links
ELTE
Budapest

?

Ruud Abma
rechts
JATE
Szeged

1. ESHHS Annual

29. August - 2. September 1997.

CONTENT

18

- **Irmingard Staeuble**, Freie Universitat, Berlin, Germany: Postcolonial constructions of cultural others in the human sciences

- **Renate Topel,** Otto von Guericke Universitat, Magdeburg: Herbart`s views on the freedom of the personality

- **Zsuzsanna Vajda**, University of Attila Jozsef, Szeged;
A psychoanalitically oriented psychologist`s views on education in the totalitarian regime (A case study of Alice Hermann)

- **William Woodward,** Hopper University, USA..Rescuing psychoanalysis from its critics. Sulloway, Masson, Grunbaum et alia

EXPLORING THE BOUNDARIES OF PSYCHOLOGY
A HISTORY OF THE JOURNAL 'PSYCHOLOGIE & MAATSCHAPPIJ'[1]

ESHHS-Conference Budapest
August 1997

Ruud Abma
Dpt. of General Social Sciences
Utrecht University

Jeroen Jansz
Dpt. of Exp. & Theor. Psychology
Leiden University

The Netherlands

Abstract

Starting out as a newsletter for radical psychologists, the Dutch journal 'Psychologie & Maatschappij' (Psychology & Society) moved in the past decade towards the theoretical mainstream within psychology. In this paper the major changes in the journal are described and analyzed, as well as the features that did not change: (1) the emphasis on theory and history, (2) the interdisciplinary approach, (3) the emphasis on discussion. The main transformations concerned the view of psychology: from psychology as instrumental towards the goals of the progressive movement in the Netherlands, via extreme criticism towards all scientific and professional psychological activities, to adherence to the most advanced approaches within academic psychology.

In 1976 a group of young psychologists and psychology students, connected with the progressive movement in The Netherlands, decided to start a newsletter called 'Psychologie & Maatschappij' *(Psychology and Society)* - for reasons of convenience usually abbreviated as 'P&M'. In March 1977 the first issue appeared, presenting an overview of 'progressive' activities in various psychology departments and in professional settings. The stated goal was 'to create a means of communication that can meet the need for mutual support in the ideological and practical struggle of psychologists and psychology students'.

Twenty years later, the progressive movement has disappeared but the journal still exists, although in a profoundly modified form. A casual glance at the contents of the last volumes reveals that 'Psychology & Maatschappij' has become a more conventional psychology journal, although it is also clear that it places more emphasis on theory, history and cultural themes than the other psychology journals in The Netherlands.

How and why did this 'normalization' come about? As to the 'why'-question, the answer might simply be that, because of the disappearance of the progressive movement from the early eighties onwards, the 'natural' supply of critical material has stopped and, on the other hand, the need for a progressive platform within Dutch psychology has diminished. This answer certainly has some truth in it, but if it *is* true, the question then becomes: why does the journal still exist? We will come back to this question later, after exploring the course of changes in P&M - the 'how'-question.

Generations within the P&M board of editors

In analyzing the contents of almost 80 issues of P&M, we were inspired by Karl Mannheim's idea of 'generational units' *(Generationseinheiten)*. According to Mannheim (1928), a sociological analysis of generations should not start with chopping up 'biological' (or demographical) generations into chunks of 10, 15 or 20 years, but instead focus on *generational styles* (or ideologies), of which actual generational units are the bearers. In the history of societies such generational units are normally the exception, not the rule. 'Biological' generations can develop into 'ideological generations' only under specific circumstances, namely a period of disorientation (or 'crisis') in the leading political, intellectual and cultural élites. It is then that alternative conceptions of society, always present as a ideological undercurrent usually presented in the form of an ideal or utopia, can mature into 'generational units'.

From the assertion that the late sixties and early seventies are a period of ideological disorientation or struggle, it is only a small step to the assertion that the very beginning of P&M was a generational phenomenon. As it appears, P&M in its early years was indeed part of a 'counter culture' within psychology, opposed to positivism within academic psychology and to conformist professional activities in various fields such as clinical psychology and psychotherapy, psychology of work and organization, school psychology, and so on.

Mannheim's model, however, is not really suited for analyses of small-scale developments within a single journal, since it focusses on long-term transformations within society as a whole. Nevertheless, one element proved to be useful as a source of inspiration for the present analysis. An important element of Mannheim's approach consists in 'working backwards': from detecting a specific ideological or cultural style the researcher works his way back towards (a) the actual generational units that are the 'agents' of the alternative movement, and (b) the ideological controversies present in the historical context.

In the attempt to detect at what time which changes took place within the pages of P&M, this approach proved quite useful as a heuristic device. In reading and re-reading the volumes, we tried to identify the trends that typified the journal as a whole in specific periods. This was by no means an easy task, since the parameters for identification shifted every couple of years. We then decided to take precisely that as a means for typifying the transformations. The results of this first analysis (which has to be verified by more precise, quantitative research) led to the identification of four generations in the board of editors, with a fifth generation on its way.

In the next sections, each of these 'generations' is first portrayed (including the changes connected to each of them), and second, the three persistent features of the journal, in comparison to other leading psychology journals in the Netherlands, are presented and commented upon.

(1) Progressive psychology (1977-1982)

In the first period, P&M mainly provided support for various 'progressive' projects within Dutch psychology. This went hand in hand with an 'instrumental' view of psychology, as a more or less 'technical' aid to the emancipation of 'repressed groups' in society, varying from industrial manual workers to housewives (or women in general). An obvious category in need of support

was of course the patient in psychiatric institutions. The presupposition was that psychology (and psychiatry) could either be used conservatively as an instrument of the ruling classes, or, on the other hand, as a means of assisting in the struggle of 'progressive forces'.

However, this rather simplistic conception of psychology as being value-neutral in itself, was soon challenged by another, more philosophical current within progressive psychology, concentrating (a) on analyzing the implicit values within psychological theories and instruments (such as intelligence tests), and (b) on developing alternative approaches to psychology. The main sources of inspiration in both were the 'Russian' or 'Soviet' psychology (Luria, Leont'ev, Vygotsky) and the more recent 'Marxist' psychology of the Berlin School (Holzkamp and his collaborators).

Although the connection between theory and practice was said to be the main goal of the progressive movement, in fact 'theoretical' and 'practical' progressives were continuously opposing each other on this very issue, only to be temporarily united under the banner of marxism or socialism in the confrontation with representatives of 'bourgeois' psychology. Intellectually, this state of affairs of course was not satisfactory to either the theoretically or practically oriented members of this generational unit. The main source of frustration appeared to be the limited practical value of 'Marxist' psychology, which was also a point of concern for Holzkamp and his staff (see Staeuble, 1995).

(2) The cultural implications of psychology (1983-1987)

The second board of editors tried to resolve this problematic situation by introducing two 'new' types of theory in the journal, *cultural studies* and *psychoanalysis*. The first was inspired by the work of the British *Centre for Contemporary Cultural Studies* (CCCS, Birmingham), where progressive sociologists and historians such as Stuart Hall, Paul Willis and Richard Johnson tried to develop a workable Marxist cultural theory. In their analyses of youth culture, working-class culture and media, language and ideology, they tried to combine French structuralist Marxism and anthropology (e.g. Lévi-Strauss, Althusser) with American symbolic interactionism (Becker, Goffman). In P&M, this approach was mainly used in the analysis of youth subcultures and the cultures of ethnic minorities.

Although certainly helpful in making Marxism more concrete, the 'cultural studies' approach, being sociologically and historically oriented, could not provide an alternative within psychology; in fact, it lead to a strengthening within progressive circles of the already tangible distrust of psychology in general. Influential books from abroad, like Lasch's *Culture of Narcissism* and Donzelot's *The Policing of Families*, had already paved the way in The Netherlands for a critical appraisal of the cultural influence of the work of 'psy'-professionals (the 'psychologization of culture', see Abma, 1994). Focussing on individual psychological problems did not get to the heart of this issue, since the real causes of individual problems were social and material, rather than psychological, so it was said. Even psychiatric disturbances were to be seen as rooted in society; here, the heir to antipsychiatry, Italian 'democratic psychiatry', was highlighted as the new path to follow.

Strangely enough, the anti-psychological bent was accompanied by a renewed interest in the most 'psychological' theory of all, *psychoanalysis*. Although identified as the source of the evil 'psy'-complex, psychoanalysis was also seen as the theoretically most interesting theory on

the vicissitudes of human subjectivity (see Abma, 1987). Again, in this 'retour à Freud', French structuralism (Althusser, Lacan) led the way. Thematically, in P&M, this interest merged with the 'cultural studies' approach in a growing exploration of cultural themes, varying from fatherhood to pornography, and mainly concentrating on what was happening in the family.

In general, psychology changed from an 'instrument of change' to an 'object of study' during this period, and theoretical approaches to both psychology and psychological issues came from a variety of interdisciplinary fields such as 'psychohistory', 'cultural studies', psychoanalysis, criminology and the literary sciences. This does not mean, however, that Marxist psychology or the critique of 'bourgeois' psychology had totally disappeared from P&M - they had only been reduced to a more marginal position.[2]

(3) A theoretical vanguard? (1988-1992)

Compared with the previous boards, this board of editors appeared to be more modest in its aims: no longer was a grand new theory sought for as 'the' alternative to dominant psychology (be it Marxist psychology, psychoanalysis or structuralist theory), but instead the focus was redirected towards developments within psychology itself, and more precisely, to alternative *theoretical* developments, such as 'social constructionism' (Gergen, Moscovici, Billig) and the new 'cognitive' psychology. Both, however, were presented modestly as 'interesting new lines of theory and research', that deserved more than a place in the margins of mainstream psychology.

Authors were increasingly recruited from regular psychology faculties to contribute to P&M, whereas in the years before, authors from a wide variety of disciplines were contributing, such as historians, sociologists, cultural anthropologists, pedagogues, philosophers and literary scholars. Thematically, too, attention shifted from cultural themes to 'real psychological' research problems. Apart from cognition, new ideas in the study of *emotions* received much attention. Naturally, the psychological turn was commented upon from a social and cultural angle, but this remained largely confined to the discussion section of P&M.

In the history of P&M, this period appears to be the watershed of the former 'critical' psychology and the new 'theoretical psychology'. The change was signalled by the reactions to a special issue on 'obedience', which, claiming to continue Holzkamp's critique on the psychological experiment, focussed on a more sophisticated design of the famous Milgram experiments. However, in the reactions the reference to Holzkamp was seen as no more than lip service, and the issue was condemned as being the product of former 'angry young men' landed safely on the cushions of a steady university job.

(4) Psychology as such (1992-1995)

Although this period could easily amalgamated with the previous one, in the sense that the trend towards academic psychology was continued, with developmental psychology as a new focus, it stands apart for two reasons. First of all, there was a renewed interest in the history of psychology and methodology. For instance, special issues were devoted to 'One hundred years of Dutch psychology' and 'The psychology of science'. Secondly, current social problems reappeared on the agenda, as demonstrated by a special issue on 'Spangen' (a poverty area in the city of Rotterdam) and a debate on the alleged merits of 'The Bell Curve', bearing some

resemblance to contributions on intelligence testing in the early volumes.

