty, realism has been reformulated continually especially by those philosophers who abhor subjectivism. But the dichotomy of objectivism/subjectivism is one of the myths with which the history of philosophy had saddled us.

Realism has dominated in our western scientific and daily outlook. We have learned to externalize reality. Everything we study we make an external object. We invest it with its own reality independent of ourselves. This is understandable when the objects are toucheable 'real' things. We give them names and the names refer to them. But our beliefs, stories, views, or scientific theories about reality are full of concepts, classifications, distinctions of which it is doubtful whether they refer to something out there in a straightforward way: information, love, inflation, power, function, communication, agression, subjectivism, economy, culture, emotion, consciousness. We are apt to think that they pick out pieces of reality.

What kind of certainty do we have for thinking that those concepts cover pieces of the world? Traditionally the candidates for such a certainty-base were sensations. Somehow, according to this common realism, the concepts we use must be based upon sense experience. Somehow causation must come into play in the formation of concepts. Our senses must be the gates that let in reality out there. We have to observe well in order to establish the contact between subject and object and to achieve certain knowledge. But what are the sensations that cause concepts like 'power' or 'function'.

Sense experience forms just a slight part of the information we get. Experience is not sense experience only. When I perceive an elephant in a heavily built cage, I handle concepts and beliefs like 'elephant, zoo, captivity, very strong animal, heavily built

cage is necessary, otherwise ...' etc. Those concepts and beliefs are not based on sense experience only, not now and not in the past when I learned them. "There is more in perception than meets the eye" (Hanson). That is what makes computer simulation of understanding natural language in artificial intelligence work so difficult ("impossible" Dreyfus would say, 1979). In order to make an artificial system really understand even a silly and simple joke about an elephant and a mouse walking on a wooden bridge etc., we would have to teach it a lot about elephants, mouses, wooden noisy bridges, about the relative slight noise a mouse makes compared with an elephant, about jokes and what not. The experience needed to understand a simple joke or to perceive and to make sense of what is seen and heard in a simple daily situation is very large. To understand such a situation is to understand a rather large context. And this context is not made up merely by sense experience. Only in a very preliminary way are the data coming from outside and received by our senses the causes of our beliefs or facts. The idea of causation by the world out-there is a remnant of old fashioned mechanistic and materialistic epistemology; an effort to shake off the spectre of subjectivism, relativism and uncertainty. No doubt we know by experience. But the learning of beliefs and the formation of frames of mind can not be explained by a simple empiricism.

For an artificial 'semantic engine' to understand the world we see and about which we talk, it should, as it where, have had the same experiences we had. It should have grown up with us, in a world of communicating human beings. It should have learned the concepts, classifications, distinctions etc. we have learned in order to cope with our environment and to answer the questions we ask. What it should have learned is not information that comes straightaway from the outside world, an objective unchanging information, but it should have learned *our* information. That is, the way *we* make sense of the world.

The concepts we use are ours. We are present in them, so to speak. We are present in our beliefs about the world no matter how objective we think they are. We are present in the objects we think

the world is composed of; in the beliefs on what there is. Especially in science we invent new concepts every day. We make beliefs. And to a certain extent we invent the reality we cover with our descriptions and explanations. This is the inevitable subjectivist side. The whole problem, of course, is to determine in what way and to what extent the subject is involved in the knowledge of the world. Philosophers and psychologists, from Kant to the Gestaltpsychologists, from Marx and Mannheim to the phenomenologists and the cognitivists took on this job. I can not see how we could leave out the historical and social dimensions of this subjective side of the picture. 'Subject' can not be an individual. Communication, beliefs and language are formed and changed by a community; Wittgenstein and others have made this perfectly clear. Somehow we have to bring this social dimension in epistemology and in our understanding of science.

On the other hand, that worldviews are made by men does not preclude that there is a mind-independent world. Neither does it preclude that part of such a view somehow refers to that world, because no doubt the world contributes to the making of worldviews ("The mind and the world jointly make up the mind and the world", Putnam 1981: xi).

This mixture of subjectivism and objectivism affects the meaning of 'truth'. Truth in some absolute sense is unacceptable. Truth, and rationality too, is a function of the beliefs of a particular community of thinkers and doers. A function of time, place, and context. Of course we meet the mind-independent world in our actions, and we check our beliefs in these confrontations. Because of the world we change, accept, or reject our beliefs and propositions about what there is or what is happening. But we never will be able to determine theoretically the role of the world in the formation of our beliefs, because we cannot go out of our minds. Observation, verification or falsification are never mind-independent. Fortunately, we can live rather well without this philosophical scepticism. People share lots of beliefs within a community. Although we have to admit, in general, that a truth-

statement is relative, if we have no reasons to doubt a particular belief then it is the best truth we have and then we are rightly prepared to act upon that belief. We cannot live without beliefs, without truths. In a Humean mood we could say: "Philosophy, splendid, but we have to live". Still, as our concern is the history of science we cannot afford to avoid a stiff dose of scepticism.

Does science search for truth? Some philosophers contend that it does not, in order to avoid presuppositions about the veracity of scientific statements. But with enough relativism at hand there are no objections to saying that science, among some other human activities, searches for truth. I cannot agree with Laudan asserting that "the single most general cognitive aim of science is problem solving" (1977: 124). It is not unusual that theories pose the very problems for which they find solutions. Psychoanalysis is an example. Problems are part of theories. The appraisal of the rationality of a theory by "an analysis of the empirical problems which it solves" (o.c.: 124) looks like the onetime invoking of a transcendent truth. Problems, however, are not eternal; are not without history. Even the assessment of contemporaneous competing theories (or 'research traditions' as Laudan will have it) e.g. behaviorism and psychoanalysis in the beginning of the twentieth-century in America, by weighing out the problem solving power of each theory, can hardly be done, to put it mildly. Is it really possible to "determine whether our theories now solve more important problems than they did a generation or a century ago" (o.c.: 127)? Laudan tries to rescue the idea of scientific progress from the wreck of positivism. For him scientific progress consists in "the solution of an increasing number of important problems". But like truths we cannot disjunct problems from their contexts.

Of course, science is also a problem solving enterprise. That concept is valuable because it implies action. Actions are the natural consequences of beliefs (truths), and science as a whole bundle of beliefs is a powerful source of actions. This practical side of science is one of the reasons why science and interests are mingled. It is the intention of science and scientists to 'find'

truth and to solve problems. But to understand this in a study of science one has to search for the practical reasons behind this intention. History of science, therefore, is not only a study about truths, but foremost a study of human interests in a particular time, place and context.

So, 'discovery' has a too realistic and a-historic flavour: of something that is 'there', of fixed disinterested truths, of pieces of reality we can find if we observe well. We could use, however, the term and the concept in a sophisticated way meaning the discovery of conceptions (models, theories etc.), not of realities. So the concept could be harmless if there were no other objections.

Discovery-orientation contains an a-priori appreciation of what is discovered. In other words, a discovery-oriented history of science runs the risk of selecting those discovered units that have had 'success'; that have 'promoted' the scientific discipline. What counts as a success is, just as the measurement of success, determined by yardstick of the present. Sure, we cannot avoid presentism altogether. What is more, I question whether 'Einfühlung' or 'hermeneutics' has to be the most important virtue of historiography, because it could strip history of its meaning for us. To impose, on the other hand, deliberately, say, one's own version of rationality upon the past, as Lakatos would have it in his conception of the rational reconstruction of theories, is far too much presentism. Hence, we have to steer carefully between the two dangers and to be as aware of our own categories, concepts etc. as we are of the concepts of the periods and communities we are interested in.

Discovery suggests units too narrow for research in the history of science. What has been discovered is rather circumscribed: theories, by preference, or entities (e.g. the mental test). In the so-called received view of the history and philosphy of science exclusive attention was paid to theories as the finished products. Now, as the shift is to discovery, the formation of theories is the subject of research. Still, one sticks to theories supposing that it is the task of the history of science to evaluate either their claim or approximation to truth or their problem solving progressi-

veness. Instead of the onetime evaluation of the finished product, whether they come up to alleged universal methodological criteria, a history of the discovery of scientific theories runs the risk to be concerned only with the way of thinking, only with the step by step sequence of thought: *how* somebody came to a theory. Discovery is identified with reasoning.

To stick to theories and to reasoning is having the conception of rationality as ratiocination. But to have reasons for a belief is not always reasoning. We can have reasons without reasoning, even as scientists. Moreover, in every rather complex reasoning process beliefs sneak in which are so taken for granted that the scientist accepts them, without questioning, as premisses. And precisely those assumptions may be very revealing. To trace the source of those beliefs comes to leaving the path of logical reasoning.

Having the logical conception of reasoning commits one to thinking either that real science boils down to reasoning (besides observation), or that some scientists are not always reasoning but then are irrational and unfortunately a prey to all kinds of influences that do not earn them the honorable title of scientist at all. Laudan for instance in his discussion of David Bloor's so-called 'Strong Programme' of the sociology of science (Laudan, 1981 and Bloor, 1981; Bloor, 1976) contends that we have to distinguish between rational and irrational beliefs. Rational beliefs result from a proces of ratiocination and reflection. A rational belief is rational "provided the agent can give reasons for it and can show that those reasons were antecedent to the adoption of the belief" (Laudan, 1981: 187). But reasons for a belief are other beliefs. Beliefs are part of whole systems of beliefs. To give reasons cannot be giving the whole system of beliefs (not to mention the regressus). Most of a worldview we take for granted; and scientists are no exception. This is why 'ideology' is a useful concept; not in a political or conspirational sense but meaning that many reasons (beliefs) we have for beliefs are unreflected and not stated

in giving reasons. There is a difference between *giving* reasons and *having* them. Usually one has more reasons than one gives.

To have reasons is to share to a certain extent the beliefs of the community or the sub-community (like a group of scientist) one belongs to. That is what rationality is about. One cannot justify one's beliefs if the reasons are not, at least, recognizable within the belief-system of the community. Although someone does not give all the reasons he has or does not actually reason, he cannot be called 'irrational' for believing someting or acting in a certain way if his beliefs or acts fits in the worldview of his community. 'Non-rational' or 'a-rational' will not do either. In order to express disagreement with particular reasons someone gives or has, one can resort to 'bad reasons'. In short, we have to avoid using 'irrationality' in the history of science. And this is possible if we strip the concept of 'rationality' of all normativity. Laudan's "modest notion of rational action and rational belief" (o.c.: 187) is not modest enough.

As already stated, behind this idea of rationality as a well-founded reasoning process on the part of the believing agent, is the assumption that, if a scientist is irrational, i.e. is not reasoning, he unfortunately must be a prey to all kind of influences. Laudan thinks that a belief can be "caused" either by a reasoning process or "perhaps by the direct action of social and psychological forces unmediated by reasons" (o.c.: 188). That only if beliefs are irrational, i.e. unreflected, they are liable to sociological analysis. "Until the rational history of any episode has been written (...), the cognitive sociologist must simply bide his time", he points out in his book (Laudan, 1977: 208-9). The sociologist of science, it seems, can pick up the garbage thrown away from the table where philosophers and real historians of science are consuming the neatly discovered theories.

However, as suggested earlier, all kinds of beliefs sneak in inadvertently in a reasoning proces. A scientific line of argument is not scientific because it is transcendent, exalted above the petty, bustling world. On the contrary, it is scientific because a

community accepts it as such according to certain criteria. What counts as scientific are not a-historic reasons. Many 'scientific' reasons are based on 'plain' reasons which are part and parcel of the worldview, the mode of discourse, or the system of meanings of a community. The formation of a belief is never either wholly scientific or wholly social/psychological. Scientific and social or psychological reasons are mixed up. The 'self-explanatory picture' (Bloor, 1981: 205) of science cannot be accepted anyhow.

To reach the interests behind the theory, *why* a scientist came to the theory, we have to look for unexpressed assumptions, and we have to dig for hidden intentions, not mentioned in the finished and balanced product, not even in the scientist's report of the discovery. To do that, the historian has to get accross the borders of the theory. (S)he has to follow the track of concepts, ideas, methodological demands, or to appraise the pressure of 'global theories'. In this context 'discovery' does not make sense anymore. Because it becomes clear that the unity of the discoverer, and the unity of the place and time of the discovery is a myth indeed (Grmek, 1980: 19). To understand, for instance, why Watson took 'control' for granted in his version of behaviorism, the historian has to trace that concept in the American society around 1900.

When considering discovery one is in danger not to take account of an important stage in the development of a science: the stage of the understanding, the clarification, and above all the reception and transformation of the 'discovered' unit. Consideration to discovery is also theory-biased in the sense that one does not have an eye for the practical field. Certainly, not every theory is or can be applied. But theories, in a broad sense, contribute to the frame of mind, to the beliefs upon which actions take place. The historical perspective on that practical side of the studied discipline is important in order to understand the discipline as a whole.

The recent consideration to the so-called 'context of discovery' has strong philosophical intentions, that is to say, methodological aims. Underlying all this are assumptions deriving from the

positivistic-popperian tradition, notably the view that it is the task of the philosophy of science to be 'critical-normative'. Philosophy of science, in this conception, has to be after a contert-neutral methodology, a number of rules, not of theory construction this time, but of discovery. For that purpose, most attention is given to the reasoning proces, the hypothesis-generation, and to the formation of a problem-solving algorithm. There again is the time-honoured scientistic concern with control and efficiency, with generalization, out of fear of relativism and historicism. The fear that science would be a prey to anarchism or would be liable to political or social-psychological pressure is exaggerated. It is the result of a stiff dichotomy between realism and relativism. A radical standpoint on either side is untenable.

I shall now say something about what may be called a 'contextual' history of psychology. There are no context-independent knowledge claims, because all knowledge is social. Now, context can be everything. So, we have to search for that context that can clarify the origin and the sustaining of the knowlegde claim.
The concept of causation is not very useful here. There is a meaningful distinction between the cause and the reason for a belief or a disposition. I do not reject the empirical reduction of mind to brain (Boden, 1972; Putnam, 1973), but there are two different answers to the question: why is x angry? It would be better to save 'cause' for the physicalistic explanation, say, in terms of brain processes. An historian will not be interested in the 'cause' for Locke's belief that "the understanding can no more refuse (the simple ideas) than a mirror can refuse (...) the images or ideas which the objects set before it do therein produce" (Locke, 1960: II, i, 25), but he will search for the 'reasons' for this mechanistic picture of perception. On this I take issue with both Laudan and his adversaries, the Edinburgh sociologists of science Barnes & Bloor. Social events, structures, forces, or economic processes can not cause beliefs directly. That idea is a product of the same mechanistic epistemology I mentioned earlier: the world out there

causes in a straightforward way what is in our head. But we form opinions about what is socially going on; and with these beliefs we form other, say, scientific beliefs. I do not deny that beliefs too are caused, for the causes of beliefs, that is other beliefs, are 'represented' somehow in the brain, but to explain in terms of causes, provided it is feasible to do this, is not the right answer to the historian's question. To find reasons is not to explain causally. We have to be content with less certainty in the history of science (as in most of the behavioral and social sciences).

There are four objects or units which may be chosen as a startingpoint in a contextual history of psychology. First, there are the *knowledge claims*. The historical-minded philosophers of science found out that 'theory' is a too strict or too narrow object for research and they proposed 'global' theories such as 'paradigm' (Kuhn), 'research program' (Lakatos), and 'research tradition' (Laudan). For reasons I can not go into in this paper I prefer Laudan's 'research tradition'. But what is more important, it is far better not to confine oneself beforehand. So, there are ideas, models, metaphors, problems, theories and research traditions the context of which can be studied succesfully. An example of the latter is mechanistic philosophy with sub-traditions such as mechanistic optics (Descartes), mechanistic chemistry (Boyle), mechanistic fysiology (Harvey), and mechanistich psychology (Hartley).

What has to be studied is: (a) not the reasoning to, in the first place, but the reasons for ideas, theories etc. The concern here is with 'justification' in a much broader sense than in the positivistic 'context of justification'. 'Rational' here is not formal, but refers to content: the (good) reasons scientists have had or stated for their opinions, explicitly and, more often, implicitly. "(Good) reasons", that is to say, not according to the conception of reality or the moral standard of the historian in the first place, and certainly not according to unchanging principles, because there are no canons like that. But of course, the historian

after having explored the reasons, will not and can not avoid evaluating them. He will assess the reasons according to his own standards. This is an important incentive to do history of science: can we share the belief, and if so, for the same reasons? For the question of truth cannot be dismissed, as I suggested earlier. It is the human predicament to be forced to combine a notion of relativism with a belief in truth. Thus, there are two acts on the part of the historian: tracing the reasons of the knowledge claim and evaluating them.

Besides scientific, there are all kind of reasons: philosophical, religious, political, moral. That scientific knowledge, like all knowledge, originates in a social context does not mean that there are political reasons for every scientific belief. A contextual history of science units internal and external history. Anyhow, to confine oneself to the discipline, or to science would be short-sighted.

What is to be studied in addition to the 'reasons for' is (b) the impact: in what ways the ideas, theories etc. have directed research or actions, as well as scientific as political/social. And further, the reception and possible transformations of the ideas.

All this may be called a contextual history of ideas. It is not new; it resembles the work of Edgar Zilsel, among others. But in the history of psychology much has to be done. There is a need for an animated history of psychological ideas; a history of ideas not abstracted from general history, not disconnected from believing agents. But there is more. The next units might be (and are mostly) studied in their own right. But on closer examination they are involved in the formation of knowledge and form an inherent part of the context of ideas.

The second object in a contextual history is *the scientific discipline as a social institute: the scientific community*. Here the science-sociological factors and social-psychological mechanisms are studied which affected and directed the development of the discipline: e.g. professionalization, the role of the scientific institutes, the channels of publication etc. This kind of

history is not only important for the sociologists of scientific communities but also for the historian who is interested in the vicissitudes of ideas. The modes of organisation, the distribution of money, the policy of editorial boards etc. may have everything to do with the pushing or thwarting of ideas and methods. These factors are related to the keeping of professional vested interests, to tradition, status, prestige and even power. To put them aside as 'non-cognitive' sociology of science (Laudan, 1977: 197) is premature. Which factors did in fact belong to the context of a particular scientific knowledge formation can only be established by historical analysis.

A third unit are the *fields of practice*. With regard to psychology, a vast professional area liet out of the academic domain. Analysis of the way psychological knowledge has been used and of the social role of psychology as a practice and as knowledge should not be ignored in a contextual history. In this context knowledge is influenced by political circumstances, social needs, problems and interests. The dialectic relation between knowlegde and practice may result in the constitution of psychological phenomena and problems and in the flourishing of concepts and (would-be) solutions. Many knowledge claims related to the concept of intelligence can serve as an illustration of what is suggested here (Chase, 1980). A typical political and moral question, and a question of science-historical interest all the same, is whether a particular psychological theory or practice has been a social technology in order to repress a certain social factor unwanted in a particular context.

The *leading researchers and practicians* who influenced or controlled the discipline by their capacities and/or power are the last unit in a contextual history. Far from claiming that 'great men' are the key to the intelligibility of the history of science, outstanding biographies make it perfectly clear that we can learn quite a lot from these biographies in order to understand the history of a discipline in terms of power structures, the life and times of ideas, the constitution and keeping of rules etc.

We can not require that a particular history of science treats these four units simultaneously. But what is required at least is an appreciation of the connections between the domains, and the notions of the interplay of science-internal and external factors in the generation, establishing and failing of scientific knowledge.

With contextual history I want to stand up for a history of science (psychology), that has the evaluative task, suggested here, to study the broad justification and consequences of science in its theoretical and practical results. Because much of this kept ground during long periods, up until today, and because science is not self-moving but the work of men indeed, contextual history of psychology has something to say to everyone who at the supply-side wants to find his/her way in the discipline, and for many who at the demand-side expect something of it.

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FREUDO-MARXISM IN HUNGARY: SOME PARALLELS BETWEEN WILHELM REICH AND ATILLA JÓZSEF[1]

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SUMMARY

The most distinguished figure of Hungarian Freud-Marxism was not a professional psychoanalyst but a poet, Attila József who had, however, acquired intimate knowledge of and personal experience with psychoanalysis. As a Marxist *theorist* he wrote several essays on the conceptual relationship between Marxism and psychoanalysis. As a *poet*, he gave artistic expression to his ideas concerning the possible role of psychoanalysis in demonstrating and explaining the fate of the individual in contemporary society. As a *patient*, he had been treated by several analysts and analytically oriented psychiatrists. In this paper I want to show that some elements of his thinking are common with other Freudo-Marxian approached of his age. In particular, there are some striking parallels between the orientation of Attila József and Wilhelm Reich. Making a comparison between the ideas of the two thinkers, I call attention to similarities as well as to differences. Finally, I attempt to point out some general implications concerning the problem of the relationship between psychoanalysis and Marxism.

Summarizing the early history of Hungarian psychoanalysis and the role played by Sándor Ferenczi in it, Paul Roazen writes: "In Freud's 'On the History of Psychoanalytic Movement', he listed only one Hungarian collaborator, Ferenczi, but 'one that indeed outweighs a whole society'. The first meeting of the Hungarian Psychoanalytic Society had been held in 1913, with Ferenczi the leader: under his 'guidance', it became, in Freud's view, a 'centre of intense and productive work and was distinguished by an accumulation of abilities such as were exhibited in combination by no other Branch society'. At the congress of analysts in Budapest in 1918, Ferenczi was elected president of the International Psychoanalytic

Association. (...) The Budapest Congress marked a turning point for Freud's movement. For a short time, March-August 1919 (during the months of the Hungarian Concil's Republic), Ferenczi held an appointment in Budapest to the first university lectureship on psychoanalysis (Roazen, 1979, p. 361).

However, Freud's early hopes of making Budapest the "analytic capital of Europe" outside Vienna had vanished after the defeat of the revolutions in 1918-1919. To be sure, psychoanalysis in Hungary could survive and continued to exist, but the victorious political and social reaction forced psychoanalysts into a relative isolation. Their once militant role in the progressivist and bourgeois-radical movements of the pre-war Hungarian intelligentsia had been replaced by a more or less passive, strictly professional orientation. The leading figure, Sándor Ferenczi, suffered many personal and professional crises until his early death in 1933. After 1933 most of his disciples and other members of the "Budapest School" of psychoanalysis gradually emigrated from Hungary. Those who remained in the country, lost their lives - with very few exceptions - in labour camps or in Nazi concentration camps.

If one takes into account the fate of Hungarian psychoanalysis between the two world wars and the long - almost complete - official ban on "Freudianism" which followed after 1948/9, it is quite understandable that the theoretical and technical achievements of the "Budapest School", Sándor Ferenczi, Géza Róheim, Michael Bálint, Robert Bak and others, are perhaps better known abroad than in their native country. (On the "Budapest School", see e.g. Dahmer, 1976; Paál, 1976; Harmat and Hebenstreit, 1982).

There is, however, another line which has remained in almost complete darkness in Hungary as well as abroad: it is the line of those thinkers who did their share in the intellectual efforts to "synthetise" Freud and Marx[2].

The most distinguished figure in Hungarian Freudo-Marxism was not a professional psychoanalyst but a poet, Attila József[3], who had, however, acquired intimate knowledge of and personal experience with psychoanalysis. As a Marxist *theorist*, and, for a time, an

adherent to the one-time illegal Communist Party of Hungary, he wrote several, though mostly fragmentary, essays on the conceptual relationship between Marxism and psychoanalysis.

As a *poet*, he gave artistic expression to his ideas concerning the possible role of psychoanalysis in demonstrating and explaining the fate of the individual in contemporary society. Moreover, he attempted to apply some of the elements of psychoanalytic discourse in describing and interpreting his own inner world and conflicts. His one-time friend, Arthur Koestler calls Attila József's late poems "a new branch of poetry ... the Freudian folksong" (Koestler, 1955, p. 178).