(5) Which way to go? (1996-)

Of course, a sound judgement of the current board of editors is hard to give, considering the short period they have been in office. The six issues that appeared under their responsibility, however, create the impression that this board is returning to the 'cultural studies' approach, as is illustrated by the appearance in the journal of post-modern themes like the body and aesthetics.

Nevertheless, subjects that are very close to mainstream psychology are treated in a more conformist way. This is illustrated by the issue on 'Psychology and health', that merely presents an overview of this new subdiscipline, while earlier on in P&M some of the presuppositions of health psychology and mainstream psychosomatic medicine were criticized rather heavily.

Theory, history, interdisciplinarity and debate

In the landscape of Dutch scientific journals P&M has had and still has an interesting position. On the one hand, it is a *psychology* journal, mainstream psychology being the major enemy in the early periods, and a field of exploration in later years. On the other hand, it is a *critical* journal, which has never been satisfied with the state of affairs within psychology. With the adjacent Dutch psychology journals it thus shares a focus on psychological issues, but differs in approach, in that it in one way or another has proved itself to be 'alternative'. With cognate critical journals it shares a focus on new approaches in the social and cultural sciences, especially Marxism and in the more recent period social constructionism.

The first persistent element in P&M from the early years onwards has been the focus on *theory and history*. Many articles and a couple of special issues were devoted to the history of psychology, its methodology and also to the broader field of the history of mentalities. In a way, historiography also bridged the gap between alternative approaches and mainstream psychology in The Netherlands.[3] Although the authors (and editors) usually preferred a 'contextual' approach to the history of psychology, in later years also biographical contributions, for the most part on Dutch psychologists, were accepted. The fate of theory was somewhat more capricious, in that it depended on the preferences of the board of editors: from heavily Marxist in the first period, to cultural and psychoanalytical in the second period, mainstream with an alternative bent in the third and fourth period, and seeming to return to a broader cultural approach in the last period.

Apart from its focus on theory and history, P&M even in the third and fourth period stood out among the psychology journals by its *interdisciplinary outlook*, its 'looseness' in the disciplinary sense. Non-psychologists were often invited to contribute, ranging from philosophers, psychiatrists, sociologists, pedagogues, historians and anthropologists in the first, second and fifth period, to mathematicians, medical researchers and biologists in the third and fourth period. More significant, however, was that the invited scholars themselves always appeared to be operating on the boundaries of their own discipline, or rather, were involved in

interdisciplinary projects. This obviously met with the transgressive curiosity of the successive boards of editors.

Finally, P&M has always stimulated *discussion,* even going as far as allowing contributions that shook the very foundations of P&M itself. These, however, were exceptions: in most cases the debates focussed on either the (actual or potential) social, political and cultural role of psychology and psychologists, or the merits and weak spots of specific theoretical approaches within or adjacent to psychology. Naturally, there also were debates on historiography, theoretical psychology and interdisciplinarity.

Discussion and conclusion

Starting out as a radical journal, P&M has become in the course of its history more conventional, in the sense that it moved towards the theoretical mainstream in psychology, although it kept its focal characteristic of being a non-empirical - if not theoretical - journal with an open mind for the history of psychology and theoretical alternatives. Its starting point thus provided P&M with a long-standing interest in the foundations of psychology (theory and history) and, at the same time, a sceptical or relativistic view of psychology, which manifested itself in its exploration of the boundaries with the neighbouring - mainly social - sciences.

How then, to explain these transformations? As mentioned above, in its early years P&M was heavily supported by the existence of the progressive (student) movement within psychology and the social sciences in general. In the first ten years it succesfully exposed the ideological biases and theoretical weaknesses within mainstream psychology and the cultural domination that was inherent in its practices. As long as the movement existed, this provided P&M with both an attentive audience and a continuous stream of contributions.

Quite early on, though, P&M decided to become a 'serious' journal, in the sense that it would select articles on the basis of qualitative criteria. This was a first step away from 'the movement', where 'democracy' prevailed over scholarly qualities. At the same time, the principle of selection on the basis of quality increasingly favoured contributions from university staff members; although many of these had been part of the progressive movement, they tended to become socialized into the dominant academic culture, and thus more conformist in outlook.

The main factor in the decline of the progressive movement was probably the fact that it only sporadically succeeded in creating its own anchor points, its own *institutions*: it depended heavily on the continuous activities of its members, and when their motivation ran out, the movement itself dried up with it. In this sense, it is a paradox that one of the institutions it *did* create (P&M) survived - although at the cost of having to recruit contributions from elsewhere.

But this is not the whole story. To stay alive, movements need a viable alternative, a lasting ideal. A movement directed at changing society by changing the role of psychology has to develop a coherent and useful *theory.* Unfortunately, 'progressive' psychology shared with its (former) opponent, mainstream psychology, a rather diverse conception of psychological processes and human activity. And like mainstream psychology, alternative approaches to psychology and psychological issues, still lack a commonly shared solid theoretical framework.

Finally, how did P&M manage to survive? A reliable answer to this question would of

course need a survey among its readers, but it might be interesting to speculate a little on this in advance. The rapid decline of subscribers from 1200 in 1980 to a 600 a few years later is probably connected to the disintegration of the progressive movement within psychology. After 1985, the number of subscribers remained more or less the same, but it might have changed in composition. Our guess would be that a new generation of theoreticians and historians of psychology has replaced subscribers from neighbouring disciplines, and readers that have moved from general to specialized activities. In short, P&M has found a niche in the Dutch market of psychology journals, called theory, history and culture.

Notes

1. A more detailed analysis will be published in P&M, September 1997: R. Abma & J. Jansz, *Over de grensgebieden van de psychologie. Twintig jaar 'Psychologie & Maatschappij'*, Psychologie & Maatschappij, *20* (3), in press.

2. This made Dutch followers of Holzkamp decide, in 1983, to start their own newsletter on critical psychology, *Het kritieke moment* ('The critical moment').

3. An interesting point to note in theis respect is that among the professors who supported the journal from its inception on, Dutch historians of psychology such as Pieter van Strien, Hans van Rappard and Willem van Hoorn, were overrepresented.

References

Abma, R. (1987) Niet bij psychologie alleen. Tien jaar 'Psychologie en Maatschappij'. *Psychologie en Maatschappij, 11* (nr. 40), 287-299.

Abma, R. (1994) *The 'psy complex' revisited: The psychologization of culture and the case of psychoanalysis.* Conference Paper, 13th Cheiron Annual Conference, Paris.

Mannheim, K. ([1928] 1964) Das Problem der Generationen. In: K. Mannheim, *Wissenssoziologie.* Berlin/Neuwied: Luchterhand, 509-565.

Staeuble, I. (1995) *Emancipation - A failed project? Remarks on the discourse of radical critique.* Unpublished manuscript (Symposium Heidelberg).

Vera Békés CSc
Institute of Philosophy HUF

On the so called "Basis--Superstructure" - Debate in Hungary in the 50thies.

This debate is registered among the most disrepute disputes in the history of science. Started with Stalin-papers on linguistics in Pravda, the June, 1950. In these articles, Stalin "having been asked by young Komsomolysts" - defined his attitude/took a stand against linguistical theory of N.J.Marr and his School. After and according to this started the other scientific debates: the so called Pavlov-discussion, and first of all the "basis-superstructure"-debate. There were polemics and debates on the same scenario in the East-European countries - so in Hungary. The question, whether a science belongs in that dichotomy to the changeable "superstructure", might be decisively to its position for political power. George Lukacs and Béla Fogarasi - outstanding philosophers were the central (negative) figures in this debates of Hungarian variation.

The question I want to answer is:
What kind of role political power plays in normal science?
1/ Writers on political liberalism elaborated an argument on how and why the politicians have to refrain from interfering with scientific affairs. This conception emphasizes the need of independence of science (scientific life, scientific institutions ...) from the political sphere. Either explicit or hidden exercise of political power on scientific life seems for this kind of liberal conception, to be an ANOMALY. There is no illusion for a liberal thinker that the political sphere is not the realm of social justice even under the circumstances of a liberal social system and government. In the best case this is a chance for an optimal coordinating of the different interests.
Classical liberal (political) thinkers seem to share the view about the necessity of freedom and independence of the scientists from the political power.
But this postulate belongs to the realm of "SOLLEN", and so it is just a maxim for the scientists. Besides it includes a hidden assumption,

namely: that the structure of scientific institutions in a society and the structure of political systems are (or can be) completely different, and independent from each other.

Vygotsky's "catalyst"-theory:

There is an other, a very interesting conception outlined by Lev Semenovits Vygotsky, the outstanding soviet psychologist, in 1934, ~~which conception can serve as an alternative to the above mentioned.~~ Vygotsky together with some colleagues had to answer the question, how could German Nazis convert such a respectful discipline as neurophisiology so rapidly to a fascism, and to make it subservient to the purposes of the racist ideology. According to Vygotsky politics can play a role similar to that of a CATALYST in a chemical process. Political power can modify the scientific affairs (Scientific affairs can be modified by political power)- from the point of view of the scientists - arbitrarily. And yet, the only thing however, that political power can do: is to enhance or accelerate certain tendencies, tendencies that are already had vowed in the structure of science.

With the help of this "catalyst" metaphor we can get a more complex and dynamic model for the explanation of the relation between political power and science. In this model the attempt of the political power at a direct or indirect intervention in scientific affairs is not a peculiar and rare anomaly, but on the contrary, this belongs to the normal workings of both political and scientific life. And really: In the history of science we can not find any period free of attempts at political interference. Such extreme examples of direct exertion of political power, like Stalinism or Hitlerism can give us a deeper insight in the real nature of the complicated connection between science and political power.

We all have heard about the irrationality of the Stalinist interference with and repression of soviet science. But we shouldn't forget, that ~~often the very criteria of rationality taken in a political and in a~~ scientific sense are incommensurable. Looking at the map, we can find a very rational explanation for the Stalinist pursuits: Stalin wanted to concentrate all military, economic, political, cultural and scientific power

into one and only center, that is to Moscow. And this is the main reason why Stalin persecuted such an outstanding scientist as Nikolai Vavilov, the so called bourgeois genetist as well as the allegedly dilettante linguist, Nikolai Jakovlevits Marr - at the same time. They shared one important feature: Both belonged to the Leningrad academic circles, to a center relatively autonomous from Moscow. From the political point of view - one can say - the question of the scientific conviction of this scholars was a matter of secondary importance.
It is a commonplace among historiographers of science, that these artifactual debates didn't have any reason from the scientific point of view. But there is a paradox that need an explanation. There was a rehabilitizing process after Stalin's dead and the XXth Congress of Communist Party. The soviet scientific community performed the rehabilitation of scientific schools and scholars in a very selective way - according to its no more political, but rather scholarly (paradigmatic) point of view. (E.g. Vavilov was rehabilitated, Marr wasn't.) In these developments the political question was a matter of secondary importance behind the truly scientific affairs.

The catalyst metaphor directs our attention to the inner structure of scientific life. It suggests us that we have to conceive of this rivalry: (very often in fact a paradigmatic fighting), between divergent scientific schools. From this point of view, - as we know it already from Thomas Kuhn - it is a MYTH, that science in society is pursue by a socially unified group consisting of members who share the same interest and are concerned only with Eternal Truth. A scientific collective is never homogeneous.
The catalyst metaphor focuses our attention to controversies between rival scientific schools, where in the fighting - as we know from Feyerabend - "anything goes".

On the other hand: Political power <u>can never create any new scientific theory, or any scientific school</u>. It can only protect or strengthen certain groups against others (or can eliminate scholars by political means) But the rival groups had to be present as rivals in the system of science itself. (According to Vygotsky: what the political regime can

do is to accelerate in a catastrophic way the process of disintegration in the old scientific structure, during which a lot of obscure and hidden tendencies, come to light, and become the foundation of a new scientific system.) Political power can amplify a crisis by external, artificial means, but the responses of scientific communities to this event, the progress and the resolution of this crisis itself will run according to the inner rules or laws of the scientific system. After Stalinism, for example, the soviet scientific community performed the rehabilitation of scientific schools and scholars in a very selective way - according to its no more political, but rather scholarly (paradigmatic) point of view. (E.g. Vavilov was rehabilitated, Marr wasn't.) In these developments the political question was a matter of secondary importance behind the truly scientific affairs.

Thus we get a very good model for the explication and explanation of certain shifts in the structure of scientific progress. The explanation by means of a catalyst would not be a causal explanation in the sense of David Bloor and the Strong Program of Sociology of Knowledge. As a catalyst isn't a CAUSE of change in a process, therefore arbitrary interference by some political power isn't a cause of change in the structure of scientific life. A catalyst doesn't work as a sluice-gate in the sense of Max Scheler. ("in a definite fashion and order, existential factors open and close the sluice-gates to the flood of ideas")

The catalyst metaphor can serve as a key for us, because it lightens the complex and selective correlation and interaction between science and political power.

There is a need of course for a more precise and complex investigation of this very large topic. In this lecture I couldn't touch the very important theme of the relation between ideology - political power and sciences. But I believe the way we can study these - sometimes paradoxical - links and correlations which will be also explained by the help of the catalyst-methapor in the sense of L. S.Vygotsky.

EXPERIMENTATION, QUASI-EXPERIMENTATION AND THE TESTING OF SOCIAL TECHNOLOGY

Paper to be presented at the 16th conference of the European Society for the History of the Human Sciences, Budapest and Szeged, August 29 - September 3 1997.

SUMMARY

Since the 1960s, many historical and sociological studies have argued that there are much more methodological prescriptions and codified routines in psychology (and other social sciences) than in the natural sciences. This paper presents some ideas on how to explain the difference. First of all, I reject the common explanation, based on Kuhn's philosophy of science, that an abundance of rules is a symptom of scientific immaturity. Next, I argue that to gain understanding of the methodological "hang up" in disciplines such as psychology, we should borrow from historical sociology and social philosophy rather than standard philosophy of science. I discuss the thesis that the evolution of codified routines in psychology was bound up with the development of the bureaucratic democracies in which the discipline thrived. This thesis is illustrated at the historical development of the random group design and its quasi-experimental surrogates. Finally, I supplement my explanation by arguing that the methodology of experimental and quasi-experimental designs represents a set of means for testing social technology, and by comparing social technology testing with the testing of technological artifacts such as airplanes and t.v. sets.

Trudy Dehue
Institute of Psychology
Section Theory and History
Grote Kruisstraat 2/1
9712 TS Groningen
The Netherlands
phone +31 50 36366354/66
email G.C.G.Dehue@ppsw.rug.nl

EXPERIMENTATION, QUASI-EXPERIMENTATION AND THE TESTING OF SOCIAL TECHNOLOGY

Trudy Dehue

Mainstream psychology derives much professional pride from the meticulous ways in which it has settled its scientific affairs. In most countries, students enrolling for classes in psychology soon learn that the discipline's expertise is not to be found in intuitive sensitivity or 'arm-chair theorizing,' but in methodological and statistical competence. As is extensively taught, psychology applies methodological rules and techniques equal to those of the natural sciences.

There is a remarkable discrepancy between this self-image of psychologists and the views on science developed by historians and sociologists of science. Since the 1960s, many historical and sociological studies have been published arguing that, actually, there is no such thing as *the* rules of science. The alleged transcendental or universal prescriptions for scientific inquiry are said to be mere fabrications of idealists ignoring the productive chaos of everyday scientific life. It is argued that in well established natural sciences, the neophytes learn how to do research mainly *implicitly*, via exemplars and during apprenticeship. Only in immature sciences are methods imparted as codified routines. As a consequence, the avalanche of rules in social science textbooks, and particularly in psychology, is widely mocked as the plain-man's misdirected ambition to mimic high nobility.

As to the comparative methodological looseness of the natural sciences, these studies indeed seem convincing. However, I object against the habit of *ridiculing* the profusion of prescriptions in disciplines like psychology. This habit amounts to once more elevating the natural sciences to the standard. These critics lapse into the very same tradition of reifying manners and modes attributed to the natural sciences, for which they blamed their objects of critique.

Moreover, with such mocking and moralizing, core characteristics of psychology remain as good as incomprehensible, not only to psychologists themselves but also to scholars in science studies. This incomprehensibility particularly applies to psychology's unmistakable growth and social establishment. The question which remains unanswered is

how psychology, as a discipline strongly based on codified routines, could gain its fairly secure social position.

As far as I am aware the most direct and most elaborated answer to this question is to be found in Theodore Porter's recent book *Trust in Numbers* (Princeton U.P., 1995). His is not a book on psychology. Its topic is the prolific usage of rules in an array of sciences, mostly human sciences (including medicine), and psychology is discussed in only a few sections. However, Porter's principal thesis can easily be applied to large parts of psychology as well.

In order to investigate this increase in rules in many disciplines, Porter borrows from historical sociology. Roughly spoken, the latter discipline studies the 19th and 20th century transition from human life organized in small autonomous communities to life organized in ever expanding social networks. In informal and intimate relationships, Porter argues, there is no need for strict rules. The highly structured language of numbers and formulas is the language of people who no longer interact at the basis of intimacy and who no longer accept the former natural authority of elites and higher ups. In large-scale democratic societies, where people at wide geographical and social distances have become interconnected, procedures, numbers and tables provide means to communicate and to handle social distance and distrust.

Asking about the power of procedures in a number of sciences therefore is asking about the power of procedures in society. Particularly the growing group of administrative officials who lack the mandate of popular elections and are easily accused of arbitrariness, demand rules and facts to present their decisions as fair and impersonal. In this way, the comparative methodological rigor in an array of sciences is related to the democratic bureaucracies in which it thrives. Porter amply illustrates that the phenomenon can only be understood by seeing it as a political solution to political problems.

To my mind, Porter's analysis clearly demonstrates the inadequacy of one-dimensional theories of science. The standard view picturing rule-governed disciplines like psychology as just amateurish imitations of real science, obscures the fact that the rules and techniques are crucial to the social establishment of these disciplines, and that rules and techniques

have become the very *core* of their professionalism. To gain understanding of psychology's methodological "hang up", it therefore seems more profitable to borrow from historical sociology and social philosophy than from standard science studies. Only in this way it may become visible that psychology's procedures form part of and give shape to particular social relations.

A clear example is offered by psychology's ideal research design. Roughly phrased, in psychology (and in other disciplines for that matter) the ideal research design is represented by a comparison of experimental and control groups, which are randomly composed. Most textbooks in psychological methodology describe this random group design as simply "the" scientific experiment, suggesting that it stems from the natural sciences. Somewhat more advanced methodology textbooks ascribe its origination to the statistician Ronald Fisher, who presented it in the 1930s on behalf of agricultural research.

However, my historical analysis demonstrates that random group experimentation, nowhere as much the apogee of methodological rigor as in psychology, appears to be firmly rooted in the discipline's very own professional and social history. It was accomplished in psychology before Fisher introduced it in agriculture. Moreover, rather than an instant creation by a single genius it was the unplanned outcome of a lengthy historical process. The random group design was brought about bit-by-bit when methodological practices from 19th century psychophysical laboratories were gradually adapted, extended, and codified by 20th century educational psychologists supporting procedural objectivity in educational administration. There they served to ensure as much as algorithmic rationality as possible.

And the establishment of the random group design is only the beginning of a much longer methodological story. Far from *settling* methodological issues, the ideal's establishment again gave impetus to a substantial extension of the set of codified routines in the social sciences. The application of this design often appeared to clash with other generally accepted rules. For instance, it usually is not acceptable to allocate clients for psychotherapy at random, or to create school classes randomly in order to make them comparable as to the social status of the children's parents. Therefore, under the collective denominator of quasi-experiments, a broad array of elaborate alternative designs has been

developed with accompanying ingenious statistical techniques.

Today, this methodology of quasi-experimentation is taught and employed by sociologists, by political scientists, in medicine, in psychology etc. It also offers an important basis of an interdisciplinary and international field indicated as *evaluation research* or *program evaluation.*

The historical background of quasi-experimentation also carries back to American psychology. In the 1930's and 1940's sociologists advising president Roosevelt called in the methodological help of psychologists, trained in the evaluation of education, who as experts for assessing the effect of radio programs or propaganda movies. During the war, psychologists helped evaluating the effects of instructive movies for soldiers. When American bureaucracy was further extended during the Kennedy and Johnson periods, a genuine evaluation industry was established. New so called "threats to validity" were continuously tracked down, giving rise to an enormous extension of the range of experimental designs. This methodology of quasi-experimental designs preeminently offers an example of human science research methods giving expression to the social ethics of democratic bureaucracy. It amounts to a far-reaching regularization of both the human sciences and human life.

Let me briefly get back to Porter. As said, Porter only spends a few sections on psychology. As a matter of fact, he mostly discusses psychology in the context of the evaluative turn which his book takes in the final chapters. There, Porter argues that a strong appeal to rules reflects more than the discipline's entwinement with a changing world. According to Porter, strict regulation also is an indication of psychology's *internal weakness*. The abundance of methodological rules and statistical techniques is a surrogate for real substance.

Here Porter again invokes the Kuhnian image of "normal science" with its shared paradigmatic assumptions, and accompanying unconcern about methodological matters. From this standard, he paints a picture of psychology as the paragon weak and threatened discipline, badly lacking the cohesion of undisputed theories, and therefore anxiously clutching at strict procedures.

I really do admire Porter's book, but it will not come as a surprise that I disagree

with the latter part. As I mentioned at the beginning of this talk, there is indeed an avalanche of standards in psychology. Moreover, mainstream psychology has a strong and ethnocentrist tendency of deeming its local traditions universally valid. But I don't think that much can be won by countering this bad habit in the same vein, that is by just elevating *another* definition of science --Kuhnian or otherwise-- to an indisputable standard.