As a *patient*, he had been treated by several analysts and analytically oriented psychiatrists until his suicide in 1937, when he was only 32.

Attila József has been celebrated in Hungary as one of the greatest national poets and the greatest poet of the working class and revolutionary socialism; some of his poems have become canonized texts for schoolchildren. Until very recently, however, the relation of his poetry and thought to psychoanalysis was treated as a "taboo" or rejected as a deviation from "authentic" Marxism, even though it was acknowledged that the former sectarian policy of the Communist party leadership was also, at least partly, responsible for his silent expulsion from the movement in 1934. Apart from political implications, his relation to psychoanalysis has been regarded as a personal obsession, an idiosyncrasy, itself a symptom of his illness (allegedly schizophrenia), a part of the pathological process which finally led to his tragic death.

Recent research[4] on Attila József's poetry and life history has started to rivise the "Attila József question", which has been, for a long time, a neuralgic point in the Hungarian leftist thought and which is still largely covered by "social amnesia". It is clear now that psychoanalysis constituted an essential part of his poetic world view and of his whole intellectual outlook: without considering the Freudian impact, his relation to Marxism would be also inexplicable. One might say that indeed, in a certain sense Attila

József succeeded in accomplishing a genuine integration of Marxism and psychoanalysis - not so much on the level of pure conceptual "synthesis", but by expressing in an artistic, self-reflective way the dilemmas and ambivalences, optimistic expectations and bitter disappointments of a generation which tried to define its own place and identity in Marxian as well as in Freudian categories. From the collectivistic messianism of his "early" poetry to the existential loneliness of his "late" poems one can feel the presence of a person, a "mere person" who experiences all the traps and labyrinths a revolutionary spirit has to face with in a non-revolutionary age and who finally chooses to remain human in a dehumanized world. His personally reflected Freudo-Marxism is a powerful critique of doctrinaire Freudo-Marxism, a doctrinarianism to which he himself fell victim in some of his theoretical writings.

Unfortunately, in the context of this essay it is impossible to analyze Attila József's Freudo-Marxism in its relation to his poetry - mainly due to linguistic difficulties[5]. Instead, I will restrict myself to showing that some elements of his thinking are common with other Freudo-Marxian approaches of his age. In particular, there are some striking parallels between the orientation of Attila József and Wilhelm Reich, the leading figure of Freudo-Marxism of the early thirties. These parallels seem to be self-evident, even though Attila József never referred explicitly to Reich's name or to any of his writings. Nevertheless, he had to know about "the father of SEXPOL". In the early thirties Reich's name, ideas and activities were fairly popular in Hungarian leftist intellectual circles[6]. The most authoritative journal of the Hungarian left, *Korunk* (Our Age) - edited and published by Gábor Gáal in Romania, in the Transylvanian city Kolozsvár (Cluj) - of which Attila József was a permanent collaborateur, from 1928-29 on reviewed all important writings of Reich and informed about the developments in the SEXPOL-movements. *Korunk* had published also a great number of articles sympathetic as well as hostile to Reichian ideas and SEXPOL. Beside *Korunk*, there was another forum which was more explicitly influenced by Reichian ideas. It was *Emberismeret*

(Knowledge of Man), a short-lived series of five special numbers (1935-36) dealing with the problems of psychoanalysis and the human sciences. The periodical was edited by two leftist psychoanalysts, István Kulcsár and Béla Székely, both analytic consultants, friends and party comrades of Attila József. In the special number entitled *For and against psychoanalysis* they published - under the title "Psychoanalysis and socialism" - a section of Reich's contradictional essay "Dialectical materialism and psychoanalysis", which originally appeared in the bilingual journal of the Communist International *Pod znamenem marksizma (Unter dem Banner des Marxismus)* (Reich, 1929). In other numbers the editors of *Emberismeret* published texts by S. Bernfeld, O. Fenichel, K. Teschitz and of other Freudo-Marxists of the time. Attila József was also a contributor of *Embiresmeret* with an essay that appeared in the special number *On suicide*. Maybe it is no accident that Attila József did not refer to Reich: the two men had very different intellectual constitutions. Reich was, first of all, a propagandist and a prophet; he preferred to vulgarize, to put things as simply as possible. Attila József - though in the communist movement he did not refuse the role of agitator - was predominantly a meditative character, he used highly, sometimes oversophisticated arguments in the Hegelian-Lukácsian tradition. Nevertheless, the parallels are striking - at least until 1933, and on two essential points: on judging the significance of psychoanalysis for Marxism and on the emphasis on sexual repression and its abolition. As Attilia József put in his article "The sexual problems of youth", "Marxism is a science of liberating the oppressed proletariat, psychoanalysis is the science of healing the soul full of repressions" (József, 1932a). The Reichian formula manifests itself in his other theoretical essay of 1932 ("Individuality and reality"): psychoanalysis is a natural science complementary to Marxism; a *"Hilfswissenschaft"*,[7] as Reich put it in his "Dialectical materialism and psychoanalysis", inasmuch as it can show what processes are taking place in the minds of class individuals and, consequently, it can contribute to enhancing class consciousness (József, 1932b).

In "Individuality and reality" Attila József introduces the dichotomy of "neurotics" and "revolutionaries" which is equivalent to the Reichian characterology of "neurotic" and "genital" types (Reich, 1933a). The aim of psychoanalysis is to bring repressed sexuality into consciousness, thus liberating the proletariat from the bourgeois morality. This can take place only on a social scale, the relationship between patient and doctor is already a social relation; thus, orthodox psychoanalysis should be transformed into a *therapy for the masses, into a "human technology" of revolution*[8].

This conception of psychoanalysis in a Marxist framework is a typical product of the messianism of the twenties which envisages that "bringing into consciousness" will automatically lead to a social revolution which will immediately re-establish the lost harmony between individual and society, and abolishes alienation. The messianistic role attributed to psychoanalysis is rooted in the hopes and illusions of psychoanalysts in the Russian revolution which seemed to change radically not only the "economic base" and "political-ideological superstructure", but everyday life as well (including morality in general and sexual morality in particular). The early, partly favourable attitude of Soviet Marxism toward psychoanalysis seemed also to justify a "natural alliance" between Marxism and psychoanalysis: in a society where no antagonistic class contradictions exist any more, psychoanalysis can freely advance and can assume its genuine mission, first of all, in the *social prevention* of neuroses[9].

The "honeymoon" of psychoanalysis and Marxism ended, however, in a quick and drastic way. In the second half of the twenties, Soviet ideology started to identify "Freudianism" with "bourgeois reaction" and "social fascism" (i.e. social democracy). "Freudianism" as an ideological phantom assumed the role of "public enemy No. 1". The motives of this crusade against psychoanalysis can be - at least partly -explained by the self-defensive ideological needs of the victorious Stalinist ideology. Thus, socialism can be built up in one country; Soviet socialism already had realized practical-

ly all messianistic ideals, the people in this country must be happy *per se*, consequently, there is no question of individual "discontents", which in the final analysis equals "bourgeois individualism". The paranoid logic of the attacks against Freudianism was projected immediately onto Freudo-Marxism, which became even more dangerous than "pure" Freudianism, because, according to these critics, it "steals back" bourgeois ideology under the mask of pseudo-Marxist terminology. It is instructive how Reich's attempts at "reconcilation" were refuted by his Soviet (and also Hungarian) critics. The more he was willing to put psychoanalysis into the framework of dogmatic Marxism, the more he was stigmatized as a "deviant", as a "renegade"[10].

It is this context which explains the main characteristics of Attila József's Freudo-Marxian writings: an attempt to place the problem of individuality into the framework of a rigid and deterministic Marxist orthodoxy on the one hand; a real understanding of the emancipatory function of psychoanalysis, a real faith in the liberation of the individual on the other. This contradiction between the technological exploitation and the emancipatory mission of psychoanalysis found a sort of *poetic* solution in his famous poem "On the edge of the city" (1933):

> Until brightens up
> our beautiful gift, the order
> by which the mind understands
> the finite infinite,
> the forces of production outside
> and the instincts inside.

After 1933 the victory of fascism put an end to messianistic hopes and illusion. Wilhelm Reich had drawn the conclusions in *The Mass Psychology of Fascism* (Reich, 1933b). In this controversial book he blamed the workers' parties for neglecting the vital needs of the masses and thus permitting and even promoting the Nazi seizure of power. Reich pictured fascism as a mass movement of the

lower middle class, subsequently, however he extended his conception of fascism to a primordial, cosmic Evil, that is, to the authoritarian character structure of all men living in patriarchal societies for thousands of years. The break between Reich and the Communist movement became full and irreversible: the Communist analysis of fascism stressed that it was only a provisional defeat of the working class, a "conspiracy of finance capital"; for Reich, it was the "emotional plague of mankind" (which includes Stalinism, the "red fascism" as well) (Reich, 1970).

Between the poles of the self-defensive and hypocritical shortsightedness of the official Comintern politics and the Reichian mysticism a new approach had emerged: that of the Frankfurt School which defined its main task not in simply putting together Freud and Marx but in *reconstructing* historical materialism as well as psychoanalysis into a philosophy of history and a social theory which must be able to give a full account of the "dialectics of enlightenment" (see e.g. Jay, 1973).

After 1933, in the remaining few years of his life Attila József went beyond the Freudo-Marxism of his earlier period. He also echoed the Reichian myth of the "emotional plague" ("A primeval rat spreads disease among us", he wrote in one of his poems), but his basically rational mind prevented him from mere mythologization. In his late essays he outlined a "critical theory of the subject" based on the anthropological humanism of the Marxian *Economic - philosophical manuscripts* and on psychoanalysis (see especially his theoretical essay from 1935 entitled "Hegel, Marx, Freud": József, 1972).

Surely, these theoretical attempts lacked the comprehensiveness, philosophical depth and sophistication of the Frankfurt School thinkers. Nevertheless, they demonstrate a radical shift from a dogmatic, pre-deterministic Marxism in which psychoanalysis plays only the subordinate role of an "auxiliary science", toward a social theory as well as toward a historically more reflected, more concrete *social psychology* which is able to understand and explore complex relationships between individual and society.

This intellectual effort, however, could not prevent the poet's personal breakdown. He was not, after all, a rich psychoanalyst, a prophet or a university professor in Frankfurt or in New York. He was only a poor poet in Hungary, a "mere person" who - with his own poetic words - had to descend into hell "in order to play on the bagpipe".

His Freudo-Marxist "adventure" ended in a negative result. As he wrote in one of his last poems ("You know there is no pardon", 1937):

> In distress you called for father,
> for man, if no god exists.
> And you found perverse kids
> in psychoanalysis.

This negative experience, whether justifiable or not on a theoretical level, is a warning against the doctrinarianism and abstract intellectualism of most Freudo-Marxian integration attempts.

NOTES

1. This paper is based on the author's unpublished dissertation (Erös, 1980). See also: Erös, 1981.
2. As documentary sources and interpretations of Freudo-Marxism, I used the following publications: Bernfeld, Reich *et al.*, 1970; Dahmer, 1973; Dahmer (hrsg.), 1980; Gente (hrsg.), 1970; Jacoby, 1975; Jay, 1973; Mitchell, 1974; Schneider, 1975.
3. There is, of course, a lot of editions of his works in Hungary. The critical edition of his complete works: József, 1958.
4. See Bókay, 1980; Bókay, Jádi, Stark, 1982.
5. Some of his verses have been translated into different languages and published mainly in various anthologies of Hungarian poetry. See a recent selection of his poems in German: József, 1978.