I think that there is more to be gained from continued curiosity about how particular characteristics could emerge and survive than from evoking transcendental definitions of real science. In order to understand the emergence and establishment of the ideal random group design and its quasi-experimental extensions, it is important to bear in mind that this influential methodological style was not primarily devised for the Popperian aim of testing theories, nor, for that matter, the Kuhnian aim of completing paradigms.

The historical examples which I discussed point at another direction. They suggest that psychology's main methodological style was gradually constructed and extended for evaluating or testing all kinds of psychological treatments and social interventions, that is for testing *psycho-social technologies*. Much psychological research concerns controlling the efficacy of means for guiding individual behavior and social relations into desired directions. Therefore, a comparison with the testing of hard technology in engineering such as airplanes, t.v. sets, and bridges might be more illuminating than equations with natural scientists fortifying paradigms.

An important difference between social science technology testing and the testing of hard technological artifacts is that for the latter kind of testing there is not a methodology as general, vast, and standardized as for the former. To a relatively large extent, in psychology, the methodology for testing technology, has *itself* become a kind of technology, a technology for testing technology, a so called "second order" technology if you like.

My hypothesis is that the latter difference is due to the fact that social technologies most often are designed by all kinds of people. Politicians, administrators, teachers, journalists, clergy men, and the legendary men-in-the-street, they all devise ways of education or personnel selection, they have their means of opinion polling and inducing attitude change, they discuss ways of reducing unemployment or racial discrimination.

Whereas "real" engineers can demarcate their profession mainly via their products which only *they* can design, psychology needs more to prove its indispensability. It is therefore that psychology specialized in standardized procedures for testing the *validity* of answers, for evaluating the usefulness of social techniques. Psychologists employ their methodological expertise both to control *other* parties' social technologies, and to offer their *own* technologies as *the better tested ones*. This is how psychology developed its extended methodology as in itself a kind of technology, that is a technology for testing technology, a second order technology.

Looked at it in this way, methodological elaborateness and substantial minimalism seem to have been *twin* conditions to psychology's explosive growth in American and European welfare states. In psychology (and other social sciences, for that matter) methodological expertise for testing social technologies have become the core of their professionalism. I add to Porter's thesis that it is mostly via these methodological technologies for testing social technologies, that is via second order technology, that psychologists provide themselves -as well as the decision-makers who are its main clients- with the indispensable image of objectivity, fairness and disinterestedness. I think that in the end, a conclusion like this has more to offer to a normative debate about psychology and the social relations it reflects and sustains, than imposing transcendental models of genuine science.

"We are living - nebbish - in a great age!"

Hungarian psychoanalysis and politics in times of crisis

Dr. Ferenc Erős
Institute of Psychology of the Hungarian Academy of Sciences,
Budapest

The paper discusses the voluntary and involuntary involvement of Hungarian psychoanalysts with politics in various critical periods of Hungarian and European history - from the First World War and the revolutions thereafter, through the period of Nazism and the Holocaust, up to the (self)dissolution of the psychoanalytic movement after the Second World War. The utopian character of the psychoanalytic politics and the totalitarian character of the state politics will be contrasted and their relationship will be examined in details. Finally, some general conclusions will be drawn concerning the historical and political context of the development of psychoanalysis in Central and Eastern Europe.

Arthur Still and James Good
Department of Psychology
and Centre for the History of the Human Sciences
University of Durham

Relating Organism and Environment: Some Historical Reflections on the Ontology of Mutualism

Abstract

A central element in the psychology of perception of James Gibson is the notion of the mutuality of organism and environment. What is perceived is the outcome of a relationship between the organism and the environment. It is constrained by the effectivities of the perceiver and points both ways to the perceiving organism and the environment perceived. An obvious weakness of this "mutualism" is that the "reality" of organism and environment is assumed in order for "reality" to be created through a relationship between them. But does mutualism really fall into this trap? To borrow a distinction made by Tighe and Tighe (1966) mutualism is a Jamesian differentiation theory, rather than a cognitivist enrichment theory of perception and learning. As in learning a new language, differentiation involves an uncovering of structures that are not at first apparent. In William James "sensations" (the immediate deliverances of the senses) embody these newly uncovered structures directly, and therefore change during the process of differentiation. For William James and James Gibson learning is an education of attention as well as of articulation. It follows that in ecological psychology the affordances available are relative to an educated "frame of reference" (Turvey, 1992) which the individual brings to the situation. But this is not to say that the relevant structures did not exist prior to education, only that they had not been differentiated. In previous papers we have suggested that such differentiation theories are more compatible with a pragmatist philosophy of science than Gibson's own "realist" commitments. Recent work has opened the way to a deeper understanding of the radical ontology required by differentiation theories. Kadar and Effken (1994) look to Heidegger, and in this paper we attempt to frame this choice historically, and to uncover the tradition shared with James Gibson.

THE INDIVIDUALISATION OF SOCIAL PSYCHOLOGY IN NORTH AMERICA

ROBERT M. FARR
Department of Social Psychology
London School of Economics and Political Science
Houghton Street
The Aldwych
London WC2A 2AE

16th Annual Conference
European Society for the History of the Human Sciences
University of Attila Jozsef, Szeged, Hungary
29th August - 3rd September 1997

ABSTRACT

Graumann's thesis that the behaviourism of F.H. Allport (1924) was primarily responsible for the individualisation of social psychology in America is critically evaluated. It relates to the period between the two World Wars. There were other contemporary bases to the individualisation of the social besides behaviourism, most notably the cognitivism of his brother, G.W. Allport. Graumann is correct in relation to the individualising effects of behaviourism, his claim that the individualisation of the social is equivalent to the desocialisation of the individual is rejected. Two further waves in the individualisation of the social are noted beyond the one described by Graumann. The first is linked to the migration of the Gestalt psychologists from Austria and Germany to America and their contribution to the emergence, there, after 1945 of a cognitive social psychology. The other relates to the emergence of the behavioural sciences in the 1950s which resulted in the individualisation of the other social sciences and the appearance, in the late 60s and early 70s, of such new multidisciplinary fields of research as cross-cultural psychology, organisational behaviour, political psychology, economic psychology, environmental psychology etc. The history of the behavioural sciences is a product of this era.

'While the roots of social psychology lie in the intellectual soil of the whole Western tradition its present flowering is recognised to be characteristically an American phenomenon.'

(G.W. Allport, 1954, pp.s 3-4)

My point of entry into the historical process is Gordon Allport's classic chapter on the historical background of modern social psychology (Allport, 1954). It marks the point of transition between the long past of social psychology as part of the whole Western intellectual tradition and its short history as an experimental, mainly American, social science. It belongs to the history of ideas approach to the writing of history. Samelson (1974) criticised Allport for creating a false origin myth for social psychology and for presenting a Whig interpretation of its history. By choosing Comte as its founder Allport was reflecting his own belief that social psychology had now entered the positive phase of its development as a modern social science. Farr (1991) is critical of Lindzey and Aronson (1968/69; 1985) for retaining, with only slight modifications, Allport's account in subsequent editions of the Handbook of Social Psychology. This, together with editorial changes elsewhere, reflects, Farr suggests, the influence of positivist philosophies of science in shaping historical accounts of social psychology in the modern era.

THE ROOTS OF SOCIAL PSYCHOLOGY

The whole Western intellectual tradition. The roots, here, are to be found in the human and social sciences (Smith, 1997; Jahoda, 1992) and are essentially European. Prior to Comte, according to Allport (1954), the roots of social psychology are to be found in what, today, would be called political science. These were theories about the nature of human nature in relation to the state. He devoted some space, for example, to an exposition of Hobbes's Leviathan (including a full-page reproduction of the frontispiece). Here, we are in the realm of speculation. It is part of what Comte called the metaphysical phase in the evolution of any discipline.

I prefer to start with the emergence of the Wissenschaft tradition within the German university system which marked the birth of the modern research university (Farr, 1996, chapter 2). It dates from the time of Humboldt's re-establishment of the University of Berlin in 1809. A controversy developed within this tradition between the Geisteswissenschaften (roughly the human and social sciences) and the Naturwisssenschaften (the natural sciences). Here we have a controversy between two rival forms of science. Social psychology, at least in terms of its European roots, formed part of the Geisteswissenschaften e.g. the ten volumes of Wundt's Völkerpsychologie (Wundt, 1900-20). Manicas (1987) traces the transformation of this European tradition of the Geisteswissenschaften after they crossed the Atlantic and took root (or, rather, failed to take root) in American soil.

Collective mental phenomena. Next year is the centenary of Durkheim's concept of collective representation (Durkheim, 1898). By distinguishing between collective and individual representations (with the former being objects of study in sociology) Durkheim effectively separated sociology from psychology, creating, thereby, an identity crisis for social psychologists which they have been unable to resolve in the course of the present century. Social psychology could develop and has developed within the context of either parent discipline. There are now sociological as well as psychological forms of social psychology.

Durkheim was not alone in insisting that collective and individual phenomena should be treated separately. The objects of study in Wundt's Völkerpsychologie were language, religion, customs, myth, magic and cognate phenomena. These, which were comparable to Durkheim's collective representations, could not be explained in terms of the consciousness of the individual which was the basis of his laboratory science. Wundt, like Durkheim, was a strong anti-reductionist. This was why he separated his social from his experimental psychology treating them as two quite distinct projects. Mind in its external manifestations (i.e. collective representations), being the product of the interaction of the many, was different from mind in its internal manifestations as revealed, for example, by introspection. Le Bon (1895) contrasted the rationality of the individual with the irrationality of the masses. In the 1920s Freud (1921, 1923) switched his attention from the clinical study of the individual to a psychoanalytic critique of culture and mass phenomena.