6. A more detailed analysis of Reich's impact in Hungary see in Erös, 1980.
7. "As a science, psychoanalysis is equal to Marxian sociological doctrine: the former treats of psychological phenomena and the latter of sociological phenomena. And only insofar as social facts are to be examined in psychological life or, conversely, psychological facts in the life of the society, can the two act mutually as auxiliary sciences to one another" (Reich, 1929; quoted after the 1966 English translation, p. 8).
8. In the early twenties, Reich advances the idea, that "Neurosis is a mass sickness, thus, it cannot be prevented but on a social scale". However, "the feudalistic conception of psychotherapy, which, by its very nature, extremely individualistic, naturally came into conflict with the requirements of medical work (...). At the Budapest congress in 1918, Freud had spoken of the necessity of founding psychoanalytic clinics for those who could not afford private treatment. However, the pure gold of psychoanalysis would have, he said, to be mixed "with the copper of suggestive therapy". Mass treatment would make this necessary" (Reich, 1973, p. 73).
9. "Because psychoanalysis, unless it is watered down, undermines bourgeois ideology, and because, furthermore, only a socialist economy can provide a basis for the free development of intellect and sexuality alike, psychoanalysis has a future under socialism" (Reich, 1929, quoted after the 1966 English translation, p. 56).
10. See the various attacks on Reich documented in Bernfeld, Reich *et al.*, 1970; Gente (hrsg.), 1970.

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IMAGES OF MAN IN EARLY FACTOR ANALYSIS - PSYCHOLOGICAL AND PHILOSOPHICAL ASPECTS

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Summary

This paper covers the psychological theories and philosophical positions of three major early figures in factor analysis, Charles Spearman, Godfrey Thomson and Louis Thurstone. Such a broad view enables one to provide plausible answers to fundamental problems about their relations, for example, the depth and length of their various quarrels, given that *mathematically* the three scarcely differed.

It is argued that Spearman, with the strong Kantian and Leibnitzian attitudes inherited from his time with Wundt, used factor analysis as a technique to demonstrate certain pre-existing hypotheses about the structure of human intelligence and abilities. Thomson and Thurstone, on the other hand, showed themselves to be instrumentalists and conventionalists for whom factor analysis was a natural exploratory and inductive machine. This philosophical clash reveals itself as much in the strength with which the various psychological positions are held as it does in the nature of these positions. The paper illustrates these points in retelling the theoretical progress of all three workers.

In addition, recent ideas on the historical relationship between science and technology are used to illuminate certain further difficulties in the work of Thurstone whose psychological position seems puzzlingly archaic.

Introduction

The paper's main aim is to provide a much fuller historical account than can be found in either the intelligence of psychometric literature of the psychological theories and philosophical positions behind the early work on factor analysis. Although the account will concentrate on the work of Charles Spearman and God-

frey Thomson, some space will be devoted to the origins and development of Louis Thurstone's psychological underpinnings of his system of multiple factor analysis. It is hoped that the history of these ideas will better illuminate the reasons for the disputes between the three participants since, as various people have recently pointed out, the *mathematical* differences between them do not appear to justify either the seriousness or length of their various quarrels. It is also hoped that a longer treatment of the substantive psychological and philosophical issues will provide the foundations for a more rounded history of the area, since it is difficult to separate the psychological from the mathematical in early factor analysis.

Since 1940 factor analysis has been viewed as a general purpose statistical tool for exploring data whose range of application is not confined to any particular discipline. This was not the case, however, in the beginning, as will be seen in the next section on Spearman. The evolution of factor analysis from a technique of hypothesis confirmation to one of data exploration is also better understood if one considers the nature of the changing philosophical and psychological positions that lay behind the technology.

In the Beginning was the Word: Charles Edward Spearman (1863-1945)

The earliest paper of importance is the well known 1904 one '"General Intelligence", Objectively Determined en Measured'. Here Spearman report the analysis of a large scale psychometric study. There are several initial remarks to make about ths paper: first, most later commentators, for example Holzinger and Harman (1941) and Steiger and Schönemann (1978), have claimed too much too early for Spearman. He did not, for example, offer a fully worked out theory of intelligence in 1904, nor did he have an elaborate mathematical model for such a concept. Second, the paper is as much a practical demonstration of the value of Spearman's own correlational discoveries as it is an exposition of a theory of intelligence. Indeed, the existence of a correlation corrected for attenuation

was vital for the demonstration of the existence of such a psychological entity as "general intelligence". Third, the paper is not an objective test of the existence of general intelligence. Rather it is a partial report whose message is that general intelligence is alive and kicking. Finally, Spearman was clear that in this paper at least he was *not* interested in individual differences. Instead his concern was with what he saw as a crisis in *experimental psychology*. His approach was that of an *experimenter*, and his use of correlational techniques was to confirm a pre-existing hypothesis.

Although the paper is tentative over the detailed form of its concept of intelligence (Spearman, for example, talks at one place about General Discrimination as having "great approximation" to General Intelligence, while almost immediately afterwards refers to a hierarchy of general and specific *functions*) it is much firmer on the philosophical and methodological foundations of the work. It is to these latter matters that I will now turn. As is well known, Spearman spent about seven years in Leipzig in Wundt's laboratory during the years 1897-1907. In fact, Spearman's influences were not only Wundt himself but Wundt's colleagues, in particular Krueger and Wirth. He was also impressed with the work of Wundt's students: the experiments by the Danish psychologist Lehmann on limited mental energy and divided attention, for example, were used twice by him in his books.

In the first few pages of his 1904 paper, therefore, on "General Intelligence", Spearman strikes a clear Wundtian, not to say Newtonian, tone when he distinguishes between observable "Functional Uniformities" that is, "like reactions under like conditions" (page 204), and "Conceptual Uniformity", that is, a coherent theoretical system (see Danziger, 1980, on Wundt's philosophical background). This latter Uniformity "in psychology is but an indispensable substructure - and one of lamentable fallibility" (page 205). Spearman also argued that the correspondences between the observable Functional Uniformities and the Conceptual system can only be made clearer if the experimental methodology is improved. His

suggestions here were for a Correlational Psychology which, unlike Oehrn's individual difference movement, aimed "to eliminate individualities as an obstacle to further progress, being itself, no less than General Psychology, in search of laws and uniformities" (page 207).

Spearman's grand aim, therefore, was to use the system building potential of correlational methods to advance Wundtian experimental psychology into the realm of the "real" world outside the laboratory. This was also true of his students. This was to be accomplished, first, by establishing the existence of observable behavioural regularities and then by mapping them into a complex theoretical structure whose form was suggested by the prevailing psychological and physiological ethos. Not surprisingly, for Spearman such a substantive context was, in large part, that developed by Wundt and Wundt's students and colleagues. Further, Spearman believed in the final reality of these psychological ideas and consequently viewed his efforts more as an attempt to prove their viability than to objectively accept or reject them. In other words, the evidence for the reality of these ideas was not just the products of his increasingly complex systems of factor analysis, it was in addition an *a priori* commitment to a Wundtian psychological Universe. "They (the factors) do not in the least depend on any such hypothesis as that of "energy" but on the contrary supply the very facts upon which such hypotheses ought to be accepted or rejected". (1932, page 488). Notice that both factors *and* hypotheses are necessary for full understanding and exist in parallel. Of course, Spearman used later physiological and psychological evidence to support his position (he was overjoyed, for example, over Lashley's ideas on equipotentiality and mass action) but inevitably such evidence was selectively chosen.

Let me now return to Spearman's development of the ideas on general intelligence. In a paper in 1906 with Krueger, Spearman suggested that general intelligence was a measure of the ability of a person's cortex to achieve a flexible or 'plastic function'. In other words, a person with a high general intelligence would have

"a greater power of building up finally differentiated and integrated structures", (quoted in Spearman and Hart, 1912). This quasi-physiological characterisation of general intelligence was further utilised in the Spearman and Hart paper of 1912 which can be viewed as the final statement on the nature of this general function (now called the General Factor or General Ability).

Here Spearman devotes several pages to deepening and generalizing the definition suggested in the earlier paper with Krueger. First, he separates General Intelligence into the General Factor (characteristically described as "a deep underlying truth") and "a superposed mass of obscurity and error" (pag. 67). Spearman then offers a psychological description of the General Factor as a common fund of "intellective energy" which "is disposible for *any* kind of nonmechanized process" (pag. 71). Finally, he offers a physiological description, claiming that the theory of general and specific factors is paralleled by the contemporary neurophysiological picture which presented a compromise between the extremes of those who argued for "functional equivalence" of the cortex and those who adhered to a belief in cortical localization. Spearman then writes that "every particular mental operation requires two things: firstly, a specific activity of a particular system of neural structures; and secondly, the concurrence of neural energy from the whole, or a large part, of the cortex" (page. 72).

The only refinements and extensions to this view are either little more than restatements of the earlier work, or the provision of names for existing concepts, or are incomplete and hence unsatisfactory. For example, in his "The Abilities of Man" (1927 and 1932) Spearman differentiates between Monarchic, Oligarchic and Anarchistic theories of intelligence and not surprisingly plumps for the first. This hierarchical organisation of intelligence is, of course, little more than the equally hierarchical theory of general and specific abilities. Further, the phrase "Theory of the Two Factors" was suggested by De Sanctis in 1913, while Spearman's 1914 Psychological Review paper contains the first identification in English of 'g' with General and 's' with Specific Ability.

Finally, although Spearman claimed that 'g' powered the education of relations and correlates in his so-called noegenetic system of cognition, in fact this attempt to merge the results of his factor analytic studies with his qualitative work on knowing and perception etc. was never properly carried out. Consequently, the invocation of 'g' did little if anything to advance the work on cognition (see Spearman's "The Nature of 'Intelligence' and the Principles of Cognition, 1923 and 1927"). Although Spearman, for tactical reasons, placed varying importance on the factor analytic evidence during his long and productive life, he never abandoned his central commitment to 'g' or the general factor (see Spearman, 1946).

Spearman's main protagonist was Thomson whose much slighter ideas on the psychology behind factor analysis will be dealt with next.

It Ain't Necessarily So : Godfrey Thomson (1881-1955)

Thomson, as he admitted in his 1952 autobiographical sketch, "never had any teaching in psychology worth mentioning" (page 294). His doctorate was, in fact, in physics at the University of Strasbourg in 1906. Of particular importance here is that at the start of his time there (in 1904) he learnt of the views of the conventionalist Henri Poincaré from the German translator of *Science and Hypothesis* (see Thomson 1969, page 55).

This natural extension of Mach's Instrumentalist view of science overturned Kantian ideas of *a priori* truths, substituting for them a more empirical and psychological version of the scientific verities, with the *a priori* relegated to the status of good or bad, that is, useful or useless, *conventions*. Poincaré's treatment of classical mechanics and thermo-dynamics emphasised their historical and anthropomorphic nature and hence constituted an attack on the naive realism that, for example, characterised Wundt's herbartian and Kantian views of nature. Such a movement, therefore, encouraged increased methodological and logical efficiency. Poincaré also emphasized the role of experiment as the main decider between hypotheses and hence made the status of such no-

tions much more provisional. Scientific systems were viewed as cognitive aids to understanding, not ultimate truths (a more recent discussion which emphasizes such philosophical differences can be found in MacCorquodale and Meehl, 1948).

All of these attitudes and ideas can be discerned in Thomson's criticisms of Spearman whose principle sin, in Thomson's eyes, was not that the notion of 'g' was necessarily wrong but that Spearman had not demonstrated its existence. This was a view that Thomson maintained throughout most of his life. Compare, for example, his 1916 statement that "The object of this paper is to show that the cases brought forward by Professor Spearman in favour of the existence of General Ability are by no means 'crucial'. They are, it is true, not inconsistent with the existence of such common element but neither are they inconsistent with its non-existence" (page 271), with his remarks that "even supposing the tetrad-differences (early factor analytical measure) to be as closely grouped round zero as Spearman and Hart claimed, yet the Theory of Two Factors, though a sufficient explanation, was not a necessary one" (page 14) of 1946. (It will, of course, be recalled that Poincaré rejected the possibility of "crucial" experiments in science).