The theorists whose work is summarised in Figure 1, can, now, be identified with different specific disciplines e.g. sociology (Durkheim), psychoanalysis (Freud), psychology (Wundt), linguistics (de Saussure), philosophy (G H Mead), sociobiology (McDougall), mass psychology (Le Bon) and social psychology (F H Allport). It is difficult, at this remove in time, to appreciate that most of them were familiar with each other's work. This is much less likely, today, with the boundaries between disciplines. Manicas (1987) provides a useful set of temporal markers for the separation of the Geisteswissenschaften into distinct disciplines:-

Figure 1: Levels of theorising

LEVEL OF PHENOMENON			
Theorist	***Individual***	***Intermediate***	***Collective***
Wundt	Physiological psychology		Volkerpsychologie
Durkheim	Individual representation		Collective representation
Le Bon	The individual		The crowd
Freud	Clinical studies	Ego, id and superego	Psychoanalytic critique of culture and society
de Saussure		Parole	Langue
Mead	Mind	Self	Society
McDougall	Instincts		Group mind
F H Allport	Behaviour of individual		Institutional behaviour; public opinion

> "...if, as social scientists, we were to imagine ourselves transported to Oxford, the Sorbonne, or Harvard in, say, 1870, we would find almost nothing familiar. There would be no 'departments' of 'sociology' or 'psychology'; the research practices of the faculties and the modes of graduate instruction of those institutions would be for the most part alien. But we would find very little which is not familiar if we were to make a similar visit to any 'department' in any American university in 1925"
>
> (Manicas, 1987, p5)

(iii) **Reductionism in the social sciences**. All of the major theorists identified in Figure 1, with the exception of F H Allport, were anti-reductionists. That is, they believed that the phenomena listed in the final column could not be explained in terms of the phenomena listed in the first column. F H Allport, alone, believed it possible to move from the level of the individual to the level of the collective without changing one's explanatory model. This is because, for him, the individual is the only ultimate reality:-

> 'There is no psychology of groups which is not essentially and entirely a psychology of individuals. Social psychology must not be placed in contradistinction to the psychology of the individual; it is part of the psychology of the individual, ... There is likewise no consciousness except that belonging to individuals. Psychology in all its branches is a science of the individual.
>
> (F H Allport, 1924, p4)

Floyd Allport was a fierce critic of anyone - whether social scientist or journalist - who appeared to assign agency to entities other than individuals. He attacked McDougall's conception of The Group Mind (McDougall, 1920; Farr, 1986). Only individuals have minds. He was critical of Le Bon's conception of crowd consciousness. There is only the consciousness of the individuals comprising the crowd - the crowd itself cannot be conscious because it lacks a central nervous system. He was single-minded in his commitment to the cause of reductionism in the social sciences. This cause prospered when psychology ceased to be the science of mind and became, instead, the science of behaviour.

MODERN SOCIAL PSYCHOLOGY: A CHARACTERISTICALLY AMERICAN PHENOMENON

Social Psychology (F H Allport, 1924). In this, now classic, textbook Floyd Allport (Gordon's brother) established social psychology in America as an experimental and behavioural science. Graumann (1986) is essentially correct when he claims that Allport's chief contribution to social psychology was that he individualised the discipline. This was a direct consequence both of his behaviourism and of his experimentalism.

Allport's own substantive field of research in social psychology was social facilitation effects. He was concerned with assessing the effects on the performance of the individual of the presence of others, either as co-actors or as audience. Graumann (1986) traces the origins of this experimental tradition of research to the studies, in Germany, of Meumann (1914) and of Moede (1914, 1920) in the field of education. Allport had been supervised in his doctoral studies at Harvard by Münsterberg. He derived his social psychology, albeit indirectly, from Wundt's' experimental psychology rather than from his Völkerpsychologie. Indeed Allport could be classified with the younger generation of positivists who repudiated Wundt (Danziger, 1979).

Wundt had believed (see above) that psychology was only in part a branch of the Naturwissenschaften. He believed it was not possible, for example, to study higher mental processes experimentally. They were part of his social psychology which, in turn formed part of the Geisteswissenschaften. The younger generation of experimentalists rejected Wundt's claim that their science was a strictly limited project. They went on to show, at Würtzburg, Berlin and elsewhere, that it was possible to study higher mental processes experimentally. Allport, together with Meumann and Moede, showed that it was possible to study social psychology experimentally. The behaviourists in America, like the younger generation of experimentalists in Germany studied by

Danziger, claimed that psychology was wholly a branch of the natural sciences, thus repudiating Wundt. The emergence of social psychology as an experimental and behavioural social science was, as Gordon Allport (1954) claimed, a characteristically American phenomenon. His brother, Floyd had helped to ensure that this was so.

When Allport wrote his Social Psychology he could claim, quite accurately, that more sociologists than psychologists had written textbooks of social psychology. This was probably the last occasion on which such a claim could have been made in all truth. Jones (1985) cited, by decade, the number of textbooks written by psychologists and by sociologists as evidence for the dominance of psychological over sociological forms of social psychology. By the 1970s and 80s psychologists outstripped sociologists in the writing of such texts by a ratio of about four to one. Allport's text of 1924 was the start of what became the dominant tradition of psychological social psychology in America in the modern era.

(ii) **Graumann's thesis examined**. Graumann (1986) based his thesis concerning the individualisation of the social primarily on a close reading of Allport's 1924 text. He could have considerably strengthened his case if he had read Allport more widely, particularly his 1933 book on Institutional Behaviour where his reductionism is clearly evident. Institutions are analysed in terms of the behaviour of individuals.

Graumann, not surprisingly (given the context in which his chapter appeared), focused on Allport's account of the behaviour of crowds. In the literature of the day, crowds included institutions. McDougall (1920), for example, in The Group Mind was concerned with the morale of such institutions as the army and the church. The experimental studies of Meumann (1914) and of Moede (1914, 1920) referred to above concerned the effect of institutional context on schoolwork. The comparison was between work carried out at school (social facilitation) and at home (homework i.e. the alone condition). Had Graumann included Allport's analyses of institutional behaviour it would have confirmed his belief that behaviourism leads to an individualisation of the social sciences. Graumann fails to mention the powerful endorsement by Allport (1937) of public opinion polling when it was first introduced in America in the 1930s. This was a method of research which was completely consistent with Allport's own methodological individualism. It was also the necessary antidote, in a democracy, to the perceived unanimity of crowds. Individuals are in the 'alone' condition when they respond to the questions of the pollster.

Graumann's claim that the individualisation of the social is equivalent to the desocialisation of the individual is just plain wrong. There is no inherent contradiction, for example, in the social psychology of G.H. Mead (1934) between the processes of individualisation and of socialisation. In

cultures where individualism is an important set of values (as, for example, in the United States of America) children are raised to be individuals. They are highly socialised. Bronfenbrenner (1970) contrasts two worlds of childhood - that of the United States (which is highly individualist) and that of the former USSR (which was highly collectivist). The processes of socialisation were equally strong on both sides of the former Iron Curtain. Children were socialised in two quite contrasting cultures. In the course of socialisation the rival ideologies of the late Cold War i.e. capitalism and communism played a role.

Graumann's account is deficient in another respect. Behaviourism was not the only force at work in the inter-war years in America making for the individualisation of social psychology, though it was certainly the most potent. Floyd's brother, Gordon, who was a cognitive theorist, also played an important role in individualising social psychology. In his classic chapter on attitudes (Allport, 1935) Gordon individualised the key theoretical concept in social psychology. At the time attitude was a common concept to both sociologists and psychologists. It was, and still remains, a highly distinctive concept in psychological social psychology. Thomas, the distinguished Chicago sociologist, defined social psychology in the 1920s as 'the scientific study of social attitudes'. In his review of the concept for the Murchison Handbook Gordon Allport considered a wide range of definitions proposed both by sociologists and psychologists. As Jaspars and Fraser (1984) have amply demonstrated, by selectively editing out the collective and social aspects of the various definitions, Allport individualised the concept. He did the same thing, as Craik (1993) has demonstrated, for personality (G W Allport, 1937). The point I wish to establish, here, is that behaviourism was not the only device making for the individualisation of social psychology during the inter-war years. Graumann's thesis, while correct, is incomplete.

(iii) **The perspective of the Gestalt psychologists**. There were two further waves in the individualisation of the social beyond the one identified by Graumann. The first is associated with the migration of the Gestalt psychologists from Austria and Germany to America (Farr, 1996 pps 110-117). The second, which concerns the emergence of the behavioural sciences in America in the 1950s, is dealt with in the final section of this paper. Koffka emigrated to America in 1927 and Heider in 1930. Wertheimer and Lewin fled in 1933 with Hitler's rise to power in Germany and others followed later. The definitive study of the Gestalt perspective, in the context of German culture, is Ash (1995). My concern, here, is with what happened after the emigration of the Gestaltists to America where, for the first time, they encountered behaviourism as the dominant paradigm for research in psychology. I am interested, primarily, in Lewin and Heider because they directly influenced the development of social psychology in America.. Wertheimer is important too, especially for his influence on the work of Solomon Asch.

Although they emigrated at different times and for various reasons the Gestalt psychologists found themselves more or less united, in an American context, in their opposition to behaviourism. The outbreak of World War 1 in 1914 saw the establishment of behaviourism in America (Watson, 1913) and of Gestalt psychology in Germany. The war and its aftermath helped to ensure that these two quite distinct forms of psychology developed independently of each other on opposite sides of the Atlantic. The occasion of their meeting was the imminent threat, once again, of war, in Europe. While the migrations occurred before the outbreak of war, the impact of the Gestalt perspective did not become apparent until the modern era in social psychology, following the end of the war. The emergence of a cognitive social psychology in America in the post-war era was a direct consequence of those earlier migrations. Whilst Gordon Allport (1954) correctly described modern social psychology as a characteristically American phenomenon the input from continental Europe was vital.

(iv) **The co-existence of two incompatible perspectives**. The perspective of the behaviourist is that of an observer of others. The perspective of the Gestalt psychologist is that of an actor in the social scene. This corresponds, respectively, to the 'consistency of response' and 'view of the world' approaches to the study of attitudes (Campbell, 1963). Campbell shows how, historically, the 'view of the world' approach came to prevail over the 'consistency of response' approach. This corresponds to the dominance of the Gestalt perspective over the behaviourist perspective and is associated with the emergence in America of cognitive social psychology.

The Gestalt perspective individualised the social just as effectively as behaviourism had already done. The individualisation, this time, was perceptual, rather than behavioural. In the context of behaviourism the 'view of the world' approach to the study of attitudes will appear to be subjective. The only way of eliciting the perspective of others is to invite them to tell you how they see the world. This involves the use of self-report methods in the assessment of attitudes and the gauging of opinions. The perspective of the actor is just as individualised as the perspective of the observer. According to Jones and Nisbett (1972) these two perspectives are incompatible with each other. The co-existence of two highly individualised, but incompatible, perspectives through the modern era of social psychology does not constitute a social science.

Asch was only twelve years of age when his parents emigrated from Poland to America. He learned about Gestalt psychology in America, mainly from Wertheimer, who was then at the New School for Social Research in New York. In many respects he played an important role in the Americanisation of Gestalt psychology. His textbook Social Psychology (Asch, 1952), played a pivotal role in the emergence of a cognitive social psychology in North America in the modern era. It is comparable in stature to the Floyd Allport 1924 text of the same title. Like its predecessor it, too, resulted in the individualisation of social psychology.

THE INDIVIDUALISATION OF THE SOCIAL SCIENCES

(i) **The demise of comparative psychology**. Behaviourism individualised comparative as well as social psychology. By focusing on the behaviour of individuals one individualises the biological as well as the social sciences. Wundt, in his Völkerpsychologie had used the comparative method, as Darwin had done before him. The complement to the experimental control of 'variables' within a laboratory is to study the various species existing in nature. Wundt was trying to do for the human mind what Darwin had already done for the human body i.e. set it in an evolutionary perspective. Here one is limited by the experiments of nature herself. Wundt had to content himself with the accounts of anthropologists and of linguists concerning the varieties of human nature to be found around the world and of languages spoken by humans. This comparative approach to an understanding of the nature of human nature was the organising principle behind Murchison's Handbook of Social Psychology (Murchison, 1935). This was a multi-disciplinary approach to the study of social psychology. It was not possible to bring these natural variations under experimental control. Behaviourism destroyed comparative as well as social psychology. The editors of the modern series of Handbooks of Social Psychology Lindzey (1954) and Lindzey and Aronson (1968/69; 1985) measure progress in the discipline by the distance travelled since the first Handbook of Social Psychology,which now belongs to the pre-modern age in social psychology (Farr, 1991).