Since Thomson did not offer an alternative or even, as Thurstone did, a generalised system of factor analysis to that discovered by Spearman, his role was more that of critic (and propounder) of other people's views. Thomson's relatively negative contribution to the development of factor analysis, mainly I suspect due to Poincaré's somewhat sceptical view of science, also shows up in the nature of, and degree of commitment to, alternative psychological theories. The earliest of Thomson's theories was advanced in 1919 and appeared as a direct result of his criticism of Spearman. This was based on artificial data generated by the dice throws: "Let us suppose that the mind, in carrying out any activity such as a mental test, has two levels at which it can operate. The elements of activity at the lower lever are entirely specific; but those at the higher level are such that they may come into play in more than one kind of activity, in more than one mental test. These elements

are assumed to be additive like dice, and each to act on the 'all or none' principle, not being in fact further divisible.

The difference between the levels may be physiological, as between cortex and spinal cord, or it may be the difference between conscious and non-conscious, *or what not* "(page 341, the italics are mine). This rather tentative, somewhat hierarchical scheme was, however, rapidly abandoned in favour of one exhibiting little if any detectable structure, the so-called Sampling Theory (see Thomson, 1920). This change was no doubt mainly due to Thomson's speedy realization that this dice studies did not imply *any* pattern to the abilities. However, I have the suspicion that Thomson did not want to leave any hostages to fortune in the form of a degree of hierarchical structure, since Spearman would undoubtedly have seized on this as evidence of positive support for his own views (in fact Spearman did just this, but much later on, see his 1938).

The Sampling Theory took on a new name, that of the theory of bonds (see, for example, Thomson 1939), without, however, acquiring more structure. This new version follows from Thorndike's work on psychological bonds, or, as he later called them, 'connections'. Thomson was a great admirer of Thorndike, he had, for example, spent the academic year 1923-24 at Columbia at Thorndike's express invitation. Thomson had written up his lectures there for his monograph "Instinct, Intelligence and Character" (1924), a book which is full of Thorndikean wisdom. Even the chapter on Brain Localization, which shows Thomson's admiration for Head, can be seen as indirect evidence for the Sampling or Bond theory.

The end of the 1939 text, however, sees Thomson beginning to doubt the psychological reality of factors, except as cognitive aids, and, in a statement with a very Poincarésque tone, he states that: "There is a strong natural desire in makind to imagine or create, and to name, forces and powers behind the facade of what is observed, nor can any exception be taken to this if the hypotheses which emerge explain the phenomena as far as they go, and are a guide to further inquiry" (page 284).

This attenuation of the link between theory and technology can be seen as its most extreme in Thurstone's work on multiple factor analysis, a brief resume of whose rather thin psychological theorizing is given now.

Nothing Will Come of Nothing - Speak Again: - Louis Thurstone (1887-1956)

In his short autobiography Thurstone states that although he had written down the first equations for his later system of multiple factor analysis as early as 1922, he had not taken up the task of developing the full system for nearly eight or nine years (1952, page 313). Before 1922 Thurstone had had a long and varied career, not only in psychology where he was active in test development during the First World War, but also in engineering, including a short period in Thomas Edison's laboratory. After 1922 he returned to the University of Chicago where he had taken his doctorate in psychology (see Bulmer, 1981, on the ethos of Chicago during this period, including comments on Thurnstone's own contributions). Here he continued with his work on testing for the American Council on Education (see Noble, 1977, page 255,) and also pursued some of the most interesting work on scaling in America at this time. This included extensive studies of attitude and social value scales, all of which were pursued with a degree of vigour not seen before in this area. In addition, Thurstone's contributions to scaling theory were as important as his practical illustrations of the techniques.

One of the requirements of such a scaling approach to psychology, however, is that there is little room for complex and subtle theorizing. This sacrifice of substantive content in return for increased systematization can also be found in Thurstone's rather pragmatic approach to psychological theorizing in his system of factor analysis. For Thurstone, factors are quite simply old fashioned faculties, with all the conceptual problems implied by such entities, for example, circularity and lack of an inherent limit to their number. "Factor analysis is reminiscent of faculty psychology. It is true that the object of factor analysis is to

discover the mental faculties" (1935, page 53). The lessons of such a pragmatic approach to psychology, almost certainly derived from Thurstone's time as psychometrician and his stunning success in scaling social phenomena, are that if one wishes to construct a general purpose statistical system then one should avoid being constrained by an elaborate and overly structured substantive theory.

As with Thomson, the links between psychological theory and technique are almost non-existent. Further, his highly instrumentalist orientation is apparent from his opening remarks to the "Vectors of Mind": "A scientific law is not be thought of as having an independent existence which some scientist is fortunate to stumble upon. A scientific law is not a part of nature. It is only a way of comprehending nature" (1935, page 44). Also, "The laws of science are not immutable. They are only human efforts towards parsimony in the comprehension of nature" (1935, page 45). Further, "The criterion by which a new ideal construct in science is accepted or rejected is the degree to which it facilitates the comprehension of a class of phenomena which can be thought of as examples of a single construct than as individualized events. It is in this sense that the chief object of science is to minimize mental effort" (1935, page 45).

The philosophical problems that this liberation from psychology has engendered have unfortunately stayed with factor analysis to the present day but it has allowed the technique to be usefully employed in other fields. Thurstone appeared to realize this too late. Compare for example, the contradictory statements in his 1940 survey paper, thus "The method of factor analysis implies nothing about the biological, or physical, or statistical character of the primary factors" (page 204), while "My own contributions to factor analysis have been motivated by a desire to solve some fundamental problems in psychology, and consequently I have tried to discourage a tendency to regard the factor method as a self-contained and extraneous statistical technique" (page 235). Here he shows himself well and truly caught by the contradictions of a system that had become too flexible to be easily mapped on to any interesting psychological theory.

Epilogue

The period 1904 to 1940 saw an increasing sophistication of methodology and analysis in psychology, with a decrease in theorizing, general tendencies which would probably have by themselves etiolated the substantive theories behind factor analysis. In addition, the fact that both Thomson and Thurstone were in the practical business of developing psychological tests for specific groups meant that the needs of developing an efficient technology overwhelmed any wish that they might have had to match it with sophisticated theorizing about people. Edwin Layton, the distinguished historian of technology, has recently pointed out that technologists do not necessarily share the same aims or reward systems of scientists and hence are usually content with rather conservative scientific ideas (see, for example, his seminal paper on science and technology, 1971).

Perhaps Spearman should have the last word: "The intuitionist tries to make ideas work without mathematics. The psychometrist, mathematics without ideas. When will both learn that two legs are better than either alone? (1934, page 407).

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EXPERIMENTAL APPROACHES TO CHILD PSYCHOLOGY BEFORE DARWIN

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Summary

English speaking historians of psychology have long subscribed to the presentist position that no significant research in developmental psychology was carried out before the end of the 19th century. The present study had attempted to refute this belief by presenting in some detail the experimental investigation of the behavior of newborn children with the German internist, *Adolf Kussmaul* (1822-1902), performed and published (1859) more than twenty years before Preyer's book was published (1882). Kussmaul's methods and results were compared with modern work in the same field.

English-speaking historians of psychology have long prescribed to the presentist position that no significant experimental research in child psychology was carried out before the late 19th or the early 20th century. This perspective has found clear expression in Diamond's sourcebook, *The Roots of Psychology* (1974):

> Before Darwin, child psychology was limited to occasional bits and snatches such as Aristotle's mention that children do not dream before the age of four (1) or Locke's reminder that supposedly innate thruths are not known to children in advance of experience. In the eighteenth century Smellie recognized the need for the systematic study of child behavior (p. 469).
>
> Modern interest in the "child mind" arose as a direct result of Darwinism. Darwin himself published a "Biographical

> sketch of an infant mind", based on observations of his own son ... Preyer's work must have been initiated at just about this time, and it was followed by similar studies by Miss Shinn, the Scupins, and others (p. 471).

The present paper will present in detail a truly experimental study of child behavior which was carried out and published almost two decades before Darwin's *Biographical Sketch of an Infant Mind* (1877), 23 years before Preyer's famous work, *Die Seele des Kindes* (1882) and nearly half a century earlier than Meumann's classic, *Vorlesungen zur Einführung in die experimentelle Pädagogik (1907-1908)*. This research was performed by the noted German internist, *Adolf Kussmaul* (1822-1902) and published in 1859 under the title *"Untersuchungen über das Seelenleben des neugeborenen Menschen* (Investigations of the mental life of the newborn child).

One can of course, discover forerunners of almost any modern-day discovery in psychology and related fields, if one only searches long and hard enough. Kussmaul's work is remarkable because it represents a far more sophisticated form of investigation than the more popular writings by his famous successors Darwin (1877), Preyer (1882) and Hall (1883) on similar topics.

I

Elaborate details about the life and intellectual career of Kussmaul are readily available in the two volumes of his autobiography, which are rightly counted among the very best medical autobiographies (Bringmann & Balance, 1976; Kussmaul, 1899, 1903). In addition, his encounters with scientific luminaries like Puchelt, Henle, Semmelweiss, Rokitansky, Virchow, Roller, Helmholtz and many others are remarkably candid and enlightening.

Although he was about ten years older than Wundt, Kussmaul knew Wundt well. They had both graduated from the same high school, had both received the coveted research award of the Medical Faculty of Heidelberg University and both were among the founders of the Natural Science Club at the same university. Wundt reviewed Kussmaul's book on speech pathology and Kussmaul, in term, characterized Wundt very positively in his autobiography (1903):

> Despite his youth he was exceptionally well read. Although his critical comments were often poignant, he expressed them gently. Wundt was personally warm hearted, and we spent a good deal of time on bikes through the beautiful Neckar valley. (p. 72).

Their acquaintanceship has been documented as early as 1856, and it is quite possible that Kussmaul was influenced in his developmental and experimental research by Wundt. Unlike Wundt, however, whose medical education was geared toward an academic and research career, Kussmaul, throughout his life, was strongly interested in clinical practice and was acclaimed as an outstanding physician.

Near the end of his association with Heidelberg University Kussmaul researched his book on the psychology of newborn children. At that time he was supporting himself and his family as a public health physician. It is likely that his experiments with newborn children were made possible by this appointment. Kussmaul had demonstrated a special interest in pediatrics and obstetrics during his postgraduate studies at Vienna and Prague more than ten years before. Discussions of normal child development were also an essential part of his lectures on psychiatry during the Heidelberg period (1959):

> As a teacher of psychiatry I began my lecture with a developmental history of the human mind, since I had observed that, in this manner, I was able to give my students the quickest and most vivid picture of the elements of mental events (p. 7).

Dissatisfaction with the existing literature on infant behavior led Kussmaul to conduct his own experiments (1859):

> I discovered big gaps which the empircal study of the mind still has to fill in. It was primarily the earliest time of life about which I was able to tell my students little that was certain. This is the reason why I carried out several observations and experiments with newborn children ... (p. 8).

Kussmaul's *Investigations* (1859) served as his inaugural dissertation for Erlangen University. Although the little book was reprin-

ted three times (Kussmaul, 1875, 1896; Brigmann and Balance, 1976), it generally attracted much less attention than his famous monograph on speech pathology, *Die Störungen der Sprache* (1877).

II

Introduction

Kussmaul, like a good scholar, began his monograph by defining his subject matter as the psychological processes of "... sensation, imagination, thought, and motivation ..." in the newborn child (1859, p. 31). It is clear that he fully appreciated the difficulties of studying these activities in young children (1859):

> Knowledge of our own mental activities is mediated directly by consciousness. It is more difficult to understand the mind of another organism, since we can only make indirect inferences from their movements ... One and the same movement frequently can either have a psychological or merely a mechanical cause. It is not enough to simply conclude from the similarity and form of a given moment to another which is commonly mediated by the mind, that the mind has caused a given movement. Rather, one must document that a given movement or a sequence of movements ... could not ordinarily have been the result of mechanical causes ... (p. 4).

Kussmaul also avoided using "purposefulness of movement" as a dependable criterion of psychological causation because robots - or to use his term - *"automata"* (1859, p. 4) existed in his time which were able "to write, draw, make his music and swim in the most purposeful and deliberate manner" (p. 4-5). In fact, he took the remarkably modern position that (1859):

> ... the making of errors in the choice of means, can be considered better evidence for the existence of mental abilities than the apparently superior purposefulness with which the most ingenious machine works (p. 5).

Review of the Literature

About one-fourth of the book (pp. 8-22) is devoted to a scholarly review of classical and recent literature concerning the behavioral repertoire of the human neonate. Kussmaul found the developmental position of Aristotle more compatible with his experimental findings than those of the British Associationists, like Locke, who held that the mind of the newborn child is in effect a *tabula rasa*. With rare exceptions, Kussmaul saw little value in the literature (1859):

> As far as the mental activities of the newborn are concerned, many physicians and philosophers have devoted much attention to them. However, no one has, to my knowledge, subjected these (speculations) to careful study or used experimentation in the process. The assertions of the various authors are full of contradictions and, on closer examination, prove to a large extent to be incorrect (pp. 10-11).

The Experiments

In the remaining part of the book (pp. 22-47) Kussmaul describes the experiments he performed to collect baseline information about the sensory repertoire of newborn children. He studied the senses of taste, touch, smell, vision, hearing and the muscle sense. In addition, Kussmaul addressed himself to hunger and thirst in the neonate. The book closes with a fascinating discussion of various responses, which he regarded as evidence for the presence of intelligence in the newborn child. Each set of experiments was described by him with the traditional information about the subjects, the experimental equipment and materials, the testing procedures employed and finally both quantitative and qualitative results. Kussmaul also critically evaluated the results of each experiment and attempted to integrate his findings with existing information in the field. Whenever his own results were incomplete or unsatisfactory, he made use of other data which we believed to be more reliable and valid.

Experiments on Taste, Touch, and Hunger

Taste. The subjects for Kussmaul's investigation of the sense of taste consisted of slighly more than 20 newborn children. Although we do not know the exact composition of his sample, he informs us that it included children (1859):

> ... who had barely left the womb and who had not yet taken any milk ... full-term and strong babies of both sexes, as well as ... some children who were born prematurely in the seventh or eight month ... (pp. 22-23)

"Sweet" and "bitter" liquids were used as experimental stimuli for the experiments on taste. These substances were first warmed to body temperature and applied to the mouth of his research subjects with a "fine camel hair brush". Kussmaul's rigorous concern with experimental control is illustrated in the following direct quote (1859):

> These liquids consisted of a satiated sugar solution and a solution of 10 grains of sulphate of quinine in half an ounce of water. This substance had a very bitter taste and was used in this concentration in all experiments without exception. The application of such precautions seemed indicated, in order to draw correct conclusions from comparative experiments with different individuals (p. 22).

His major finding was (1859):

> The sugar and quinine solutions produced the same muscular movements in the neonate, which are designated in adults as the facial expressions of sweet and bitter taste (p. 22).

Kussmaul's graphic description of the children's behavior clearly demonstrated the presence of differential response patterns to the ingestion of sweet and bitter stimuli (1859):

> When sugar was brought into the mouth, the children shaped their lips like the snout of an animal, pressed their tongues between the lips and began to suck with pleasure ... When small amounts of quinine were applied, only the muscles controlling the nostrils and the upper lips contracted ... larger amounts of quinine caused the muscles, which control the

> wrinkling of eyebrows and eyelids, to be activated. The eyelids were ... pinched together and kept closed for a length of time. The throat contracted spasmodically, the children choked, the mouth was opened wide and the tongue protruded as much as an inch, and the applied liquids were often partially expelled together with a great amount of saliva. At times the children actively shook their heads like an adult might do who is overcome by nausea (p. 26).

On the basis of the above observations, Kussmaul concluded that (1859):

> ... the sense of taste already functions among newborn children in its major forms ... they do not merely experience taste in a vague manner, as Bichat thought ... (p. 26).

Touch. Kussmaul's experiments on the sense of touch in newborn children were extensive, although they concentrated primarily on the facial area. He specifically explored the touch sensitivity of the tongue, the lips, the nasal membranes and the eyelashes. His experiments on the functions of the eyelashes as sensory organs provide a particularly valuable illustration of his innovative and painstaking methodology (1859):

> The eyelashes are extremely sensitive to the slightest touch. In the waking child has opened his eyes, one can proceed with a thin glass rod almost to the cornea before the eye will be closed. However, as soon as a single eyelash is touched, the eye closes at once. The touching of the eyelid is by no means as effective in producing a closure of the eye.
>
> The extreme sensitivity of the eyelashes can be beautifully demonstrated by the following experiment. If one blows on the cheeks or the forehead of the newborn, it blinks with his eyes. At first, I had incorrectly explained this behavior as a response to changes in temperature. If one, however, directs air through a narrow paper tube alternately to different parts of the face, one can observe that the child will blink only if the airstream touches one of the eyelashes. The

> eye on the stimulated side responds more intensely and quickly (pp. 32-33).

Kussmaul concluded that this reflexive response served to guard the eye against injury at a time when conscious experience cannot as yet control the closure of the eyelids. As a good physiologist, he also suggested that it might be worthwhile " ... to determine if fully formed endings of the nerves of touch exist at the roots of eyelashes" (p. 33).

Kussmaul concluded this section of his research with the observation that newborn children respond to the tickling of their palms and the soles of their feet with a feather. He was unable to duplicate these findings with premature babies, however.

Hunger and Thirst. Kussmaul's research on hunger and thirst in the neonate is also very valuable. He suggested that neonates experience a mixture of hunger and thirst during the first 6-24 hours of their life. An experiment, which he performed with "a lively, pretty, newborn girl" (p. 45) provides further information about his methodology (1859):

> She was born around 7 a.m. and soon gave repeated signs of hunger but was not fed until noon. By that time she had become very restless moving her head back and forth as if searching for something and cried a lot. I stroked her left sheek softly with my index finger without touching the lip when she did not cry. Quickly she turned her head to the left side, grabbed my finger and began to suck. Next, I removed my finger and began stroking the right cheek. Just as quickly she turned to that side and once again took hold of my finger.
>
> Again I removed the finger and stroked the left side. It was quite a surprise how deftly the child turned back on her left side and grasped the finger (pp. 45-46).

Kussmaul continued alternating the stimulation of the baby's cheeks until she began to scream loudly and became quite upset. He then placed the little girl at her mother's breast without, however, placing the nipple directly into her mouth (1859):

> She again calmed down and moved her head back and forth in a searching manner but was unable to find and take hold of the nipple. The nipple had to be placed between her lips and jaws and only then did she begin to suck. Thus, it is clear the child was able to grasp the firm, long index finger at once but not the soft, small nipple (p. 46).

He concluded from his researches that newborn children are already able to make sucking movements but cannot nurse well without assistance. He further observed that there are important individual differences among children in the acquisition of this essential skill and that some very clumsy children may never learn to nurse successfully.

III

Replication by Genzmer

Kussmaul's dissertation (1859) was partially replicated in the early 1870s by Alfred Genzmer, a doctoral candidate in "Medicine and Surgery" at Halle-Wittenberg University (1873). In contrast to Kussmaul, Genzmer did not summarize the relevant research literature in the intervening years. His study focused exclusively on "... the sensory perception ..." of newborn children.

Genzmer collected his data at a school for midwives in Leipzig. It is, therefore, very likely that this subjects came from the same lower middle class sample of the population as those studied by Kussmaul in Heidelberg almost 15 years ago. Although he claimed to have studied the sensory behavior of 50 infants he did not record the same observations for each of these neonates. For example, his results on the sense of touch were derived from only 20 children but pain sensitivity was assessed in "almost 60 Children".

Kussmaul did not examine the sense of pain and the reactions of newborn children to oxygen deprivation empirically for obvious humanitarian reasons. Instead, his discussion of these topics was based on incidental rather than on experimental observations. Genzmer, however, collected his own data about both of these sensitive topics (1873):

> The sense of pain is exceptionally poorly developed in the neonate ... During the first day (of their lives) I pricked premature infants with fine pins in the most sensitive parts (of their) noses, upper lips and hands so intensely that small droops of blood oozed from these injuries. They gave no evidence of discomfort - not even a slight quivering (was observed) (p. 12).

Genzmer acknowledged that "the wetness of their eyes increased" when he peirced the children's faces with needles but concluded that the children were only suffering from a cold (!).

He also studied the effects of oxygen deprivation by pinching closed the noses of his newborn victims and reported the following consequences (1873):

> ... after about five seconds one notices a few attempts at swallowing. Soon afterwards the children become restless, toss their hands violently from side to side, wake up from sleep, and finally begin to scream and breath through their mouths ... full-term babies tend to cry sooner, while premature infants bear the lack of air more than half a minute without a reaction (pp. 17-18).

Genzmer did not share the reactions of the mothers to his experiments with us, however. While his findings are novel and may have important implications, Kussmaul's caution and human concern are preferable by far.

Kussmaul and Psychology

Both Kussmaul and Genzmer were included among the authors whom Preyer quoted most frequently in his classic, *Die Seele des Kindes* (1892). Preyer seems to have regarded both authors primarily as convenient providers of normative information and to have disregarded the experimental sources of their data and their original methodological contributions.

Specific findings from Kussmaul's dissertation (1859) found their way into major reference works in developmental psychology from Murchison's *Handbook of Child Psychology* (1930) to Mussen's

third edition of *Carmichael's Manual of Child Psychology* (1970). A quick examination of these handbooks revealed that Kussmaul was mentioned an average of 15 times in each volume. The comparative figures for Genzmer, G.S. Hall, Preyer and Tiedemann were 10, 4, 12 and 33 quotes respectively. Unfortunately, references to Kussmaul and Genzmer seldom exceeded a few words, while Hall, Preyer and Tiedemann and their approaches attracted substantially more attention and discussion.

Among standard histories of psychology, only Hehlmann's *"Geschichte der Psychologie" (1963, 1967)* mentioned on page 153 that Kussmaul published "his investigation of the mental life of newborn children (1859) in the same year in which Darwin's *On the Origin of Species* (1859) appeared". Moreover, Hehlmann places Kussmaul's name and accomplishments at the very beginning of his chronology of child and adolescent psychology (1967, p. 435). A facsimile copy of Kussmaul's dissertation, which has recently been reprinted in the USA (Bringmann & Balance, 1976), and an English translation, which is being readied for publication, will hopefully introduce this seminal work to modern historians of developmental psychology.

A "Modern" Successor of Kussmaul

In recent years test procedures, which are strinkingly similar to those applied by Kussmaul more than one hundred years ago, have been standardized to provide objective information about the behavioral repertoire and developmental potential of newborn children (Self & Horowitz, 1979). The basic research has been carried out by T. Jerry Brazelton, the Medical Director of the Child Development Unit of the Children's Hospital Medical Center in Boston.

The *Brazelton Neonatal Behavior Assessment Scale* (BNBAS) consists of 20 neurological observations which are rated on 4-points scales for intensity of response. In addition, 11 specific behaviors are observed or elicited with simple instruments like (Quinn, 1982):

a red spice box with popcorn kernels, a small bell, a flash light, an orange rubber ball ... (and) a paper clip ... (p. 25).

The following example of specific test items is highly similar to the techniques applied by Kussmaul and Genzmer (Self & Horowitz, 1979):

> 1. response decrement to light, 2. response decrement to rattle, 3. response decrement to bell, 4. response decrement to pinprick, 5. focusing and following an object, 6. reaction to auditory stimulus ... (p. 155).

These behavioral items are each rated on a 9-point scale. The midpoint of each denotes the expected performance of a normal three-day old infant.

The BNBAS has been found useful in the assessment of cross-cultural differences between newborn children and the impact of socio-economic conditions on infant behavior. The instrument has helped identify the effects of maternal drug taking on neonated and has been successful in predicting the physical and mental development during a child's first year.