The behavioural sciences. In the 1950s it became convenient for the human and social sciences (what, in the context of German culture, would be the Geisteswissenschaften) to refer to themselves as the behavioural sciences. This was because politicians and the corporate Foundations who controlled funds for research were thought likely to confuse social science with socialism. We are, here, at the beginning of the late Cold War. The behaviourism which had already individualised social psychology now had the same effect on the other social sciences. At the heart of this newly designated group of sciences was psychology (i.e. the science of behaviour) and not social psychology. This greatly accelerated the process of the individualisation of the social. This was a second wave in the process, well beyond the one identified by Graumann (1986) and linked, by him, to Allport's text of 1924. The effects are much more pervasive. It also destroyed the possibility that psychologists could re-socialise their discipline by turning to the other social sciences in the American scene. Instead, they would need to turn to the Geisteswissenschaften. What Graumann called the individualisation of social psychology is a special case of what Manicas (1987) called "the Americanisation of the social sciences". The behavioural sciences are the end result of that process.

(iii) **Multi-disciplinary social psychology**. Social psychology is now becoming, once again, a multi-disciplinary enterprise (as in the bad old days of the Murchison Handbook). This time the

other disciplines have become sanitised by virtue of being behavioural sciences. Starting in the late 60s and continuing right up to the present we have the emergence of new fields of inter-disciplinary research such as cross-cultural psychology; organisational behaviour (shades of institutional behaviour!), behavioural medicine; environmental psychology; political psychology; economic psychology - to name but a few. This is all very different from the crossroads between culture and mind (Jahoda, 1992) which, in the past, produced such classics of social science as collective representations (Durkheim, 1898), Völkerpsychologie (Wundt, 1900-20) and, even, the first Handbook of Social Psychology (Murchison, 1935). We have come full circle back to pre-Comtean political science, to which Gordon Allport made reference (see above) except that this time it is behavioural science rather than a human and social science. We also now have a choice as to whether we should publish our historical research in The Journal of the History of the Behavioural Sciences (established in America in 1965) or in The History of the Human Sciences (established in Europe in 1988).

REFERENCES

Allport, F.H. (1924). Social Psychology. Boston: Houghton-Mifflin.

Allport, F.H. (1933). Institutional Behaviour. Chapel Hill: University of North Carolina Press.

Allport, F.H. (1937). Towards a science of public opinion. Public Opinion Quarterly, 1, 7-23.

Allport, G.W. (1935). Attitudes. In C. Murchison (Ed.), Handbook of Social Psychology. (pp. 798-844). Worcester, Mass: Clark University Press.

Allport, G.W. (1937). Personality: A psychological interpretation. New York: Holt.

Allport, G.W. (1954). The historical background of modern social psychology. In G. Lindzey (Ed.). Handbook of Social Psychology. (pp. 3-56). Reading, Mass: Addision-Wesley.

Asch, S.E. (1952). Social Psychology. Englewood Cliffs, NJ: Prentice-Hall.

Ash, M. (1995). Gestalt Psychology in German Culture 1890-1967: Holism and the quest for objectivity. Cambridge: Cambridge University Press.

Bronfenbrenner, U. (1970). Two Worlds of Childhood: US and USSR. New York: Russell Sage Foundation.

Campbell, D.T. (1963). Social attitudes and other acquired behavioural dispositions. In S. Koch (Ed.), Psychology: A study of a science. (pp. 94-172). New York: McGraw-Hill.

Craik, K.H. (1993). The 1937 Allport and Stagner texts in personality psychology. In K.H. Craik, R. Hogan & R.N. Wolfe (Eds.), Fifty Years of Personality Psychology. (pp. 3-20). New York: Plenum.

Danziger, K. (1979). The positivist repudiation of Wundt. Journal of the History of the Behavioural Sciences, 15, 205-230.

Durkheim, E. (1898). Représentations individuelles et représentations collectives. Revue de Metaphysique et de Morale, VI, 273-302.

Farr, R.M. (1986). The social psychology of William McDougall. In C. Graumann & S. Moscovici (Eds.), Changing Conceptions of Crowd Mind and Behaviour. (pp. 83-95). New York: Springer-Verlag.

Farr, R.M. (1991). The long past and the short history of social psychology. European Journal of Social Psychology, 21(5), 371-380.

Farr, R.M. (1996). The Roots of Modern Social Psychology (1872-1954). Oxford: Blackwell.

Freud, S. (1921). Massenpsychologie und Ich-Analyse. Vienna: Internationaler Psychoanalytischer Verlag.

Freud, S. (1923). The Ego and the Id. (Standard ed.). (Vol. 19). London: Hogarth Press.

Graumann, C.F. (1986). The individualisation of the social and the desocialisation of the individual: Floyd H. Allport's contribution to social psychology. In C.F. Graumann & S. Moscovici (Eds.), Changing Conceptions of Crowd Mind and Behaviour. (pp. 97-116). New York: Springer-Verlag.

Jahoda, G. (1992). Crossroads between Culture and Mind: Continuities and change in theories of human nature. London: Harvester/Wheatsheaf.

Jaspars, J.M.F., & Fraser, C. (1984). Attitudes and social representations. In R.M. Farr & S. Moscovici (Eds.). Social Representations. (pp. 101-123). Cambridge: Cambridge University Press.

Jones, E.E., & Nisbett, R.E. (1972). The actor and the observer: Divergent perceptions of the causes of behaviour. In E.E. Jones et al. (Eds.), Attribution: Perceiving the causes of behaviour. (pp. 79-94). Morristown, N.J: General Learning Press.

Jones, E.E. (1985). Major developments in social psychology during the past five decades. In G. Lindzey & E. Aronson (Eds.), Handbook of Social Psychology. (pp. 47-107). New York: Random House.

Le Bon, G. (1895). La Psychologie des Foules. Paris: Alcan.

Lindzey, G. (Ed.). (1954). Handbook of Social Psychology. (2 Vols.). Reading, Mass: Addison-Wesley.

Lindzey, G. & Aronson, E. (Eds.). (1968/69). The Handbook of Social Psychology (2nd ed.). (5 Vols.). Reading, Mass: Addison-Wesley.

Lindzey, G. & Aronson, E. (Eds.). (1985). The Handbook of Social Psychology (3rd ed.). (2 Vols.). New York: Random House.

Manicas, P.T. (1987). A History and Philosophy of the Social Sciences. Oxford: Blackwell.

McDougall, W. (1920). The Group Mind: A sketch of the principles of collective psychology with some attempt to apply them to the interpretation of national life and character. Cambridge: Cambridge University Press.

Mead, G.H. (1934). Mind, Self and Society: From the standpoint of a social behaviourist. Chicago: University of Chicago Press.

Meumann, E. (1914). Haus- und Schularbeit. Leipzig: Klinkhardt.

Moede, W. (1914). Der Wetteifer, seine Struktur und sein Ausmass. Zeitschrift für Padagogische Psychologie, 15, 353-368.

Moede, W. (1920). Experimentelle Massenpsychologie. Leipzig: Hirzel.

Murchison, C.A. (Ed.). (1935). Handbook of Social Psychology. Worcester, Mass: Clark University Press.

O'Donnell, J.M. (1985). The Origins of Behaviourism: American psychology 1870-1920. New York: Columbia University Press.

Samelson, F. (1974). History, origin myth and ideology? 'Discovery of social psychology'. Journal for the Theory of Social Behaviour, 4(2), 217-231.

Smith, R. (1997). The History of the Human Sciences. London: Fontana.

Watson, J.B. (1913). Psychology as a behaviourist views it. Psychological Review, 20, 158-177.

Wundt, W. (1900-1920). Völkerpsychologie: Eine Untersuchung der Entwichlungsgesetze von Sprache, Mythus und Sitte (10 vols). Leipzig: Englemann.

Gordana Jovanovic
University of Belgrade
Yugoslavia

PSYCHOLOGY OF SOCIAL CONFLICTS

My starting point in this paper are the profound social changes we are witnessing in the last decade of our century. According to my understanding of psychology as a socially embedded science, I raise the question whether,to what extent and in what ways psychology reflects these changes.

I next try to analyse psychological conceptualizations of social conflicts as one of the main features of the actual social situation. In my opinion, dominant psychological models of social conflicts (Deutsch,M., Rubin, J.) are derived from a focus on psychological states of the participants in groups already designed according to such psychologized and decontextualized models.

Instead of substituting psychology for politics I argue for a socio-historically as well as politically reflected psychology of social conflicts. After discussing the shortcomings of the translation of social conflicts into psychological terms (cf. individualistic orientation as a recommended attitude) and the problem of false attribution of responsibility, I discover some remnants of neo-colonial gesture in programs for calming "wild souls" by means of psycho-fundamentalism.

As a consequence of this critique - for which I rely on critiques of individuocentrism elaborated in other fields of psychology, e.g. developmental psychology - I argue for the reconstruction of historical psychology which includes in its subject-matter transformations and conceptualizations of actual socio-historical patterns.

Gordana Jovanović
Universität Belgrad
Jugoslawien

ZUR PSYCHOLOGIE DER SOZIALEN KONFLIKTE

Daß wir in einer Welt der einschneidenden Veränderungen leben, kann wohl nicht bestritten werden. Nun möchte ich auf diese spontanen Erfahrungen von einer andersartig gestaltenen psychologischen Seite eingehen.

Für diese Zwecke läßt sich mein Verständnis der Psychologie folgendermaßen bestimmen: Psychologie ist Sozialwissenschaft im doppelten Sinne. Zunächst ist sie in einem sozialen Zusammenhang verankert: ihrer Herkunft nach wie auch ihrem Gegenstand nach enstammt sie diesem Zusammenhang, dessen Möglichkeiten, Bedürfnissen oder der 'zone of proximal development'. Dieser sozialen Verankerung der Psychologie – die, wie wir wissen, von der main-stream Psychologie noch nicht angeeignet ist – wird noch eine soziale Bedeutung hinzugefügt: Psychologie als Mitgestalterin der Lebensformen – Denkfiguren, Gefühlsschemata, Verhaltensmuster.

Dieses Verständnis der Psychologie verpflichtet die folgende Frage zu stellen: wie steht es mit der Psychologie angesichts der einschneidenden Veränderungen in unserer Welt. Darin sind zwei weitere Fragen zu erkennen. Erstens: wie haben sich die Umwälzungen des letzten Jahrzehnts unseres Jahrhunderts auf die Psychologie ausgewirkt – theoretisch als auch unmittelbar sozial-praktisch? Zweitens: wie hat sich die Psychologie daran beteiligt, welche Rolle wurde ihr zugeschrieben.