Kussmaul's importance for the scientific study of child behavior has been expressed more than a hundred years ago by an anonymous reviewer of his study in the *Cornhill Magazine* (1863):

> Dr. Kussmaul of Erlangen ... first bethought himself of making newborn infants subjects of *experiment*. This would, no doubt, have drawn upon him the voluble execrations of outraged womankind, were it not for one mollifying circumstance ... Dr. Kussmaul ... will no doubt be forgiven for the sake of the results which so dramatically indicate the psychological integrity of the infants (p. 651-652).

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THE DANGEROUS FOOL AND THE 1838 LAW ON INSANITY IN FRANCE

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Summary

This paper deals with the consequences of the 1838 law on insanity in France, especially on forensic psychiatry.
To this effect, the historical and contemporary legislative, sociological, and theoretical contexts of this law were analysed.
It is demonstrated that psychiatry became a medico-legal discipline in the interest of public safety once the notion of dangerousness was legally accepted.

From time immemorial there have been dropouts, vagrants or lunatic people. It is usually assumed that, as far as lunatics are concerned, in 1838 a sociological dividing line was drawn between a previous period when lunacy was familiar and integrated into everyday life, and a subsequent period when it was kept silent, locked up, and excluded from social life.

It might be necessary to start by focussing on two arguments that blur, or rather move that dividing line somewhere else. On the one hand, were the lunatic really integrated into society? They were laghed at, too, or exhibited, or feared. On the other hand, their being locked up did not date back to 1838; the "great locking" as described by Foucault (1972) dated back to the end of the seventeenth century, together with prisons and hospitals, where lunatics were to be found beside prisoners and poor or sick people.

And yet, 1838 is a shock-date, when psychiatry was rushed into a social and therapeutic function. It was granted great administrative facilities; an immense task was awaiting it, which it has never, in spite of some frictions, stopped fulfilling within the

frame set in 1838. In that way, 1838 really marked the advent of psychiatry in France. It is the date when lunacy came out as a true social matter taken in charge by public authorities on the two levels, therapeutic care and social control, as pointed out by sociologists of psychiatry, in particular Foucault and Castel.

The 1838 law was an organic law with two aspects; it both rarified what had been done, it answered suggestions and wishes from political men, it took into account claims from doctors and philosophers moved by the fate of those lunatics and at the same time, it took radically new steps, going further and instituting a new order. My hypothesis is that this new order was established by making dangerousness the effective criterion for lunacy. At the same time it meant making lunacy a matter of state concern and a matter of safety for the citizens; a matter of individual weakness, and a requisitioning psychiatry.

In order to consider the 1838 law and analyze it from that point of view, I first have to examine which elements it dealt with. That's what I mean to start with in the first part of this article; next, I must define the breaking off from what was done before and the innovation it brought.

I. Before 1838

At first sight the different ways lunacy was dealt with before 1838 seemed confused and even contradictory. It is no surprise if we consider them as reflecting the fears and the successive visions of the world rather than proceeding from a consistent and coherent approach. Hintermeyer (1981) talking about the history of social work, determines periods following each other chronologically, leading to prevailing customs, together with surviving customs having had their full meaning at a previous stage. A similar process seems to be at work in the realm of lunacy, giving the impression of cross or incoherent purposes.(1)

1 - 1 Monsters, lunatics and dangers

My purpose here isn't so much to show a progressive assimila-

tion in a linear history from the monster to the lunatic, as to wonder about the disturbing similarities between the ways monsters and lunatics were represented and dealt with; there was a wavering movement between the inclusion into the realm of creation and the exclusion from the realm of reason.

The relations of men with this "elsewhere", this gap, this stray from the norm which monsters are, or their relations with the other side of reason which is lunacy varied according to the times. The different shapes these relations took might allow me to consider how the notion of danger, linked with the representation of lunatics and monsters was built up, and how little by little conjuring habits settled as well as numerous tentative ways to get "scientifically" hold of those categories so disturbing to the human mind. Medieval theologians knew about Aristotle and St Augustine's ideas about monsters. For the first to monsters belonged anything in which nature got out of the limits of the original type; so Aristotle questioned about the monstrous meaning of the difference which is the formation of a female instead of a male, primeval imperfection, and woman escaped the criterion of monstrosity only owing to her necessary part in the survival of the species. For the latter a monster meant a departure from the norm and the form, and was distinguished by its rarity. According to Kappler (1980 ch. VI) "three ways of reasoning on monsters" can be distinguished; "the genetic type (Aristotle), the theological and aesthetical (St. Augustine), the exemplarist and normative, referring to models monsters would depart from as bad copies".

Towards the end of the Middle-Ages a shift slowly took place in the meaning of monsters, from a remote and necessary cosmological monster toward an individual monster, hic et nunc, a fall of the monstrous towards the diabolical. Under the joined influences of calamities, such as epidemics or massacres, and of the first scientific breakthroughs of the preclassical era, monsters and the world lost their sacredness. From the fifteenth century on, monsters got out of the gap in man caused by anguish in front of the evident disharmony of the world, and rushed into everyday life,

art, religion, theology. Monsters, women and devils more and more often met in works of art, as if they were created out of the perils brought upon men. One might wonder about a possible parallel (drawn) between lunatics and monsters when reading in Kappler (1980) a passage from Mandeville's work dating back to 1598-1600: "Monstrosity is a kind of lunacy and in the very first place an insanity of the imagination". Monsters and insane people were distinguished by (a difference shown in) their straying from the norm.

Lunacy is, in turn, viewed as reason taking issue over itself, or the beast which reason endeavoured to defeat. Such an ambiguous attitude is to be found in all societies. It is chased away, exhibited as an image which threatens everyone.

It seems remarkable that in the fifteenth century the theme of lunacy spread more and more widely and obsessively in art, iconography and literature.

Foucault (1972) showed in *Histoire de la Folie* how the classical age expelled the lunatic out of society. At the same time a parallel process expelled monsters out of art. They were first expelled out of the religious field and, after the Council of Trento, the Church refused that monsters be represented. A similar process of exclusion tended to exile monsters and lunatics, which disturbed the order and harmony of Nature and Reason. With monsters anguish and terror settled. They were rejected together with extravagancy or vain delirium, just in the time when the lunatic was looked upon as the symbol of all the threats awaiting man in the quest of his salvation.

1.2 How lunacy was taken into account on a social level

One of the great social fears during the eighteenth century rose from vagrants and beggars. In 1687 already the King reminded his subjects that because of serious disturbances caused by idleness he absolutely prohibited beggary, punishing it with flogging or the galleys. From 1700 to 1750 seven successive laws tried to prevent beggary and vagrancy, with sentences raging from flogging

or wearing the iron collar to the galleys, opening, in return, hospitals and charity houses where workshops would provide them with work to pay for their subsistence. But the very fact of these successive laws shows (in itself) how ineffective they were, were it not confirmed elsewhere. A *Mémoire sur les vagabonds et les mendiants* written by an Agricultural Society in 1763 still showed them as a serious danger for the inhabitants of the country and the worst plague for industry and agriculture. They were a (very heavy) burden, (not working) leading debauched lives and living on the very heavy contributions they imposed on the peasants who were frightened by their numbers, their harmful secrets to kill cattle, and their arsons. The Academy of Sciences, Art and Literature in Châlons-sur-Marne set the following question for the 1780 competition: "How to reduce beggary by making the beggars useful for the State without making them unhappy".

Mixed with the vagrants, begging, the lunatics were found in the same hospitals (or houses) as the sick, the poor and the criminals. A report from the Committee on Beggary of the *Assemblée Constituante* in 1790-1791 showed the example of *Bicêtre* where one locked up indiscriminately "men, epileptic children, scrofulous, paralytic, insane people, locked up by order of the King or acts of Parliament, children arrested by order of the police, or sentenced for theft or other offences, children without any vice or illness, men and women treated for veneral disease" (Bloch and Tuety, 1911). The same report mentioned the lunatics locked up in the different hospitals in Paris in the following chart:

HOUSES	RAVING MAD MEN	RAVING MAD WOMEN	IMBECILE MEN	IMBECILE WOMEN	EPILEPTIC MEN	EPILEPTIC WOMEN	TOTAL
HOTEL-DIEU	42	32	-	-	-	-	74
LA SALPETRIERE	-	150	-	150	-	300	600
BICETRE	92	-	138	-	15	-	245
CHARENTON	1	-	77	-	4	-	82
LES PETITES MAISONS	22	22	-	-	-	-	44
LES 18 MAISONS DE POLICE	6	10	131	136	3	-	286

In the rest of the country too, the lunatics were locked up with other categories of people; in the Harcourt Tower in Caen, for instance, from the seventeenth century on dangerous prisoners and lunatics had been kept together. The town-council allowed their families to put them there provided they paid for maintenance (Quetel and Morel 1979).

In those places which, of course, should not be compared with our modern hospitals, the living conditions of the lunatic ranged from total insalubrity to a tolerable sanitation. Treating them for their mental disorders was out of the question; at the very most the warders "tried to make captivity as nice as possible" (Bloch and Tuetey 1911 on Charenton), and in an other case they "exhibited their boarders for the first rustic willing to pay 6 pence to stand and stare at them". (Mirabeau 1788, in: Quetel and Morel 1979) (trad. author).

The lunatics come to those places with the help of the constabulary, or they were put there by their families who had then to state their identity and pay for the boarding, or they were sent by *"lettre de cachet"*. It is certain, however, that the use made of these letters for repressive puroposes came as a corruption of their first attributions. (Quetel, 1981: the general ordinance made after the complaints, grievances and remonstrations of the states assembled in Orléans in 1560). This ordinance is intersting from two points of view; first it is the first document to use the term *"lettre de cachet"*, which tends to imply that they were rare before, on the other hand it denounces the bad use made of them. Brentano (1903) quoted Malesherbes (p. 11) who divided the *"lettres de cachet"* for police and criminal matters into three categories; the third covered, "those in charge of defending society against subjects who would be dangerous and who would disturb its good order and quiet".

The lunatic seemed to be a relatively important target, and represents 34% of known motives of imprisonment, before dissoluteness, lose behaviour or violence. As early as 1646 in the Bastille were written down in a recapitulative list prisoners locked up

officially for lunacy or insanity; so were the Knight of Lorraine, since 1636, Godonvillier a Captain, the Lady Vezilly in 1659 ... and so on.

1.3 The inheritance of 1789

The Declaration of the Rights of Man and Citizen conveyed all the philosophical ideas of the eighteenth century. The "new rights" were asserted from Rousseau's conceptions in *"Le Contrat Social"*. Man was "born good", "society makes him miserable" and "Governments should rule only with the consent of the governed". Law closely followed the permanence of human nature on which it was based, uninvolved in social relations. The State had no other "raison d'être" then to guarantee the natural faculties of individual rights, then, set up as many limits to the action of the State. So liberty was defined as "being able to do whatever doesn't do wrong to anybody", and the norms of natural rights were those that ensured others the enjoyment of the same rights ... the bounds cannot be determined by law; "law only has to prohibit actions harmful to society" (art. 11).

On the other hand it is interesting to note that the preamble settled some social matters in a series of measures concerning public help; bringing up deserted children, relieving the poor suffering from disabilities and providing work for the unemployed, as well as creating and organizing public education. In those occupations one might detect the influence of Adam Smith's "theory of moral feelings" (1759), a moral philosphy based on the natural sympathy men feel toward each other, and the natural need for harmony in feeling and dispositions.

Lastly, I would like to emphasize the radical shift in the notion of the state which took place between 1614 and 1789; it swung from the religious to the lay field; "what was condemned was no longer sins which offended God, but misdeeds contrary to accepted standards of good behaviour".

From 1789 to 1818 legislative measures "in favour" of the insane were scarce and contradictory in their effects - e.g. the closing of many establishments run by the clergy. Always dependant

on the 1789 declaration of the rights of man, the terms of the memoranda sent round by the judiciary mostly reflect their care to lessen the double danger for individual liberty, either excess of tolerance or excess of the arbitrary (Report by Constans, Lunier and Dumesnil, 1874). Humanitarianism inherited from the age of Enlightment was not to be found in the legislator but it was conveyed by some individuals in the medical profession in attempts, as numerous as repeated - and that until 1838 - to draw the attention of governments and public opinion on the fate in store for the insane.