Die Veränderungen, nach deren Auswirkungen in der Psychologie hier gesucht wird, verlaufen in einem globalen Kontext, aber haben auch ganz individuelle lebensgeschichtliche Konsequenzen. Worum es geht, ist das Erbe von mindestens zwei Jahrhunderten.

Das vorige, XIX Jahrhundert wurde - über chronologische Grenzen hinaus – symbolisch verlängert, wie uns darauf der Historiker Eric Hobsbawm aufmerksam gemacht hat. Diese Verlängerung drückt, unter anderem, auch ein psychisches

Bedürfnis der Jahrhundertwende aus, die packende Endzeitstimmung, die auch durch symbolische Grenze mitbestimmt wurde, loszuwerden – oder mindestens zu verschieben. Und doch verschwand das Alte anders als man gedacht hat – sogar anders als man gefürchtet hat.

Inzwischen – wieder mit dem Hobsbawm sprechend – haben wir uns auch von dem zwanzigsten Jahrhundert schon verabschiedet. Das XX Jahrhundert wurde zweifach gekürzt: einmal an seinem Anfang, der fast zwei Jahrzehnte lang vom vorigen Jahrhundert besetzt wurde (worunter ich auch psychische Besetzung meine). Die zweite Kürzung ist das Verdienst des XX Jahrhunderts selbst: der Zusammenbruch der sozialistischen Länder markiert das Ende dieses `kurzen XX Jahrhunderts`. Dieses Ende wurde von einer triumphalistischen Stimmung begleitet. Man wurde nicht nur den ideologischen Feind und seiner Welt los, sondern das ganze Jahrhundert, das der Besiegte doch mitgeprägt hat, sollte vorzeitig verabschiedet werden.

Auf der Weltbühne ist der einsame Triumphierende geblieben – nicht nur ohne Gegner sondern auch ohne Partner. Wenn es nur eine Seite da ist, sollte es keine Möglichkeit zur Konfliktauslösung geben. Man könnte sogar in Versuchung geraten anzunehmen, es bevorsteht der Welt eine Zeit der Konfliktlosigkeit.

Aber nachdem der Triumphalismus ein bißchen nachgelassen hat, sind die Probleme zu erkennen, die der Triumphalismus verkannte. Das formal-logische Spiel – ohne zwei konkurrierende Seiten kein Konflikt – kann die Einsicht in eine ganze Menge von Konflikten gerade in der Zeit nach dem vollendeten kurzen, aber dennoch grausamen, wenn nicht auch grausamsten XX Jahrhundert, nicht versperren. Posthistorie ist doch kein Abschied vom guten historischen Erbe.

Mein Anliegen hier ist es, über den Status der Konflikte in der gegenwärtigen Psychologie nachzudenken. Die Analyse ist als ein Beitrag zur Soziogenese der gegenwärtigen Psychologie der sozialen Konflikte gemeint.

Die auf den gegenwärtigen Zusammenhang fokusierte Frage verstehe ich also als eine Fortsetzung der historischen Rekonstruktion der Entstehung und der Entwicklung der Psychologie (s. Jaeger, S. & Staeuble, I.: *Die gesellschaftliche Genese der Psychologie,* 1978) . Das bedeutet, daß ich nach möglichst neuen Begriffen oder deren Inhalte, anderen Theoriebildungen und deren Anwendungen suche. Diese Suche wird von den am Anfang gestellten Fragen geleitet: hat die Psychologie die schwerwiegenden Veränderungen im sozialen Umfeld überhaupt zur

Kenntnis genommen? Auf welche Art und Weise werden diese Umwälzungen psychologisch repräsentiert? Oder, in Piaget´s Worten ausgedrückt: wie werden sie assimiliert? Und weiter mit Piaget sprechend: wie hat sich Psychologie den neuen Objekten der Erkenntnis akkomodiert? Diese epistemologischen Fragen gehen in sozialkritische über, denn es geht hier nicht um bloße Erkenntnis, sondern um Lebensumgestaltung.

Was ist dabei in der Psychologie vor sich gegangen? Zunächst – Pluralismus von Psychologien, die oft nichts miteinander zu tun haben, die sogar füreinander unverständlich bleiben. Innerhalb dieses Pluralismus sind auch neue Ansätze zu finden – historische Psychologie, cultural psychology, critical psychology. Meiner Meinung nach sind solche Ansätze nicht nur ein paar neue unter vielen anderen. In gewissem Sinne stehen sie auch für andere. d.h. sie schließen die anderen als Gegenstand eigener Reflexion ein. Vielleicht wäre es hier angebracht, zu alten Bezeichnungen zu greifen – ich würde diese Ansätze synthetische Ansätze nennen. Sie bezeugen, daß in der Psychologie eine theoretische Bereitschaft und Fähigkeit vorhanden ist, das Terrain des psychologischen Textes zu verlassen und sich nach dessen sozialen Referenten umzusehen. Das ist ein Anknüpfungspunkt, an dem ich die Frage nach der Psychologie im Zeitalter der Posthistorie stellen möchte.

Der Begriff der Posthistorie sollte auf Erschöpfung bisheriger historischer Paradigmata hinweisen. Politische, soziale, psychologische Strukturen können nicht mehr den gefordeten gut funktionierenden Zusammenhang gesellschaftlicher Handlungen gewährleisten. Ökonomische Prozesse hinterlassen Folgen, die mit bestehenden politischen Strukturen nicht mehr zu fassen sind und psychologisch nicht mehr zu bewältigen sind. Das Politische wird, andererseits, entpolitiziert – entweder ins Psychologische übersetzt oder ästhetisch inszeniert oder als Politikverdrossenheit empfunden (s. Thomas Meyer: *Die Transformation des Politischen,* 1994). Risikogesellschaft wird unsere tägliche Erfahrung (s. Ulrich Beck: *Risikogesellschaft,* 1986).

Was bietet die Psychologie angesichts der posthistorisch veränderten Welt an?

Einmal, nach dem zweiten Weltkrieg, liest man in psychologischen Büchern, hat die Psychologie Konflikt zum Thema ihrer wissenschaftlicher Untersuchung gemacht. Das könnte als eine nachträgliche psychologische Antwort auf tragische Konflikterfahrungen verstanden werden. Inzwischen wurden psychologische Modelle des Konflikts und friedlicher Konfliktlösung ausgearbeitet. Es wurden psychologische

Faktoren genannt (etwa Vorurteile, Feinbilder, Egozentrismus), die Konflikte beinflussen und die abzubauen sind (meistens in sogenannten workshops).

Für unsere konfliktreiche Gegenwart hatte die Psychologie ihre Modelle schon bereit. Sie sollten nun überall verbreitet werden und den betroffenen zugänglich gemacht werden – das gehört auch zur humanitären Hilfe.

Aber verbleiben wir noch eine Weile bei den Modellen selbst. Ein auffäliges Merkmal der bekanntesten psychologischer Theorien der Konflikte (Deutsch, M., 1991; Rubin J., 1991, 1994) ist eine massive Psychologisierung, d.h. Isolierung vom realen sozialen Zusammenhang, der unangetastet bleibt (wahrscheinlich auch bleiben soll). Es geht ja darum, das Bestehende mit anderen Augen zu sehen. Das Auge, der Blick, nicht das Leben , ist der Gegenstand der Veränderung. Dem muß die Annahme zugrunde liegen, daß das Auge auch der Ursprung der Konflikte sei.

Nicht, daß ich die Bedeutung des Auges und des Subjektiven überhaupt vernachlässige. Aber es soll auch nicht vergessen werden, daß das Soziale für das Subjektive konstitutiv ist. Gerade das aber wird in diesen Theorien ausgeblendet, verdrängt. Die Zielsetzung ist dabei, die Herausbildung der sozial kompetenteren Individuen zu fördern. Die kognitive Matrix der modellierten Individuen enthält aber sehr viele Lücken, wenn man sie z.B. mit Harré's Beschreibung derselben vergleicht. „The cognitive matrix of a socially competent individual would be made up of knowledge of situation, knowledge of persona, knowledge of conventions of propriety in situation, knowledge of set of rules by which the conventions operative in it could be expressed." (Harré, R., 1976: 211)

Wenn die Psychologie trotz so auffäliger Lücken ihre Autorität wirkungsvoll ausübt, dann stellt sich die Frage nach der Legitimität. Nikolas Rose hebt hervor: „In a liberal society authority is only effective and legitimate to the extent that it is exercised in the light of a knowledge of those who are governed." (Rose, N., 1991: 94).

In den letzten tragischen Jahren, die eine Unmenge von Konflikten hervorgebracht haben, deren Nachwuchs auch sehr konfliktproduktiv ist, werden viele Programme zur Konfliktlösung in Konfliktgebiete eingeführt: es geht um non-violent conflict-resolution, um Symbole friedlicher Sprache. Dabei soll die Möglichkeit der gewaltlosen Konfliktlösung vorgeführt werden und die Folgen der gewaltsamen Konfikte möglichst geheilt werden. Psychologie als Friedensstifterin. Lobenswert - oder?

Morton Deutsch hat sich in diesem Bereich besondere Verdienste gemacht. Er befürwortet eine expressive Auffassung von Konflikten, die ihn zu solchen selbst-reflexiven Aussagen verleitet: das Hauptproblem bezüglich Konflikte – sowohl in sozialer als auch in wissenschaftlicher Hinsicht – sei nicht, wie man sie loswird oder vorbeugt, sondern wie man zu Kenntnissen kommt, die uns helfen könnten, zu begreifen unter welchen Bedingungen man statt eines tödlichen Streits eine lebhafte Diskussion entwickeln kann. Mit Hilfe vom „einfachen Gesetz der sozialen Beziehungen" (Kooperation im kleinem führt ohne weiteres zur Kooperation auf der globalen Ebene) ist man schnell zur sozialen Totalität gelangt. Diskussionen über Rechte und Prinzipien sind eigentlich fruchtlos – so Morton Deutsch.

In solchen Auffassungen erkenne ich dieselbe epistemologische Zentrierung wieder , die z.B. von Entwicklungspsychologie vollzogen wird, wenn sie als ihren Gegenstand das Kind bestimmt, und den Kontext der sozialen und politischen Situation fast völlig außer acht läßt. In demselben Maße wie dieses epistemologische Muster auch unter den einflußreichsten Konfliktpsychologen verbreitet ist, ist auch die Kritik, die Erica Burman vorbildhaft auf die Entwicklungspsychologie gerichtet hat, zu verallgemeinern. „Further, the model of ´man` prescribed in Kohlberg´s (and by implication Piaget´s) model derives from particular social interests, based on a liberal model of society seen as functioning by means of social contractual arrangements between people (Simpson, 1974; Sampson, 1989). The rationality which is so highly valued in the cognitive developmental model ties in with a bourgeios conception of the individual which either accepts class divisions or denies their existence (Sullivan, 1977; Buck-Morrs, 1975). In its celebration of autonomy, Kohlbergian theory therefore partakes of a liberal view that sees society as composed of independent units who co-operate only when the terms of cooperation are such as to further the ends of each of the parties. This also clearly recalls Piaget´s definition of social interaction in game playing through competition. Not only does this lead to an asocial view of the individual, in terms of the ascription of pre-social interests, it also sets up a form of conceptual imperialism in its application to cultures which do not share this underlying model. Sullivan treats this model as a case example of the political and conceptual problems wrought by an inadequate theory of the social: thought is severed from action, form from content, the abstract from the concrete and, ultimately, emotion from intellect." (Burman, E., 1994: 183)

Ich glaube nicht, daß die Psychologie sich nur als Psychologismus behaupten kann und soll. Ganz im Gegenteil – Psychologismus ist, meiner Meinung nach, eben eine Subversion gegen die Psychologie, gegen ihre Möglichkeit, das Subjektive lebensgeschichtlich zu begründen und zu legitimieren. Deswegen ist Psychologismus eine Verfälschung des Subjektiven als einer sehr wichtigen und mehr noch - unverzichtbaren Lebensform.