1.4 Insanity as a matter of concern and anxiety

At the end of the eighteenth century and the beginning of the nineteenth, insanity made doctors and political men anxious for two reasons: the insane should not be treated as criminals, and wasn't insanity at work in criminals?

The conditions in which the insane were locked up moved doctors: Pinel let chains drop in 1792 in Bicêtre, expecting to relieve the fate of the insane -to draw the line between what, in their fury, was due to their state and what was due to their present conditions where the slightest movement was impossible. Esquirol twenty years later got indignant: "how is it possible that the illness which affects man in the most precious part of his being should not have a home where those who suffer from it be alone, welcomed and treated honorably ... where those admitted should not have to be ashamed to be mixed up with the children of crime and immorality? (Dictionary of Medical Sciences, quoted in Quetel, 1979). Esquirol (1818) submitted a treatise to the Ministry of the Interior about the establishments for the insane in France and the means to improve them. In this text he pleaded for the rehabilitation of the insane as persons and social beings, "they are fathers, faithful wives, honest merchants, skilful artists, warriors dear to their homeland, eminent scientists or scholars; they are proud and sensitive souls, unfortunate people struck by a great calamity, in a state of misery increased by the blows inflicted by society which treats them worse than criminals" (p. 399).

The insane were again given human souls, they were less and less considered as degenerated and ignominious, and more and more as unfortunate. Helping them became a social duty. Part of the medical profession showed their concern to make insanity an illness (breach of integrity) different from crime as an offence. In fact a double movement was initiated; if insanity tended to be different from crime, the question of crime as insanity became more precise. There was a shift in emphasis, insanity becoming an organizing category grouping together and accounting for scattered behaviours.

The new nosography described by Esquirol (1828) clearly showed this movement. Monomania was distinguished by "strokes of insanity" in a clear-headed mind, or by a partial delirium concerning one of the mental functions, the others remaining untouched. According to Esquirol (1828) there were several types of monomanias according to the object of delirium: erotic or reasoning monomania, alcoholism, incendiary, homicidal monomanias ... The illness wasn't foreshadowed by any great pathological sign and its causes were "ordinary"; it was the same for a fair number of nosographies of the time-exaggeration of ideas, dietary indiscretions, violent passions, miscalculating self-esteem and ambition injured the moral or the intellectual spirit.(2)

The question then must be asked whether or not a criminal acted under the influence of insanity when doing his criminal act and, if the case arose, if he should be treated. It was necessary to have checked the hypothesis of insanity in crimes by competent doctors.

The case of Pierre Rivière, tried in 1835, was examplary. Several psychiatrists successively studied the case of this parricide and fratricide. Castel (1973) accounted for three expertises; Bouchard's, a general practitioner, Vastel's, an alienist in Caen, and Parisian alienists from La Salpêtrière (Esquirol, Marc, Orila, Pariset, Leuret, Mitivié). The wide differences in interpretations clearly show how far doctors were from being unanimous; monomania appeared as something new, variously received. Bouchard found in Rivière no abnormality of organic origin and concluded

criminal responsibility. Vastel looked for a weakness of intellect and understanding as a remote and prior cause of endemic insanity, which would have burst out when the crime was committed; the link between crime and insanity was made, but not as monomania. Finally Esquirol and the La Salpêtrière doctors, the "promotors" of monomania, declared a shared responsibility for Rivière; it was attested by the six who signed with all the weight of their number, their skills and titles.

II. 1838

Within this short article I am not able to make an inventory of all the new links between danger and insanity which have been branching out from 1838 to the present day. I would be content with giving an outline, in particular on the grounds of the close relation between insanity and politics, crime and insanity and of, at the same time, an ordinary everyday contact with insanity; I tried to analyse what in the law of 1838 itself bred those new trends and allowed them.

The 1838 law mostly settled three matters; the setting up of a political and administrative plan to point out and hunt down insanity that was given means to work; the definition of the danger inherent in insanity in terms of disturbance of public order and breach of individual safety; and preventing this danger before it became effective and as long as it remained possible or probable.

II.1 Insanity and political power

The concern of governments for insanity did not date from 1838, but was different then in shape and prospects from that of previous times. Without making a review, I'll take two instances and view them as "sociological symptoms". In 1784 the memorandum sent round by Breteuil to the administrators of the Kingdom about the *"lettres de cachet"* specified: "the first class includes prisoners whose mind are insane and who because of their imbecility are unable to behave in society or because of their fury would be dangerous. The point is, concerning them, only to ensure that their

state is still the same and, unfortunately, it becomes absolutely necessary to keep them locked up as long as it is acknowledged that their freedom would be harmful for society or a kindness of no use to themselves". In 1804 the *Code Civil* provided that "anyone come of age in an usual state of imbecility, insanity or fury to be interned even if he had lucid moments".

In 1784 or 1804 the danger insane people were for society was pointed out and "treated" by means of putting them away and suspending their civil rights, but without providing for any explicit procedure to that end; fury was one thing, but when it became dangerous, having to protect society from it was another. In 1838 were instituted internments, ordered by public authorities (usually called *"placements d'office"* i.e. appointed internments). The decision always finally lay within the competence of local representatives of the government, prefects and mayors (articles number 18, 19, 20, 21); danger and insanity were closely interwoven (article number 14, 18); insanity might compromise public order and individual safety and it became a matter of state concern. What is more, the use of the conditional verb forms, "would compromise" (art. 18), and "might compromise" (art. 14) clearly showed the potential danger (which was not consumated); the big field of prevention lurked in those conditional forms. The moment the danger became real was to be attested by public authorities; the duty of psychiatry was to apply laws and to follow the situation; medical authorities assisted administrative and political authorities.

On the other hand danger was not defined, it was pointed out by the attributive adjective "imminent" (art. 19), and public authorities had to detect it with the help of, among others, common knowledge (art. 19); the danger of insanity was a matter of common sense.

From then on the social setting of the close relation between danger and insanity was given; it might include forms of individual danger (monomania, homicide ...) and collective danger (after *La Commune* in 1871 new categories were defined of revolutionary insane people, of rebellious insanity ...).

II.2 Crime and insanity

Esquirol, as he endeavoured from his theory of monomania to allow for insanity in crime, wished he could spare ciminals a fate they did not deserve; they should meet with treatment rather than punishment. One might wonder about the apparent reversal of this proposition: did'nt insanity as a mitigating circumstance of the offence seem more overwhelming, and which is more, didn't the link between crime and insanity run the risk of becoming systematic? Psychiatric experts' reports were then to be found, along with others, in files in criminal trials (3).

There had already been expertises of all kinds by doctors before 1838. It was the time when the medico-legal field expanded along various lines, from the chemical analysis of the viscera of victims to the phrenological, clinical, social, and psychiatric analyses of murderers.

It is impossible here (but that work is being done) to account for the diversity in opinions and theories among the doctors and lawyers who were the experts of the time, as regards the causes of criminality. I'll give a few short instances. Bellart in 1793 (quoted in Foucault 1972) pleaded and defended a worker blinded by passion for his mistress, which blindness led him to murderous insanity, and partly excused this irreversible act. The *"Gazette Medicale"* in January 1836 (quoted in Lacenaire 1968) attempted a phrenological analysis of Lacenaire, a thief and a murderer, and thus voiced its astonishment: "phrenologically Lacenaire is a saintly man granted all the qualities of a good mild sensitive and religious man ... weren't facts here". Marc (1840) about Selestat, a child-killer stood up for the argument of a maniacal fit leading to murder; the presiding judge sensitive to his words underlined the weakening of the intellectual faculties of the accused, finally acquitted by the jury. Don't forget Bouchard's, Vastel's, and the Parisian doctors' report about Pierre Rivière in 1835. In other trials like Lafarge's in 1840 appeared toxicological expertises.

The greatest originality of the 1838 law in that respect was to point out psychiatry as an expert science - may be because

psychiatrists, and in particular Esquirol, direclty inspired it and suggested drafts for it - on condition that it should build up its own theories and practice in that field.

One of the most important tendencies to explain crime as insanity remained excess of passion, or bad life (bed hygiene and unprincipled life). So was it for Esquirol before 1838 in his theory of monomania, or after 1838 for Brierre de Boismont (1856) for whom the passions remained most important with "their harmful influence on determinations when no longer directed by reason, their weight in the pans of offences, misdeeds, crimes and insanity" (p. 352). For him bad passions - jealousy, envy, hatred, and revenge - led to hypochondria, dark ideas, melancholy, spleen, monomania and suicide.

II.3 Ordinary insanity

Nosographies at the end of the eighteenth century and the beginning of the nineteenth became precise, the old names of fury, mania and melancholy were diversified into apoplexy, catalepsy, epilepsy, hypochondria, melancholy, mania, dementia, idiocy ... (Pinel 1813) or intellectual and affective monomania ... (Esquirol 1828). Etiologies on the other hand gathered and found their ground either in disturbances of origins or psychological or mental conditions ... those are very ordinary in the end; deep fears, too sedentary ways of life, excess of narcotis, or of alcholic drinks, sadness (Pinel 1813) or bad education, false and exaggerated ideas, credulous minds (Esquirol 1828).

The ordinary aspect of insanity was even more evident in the case of monomania (Esquirol 1828) where precisely in the same individual, delirium stood by the side of reason; "monomaniacs have a feeling of general well-being, they are of happy, merry, communicative dispositions ... but get easily angry, refusing contrariety and constraints".

In this context the historical interest of the 1838 law does not lie in a definition of insanity as an ordinary fact, since the law in no way built a symptomatology, but it offers an official frame to the existing symptomatology.

Conclusion

This turning point in 1838 is of some interest now to social psychology for two reasons; because it settled new modes of taking into account of, socially speaking, a category of marginal people - and one might wonder about their sociological and psychosociological repercussions (social reaction and its effects, to use the terms in use for some twenty years now); and above all, there was coming out and being built up this notion of dangerousness which was to be so important later on as regards deviations.

In 1838 the state asserted itself as a protecting state for citizens, preceding the welfare state if we follow Rosenvallon's (1981) analysis of social interventionism; the state managed everyday life.

Insanity definitely became doctors's business, and lay doctors they were. While Orfila's report in 1837 (quoted by Trénard, 1969) put forward the very small number of doctors in France, trained in three Medical Schools -Paris, Montpellier (569 students), Strasbourg (170 students), they "entered the public arena before stepping into the anteroom of power" (Léonard, 1981). The *Academie Royale de Médicine* created in 1820 had to answer the government for all that was related with public health: epidemics, sanitary control, forensic medicine, sanitation. The sate was not the only protector, medicine was another, and the notion of public and social danger connected with insanity entitled it to the very first rank of medical concerns on the condition that medicine should define the new object of its practice.

Insanity entered its positivitst era - reminiscent of the past, of course. In fact a sign was clearly pointed out in 1838 revealing a shift from one dominating order to another; the church, the older order took charge of social problems and dealt with them or arranged them according to its dogmas. In the nineteenth century the dominating order were to become Science and Progress, which were to take charge of social problems with their principles and requirements. In that process dangerousness appeared as the triggering off and organising element.

NOTES

(1) Hintermayer (1981) talking about social work defines a first step in which power is linked with the ability to get rid of one's riches; then a second one in which charity becomes a social way of life; and a third one in which social intervention becomes scientific. It might be worth establishing a parallel between the distribution of these stages and the ways to take insanity in charge.

(2) The ordinary causes of insanity are defined along three lines; bad conditions of living, depraved morals, and weakness of mind.

(3) The questions psychiatrists are asked in todays expert evaluations are the following:

- does the examination of the subject show in him mental or psychical anomalies; if the case arises, describe them and tell precisely to which affection they belong.

- is the offence he is charged with related to such anomalies or not?
- is the subject in a dangerous state?

- is the subject within the reach of a penal sentence?

- can the subject be cured or readjusted?

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