Bei der Veralltäglichung des Subjektiven ist seine Anknüpfung an das Soziale nicht so selbstevident. Das soll aber keinesfalls die theoretische Ausblendung dieses Zusammenhangs rechtfertigen.

Angesichts der Veränderungen, die einem Welt-Erdbeben glichen, ist es unzumutbar, das Blickfeld nur auf den Blick selbst zu begrenzen und vorzutäuschen, es ginge nur um einen häßlichen Schein, der jetzt – nach dem Modell der non-violent conflict resolution – anders, ja friedlich scheinen kann und soll.

Diese Art der Täuschung ist der Psychologie nicht fremd. Der Versuchung ist auch nicht leicht zu widerstehen. Aber das Prinzip Verantwortung muß auch für die Psychologie gelten und der Inbegriff der Verantwortung ist eben die zu verantwortende Zuschreibung der Verantwortung, im Sinne, wer wofür verantwortlich ist.

Die hier zur Debatte gestellten Modelle der sozialen Konflikte gehen von einer falschen Voraussetzung über die Verantwortungsträger aus. Durch die Fokusierung auf die Erlebnisse in interpersonalen Beziehungen werden andere mitwirkende , oft sogar entscheidende Strukturen ausgeblendet. Im nächsten Schritt wird dann die Verantwortung unter denen, die zugänglich sind, verteilt. Meistens sind das aber eben diejenigen, die weniger handeln konnten, die vielmehr unter den Entscheidungen von anderen leiden mußten. Wenn dann das Leiden als Folge nur oder hauptsächlich nur des «gewählten » Blickwinkels gesehen und gedeutet wird, dann sind die Opfer schon in Täter umgewandelt – wobei die richtige Täter unberührt bleiben, unberührt selbst von der Erkenntnis derjenigen, die die Folgen ihres Tuns hautnah erlebt haben. Alle Einsicht in Psychodynamik des Unbewußten kann die Rollen der Täter und «Getanen » doch nicht als im voraus ohne weiteres austauschbar anzeigen. Dieser Versuchung ist auch zu widerstehen.

Was ich als eine Alternative zu psychologisierten Modellen der sozialen Konflikte und ihrer genauso psychologisierten Lösungsversuchen befürworten würde, wären solche Modelle, deren unverzichtbare Voraussetzung die soziale Verankerung

der sozialen Konflikte wäre. Erst unter dieser Voraussetzung wäre eine weitere Analyse der Psychodynamik sowohl theoretisch begründbar als auch moralisch legitim. Die Verkürzung um diese Verankerung, die die herrschenden psychologischen Modelle der sozialen Konflikte kennzeichnet, erweist sich deshalb als theoretisch ungenügend und moralisch suspekt.

Wenn Ralf Dahrendorf in seiner Analyse «des modernen sozialen Konflikts » über die Möglichkeit spricht, Konflikte in individuelle Mobilität zu übersetzen (Dahrendorf, 1992 :37), dann kann man das als einen Hinweis auf die soziale Bedeutung der « Übersetzungsarbeit » verstehen. Die Übersetzungsarbeit wird oft psychologisch vollendet – soziale Konflikte werden in Psycho-Konflikte übersetzt, wobei die « Originalsprache » vergessen wird – weiter gilt nur die Übersetzung. Die Warnung vor « the substitution of psychology for politics » (Lash, 1972 :46), obwohl schon längst ausgesprochen, hat nichts an seiner Bedeutung verloren – ganz im Gegenteil.

Ich sehe darin eine Geste des transformierten Neo-Kolonialismus, der in seiner gegenwärtigen Psycho-Welle die Seelen der « Wilden » - denen großzügig doch die Veränderlichkeit – in Form von Machbarkeit – zugeschriben werden mußte – friedlich umgestalten möchte – diesmal mit Hilfe der eigenartigen Religion des Psycho-Fundamentalismus.

Literaturverzeichnis :

- Beck, U. (1986) : *Risikogesellschaft,* Frankfurt : Suhrkamp.

- Burman, E. (1994) : *Deconstructing developmental psychology* , London, New York : Routledge.

- Dahrendorf, R. (1992) : *Der moderne soziale Konflikt,* Stuttgart : Deu tsche Verlags-Anstalt.

- Deutsch, M. (1991) : Subjective features of conflict resolution : Psychological, social and cultural influences. In : Raimo Vayrynen (ed.) : *New directions in conflict theory,* London : Sage.

- Harré, R. (ed.)(1976) : *Personality*, Oxford : Basil Blackwell.

- Jaeger, S. & Staeuble, I. (1978) : *Die gesellschaftliche Genese der Psychologie*, Frankfurt, New York: Campus Verlag.

- Lash, Ch. (1972) : *Toward a theory of post-industrial society*, Hancock.

- Meyer, Th.(1994) : *Die Transformation des Politischen*, Frankfurt : Suhrkamp.

- Mitchell, C. R. (1994) : *International conflict*, London : MacMillan Press.

- Rose, N. (1991) : Experts of the Soul, *Psychologie und Geschichte,* Jahrgang 3/1-2.

- Rubin, J. (1991) : Some wise and mistaken assumptions about conflict and negotiation, in : Breslin, J.W. & Rubin J. (eds.) : *Negotiation theory and practice,* Cambridge.

- Staeuble, I. (1995) : Historische Psychologie und Cultural Psychology, 5. Fachtagung « Geschichte der Psychologie », Passau.

AFTER HALBWACHS: HOW TO CAPTURE SOCIAL FACTORS IN MEMORY RESEARCH

Ildikó Király
ELTE Department of General Psychology

Halbwachs notions on collective memory

Maurice Halbwachs in the prologue of his book *The social frames of memory (Les cadres sociaux de la mémoire, 1925)* has drawn up that we mostly remember meeting other people, in the case we are supposed to answer questions. His important concept can be assumed in the following way: without talking about social frames one cannot speak about remembering.

Halbwachs, being a follower of Durkheim, was a representative of radical sociological reductionism. Apart from this, in this essay I will give an outline of those elements that made Halbwachs' notions of memory become relevant again. I try to show how recent memory investigators 'rediscover' the significance of social factors in connection with memory and remembering.

Remembering, in Halbwachs' interpretation, is nothing else but the sheer reconstruction of our past. Each event of our past is interpreted through the filter of a socialized animal. We do not have raw memories; Halbwachs denies the existence of personal memories. Every memory is constructed according to social contents; the norms created by the community (group of individuals) directs and determines the construction of memories. He mentions that the individual remembers of the group's point of view and the group's collective memory is realized through the individuals' memories.

I hope, by the end of this essay it will be clear, that remembering socially, as Halbwachs captures, means not only the frame but the content of memories which are socially influenced (Pléh, 1992).

I will quote Halbwachs' nice example on the case of re-reading children's books, by which we can get closer to his understanding of social determinants in the recontruction process. When we take a book of our childhood into our hands we always experience great surprise. We are not faced by our original experiences, we are searching for and cannot find the things -e.g. lines, characters- we remember from childhood. The reason for this phenomenon is that we learned a lot since then, our conceptual frames have been changed.

The filter of reconstruction is the immediate conceptual frame we have at the time of reading. We cannot regress to the conceptual frames of our childhood. We have to face the continous studying and the permanent change of the conceptual frames.

To illustrate it with Halbwachs' metaphore: one's knowledge of oneself is similar to those buildings which are rebuilt on their original bases, from their own stones - their identities are still the same, they are preserving their 'ancient' elements, but in a new form.

The mediator of the reconstruction, thus the cue of his theory, is the usage of languages and other similar conventions. He reckons that these conventions are the embodiements of rationality. These conventions help us to organize and interpret our experiences. In his opinion we have only these organized and interpreted experiences, because we can express everything with words and we share the meaning of these words (the content is a cultural, public phenomenon).

Accordingly, telling a memory is the same as retrieval. Conventions make it possible for us to reconstruct our past. Moreover, when we express something with words, through the common meaning of them, the actual social context forms the content of our memory, specifying how and what we remember.

The means of Halbwachs' renaissance

Halbwachs' radical conception had been neglected in the area of memory research for a long period. He was identified as a thinker of sociological reductionism, but as Csaba Pléh (1996) argues, his notions are closer to handling immediate social context, partners, rather than societal factors. I would like to stress that his focus on language strengthens and also means the importance of a community and not an organized society.

A recently proposed approach in memory research focuses on the problem of everyday memory/everyday remembering. The reflections on the limits of laboratory memory research showed a new way of looking at what we call the 'ecological' investigation of human memory. (Hirst & Manier, 1995) This trend tries to answer questions which are not met by 'system-descriptions'. In the wider spectrum of the ecological approach the process of remembering and the person who remembers are both important and essential.

This momentum directs our attention to social factors: we cannot disregard the assumption of the presence and the effects of the social factors when investigating everyday remembering. On the other hand, it is this momentum that leads forward the renaissance of Halbwachs' notion of collective memory.

Directions of handling social factors in recent memory research

I would like to introduce the three outstanding trends in the domain of ecological investigation of memory that I think follow the treatment of social factors in a Halbwachsian way.

The closest to Halbwachs' original theory are Barclay & Smith's concept of 'personal culture' (1992) and Fitzgerald's self-narratives concept (1992, 1994). The 'personal culture' is a semantic grid: the way we remember to personally relevant events is based on a special experience, rooted in the

cultural conventions of interpretaion. The formation of the self depends on the social context that is culture.

Self-narrative is also a mode of interpretation. Our consistent life-history (self-narrative) conforms to the law by which we are constrained to outline coherent, continous narrative.

Both Barclay & Smith and Fitzgerald point out the determinant role of social conventions and common conceptions with respect to life-histories.

Rubin & Kozin (1984) stress the role of language in the survival of our experiences in memory. The communication of a memory can cause the survival; and at the same time it defines how it survives and what will remain.

2) The next collection of researchers start their investigations in the field of language socialization. They seem to think that the socialization of remembering is rooted in the socialization of language, we try to introduce to children conventions which are the 'tools' of remembering. These mediate the sharing of personal experiences, the maintenance of relationships through time, and the understanding of social connections (see Bruner & Lucairello, 1989; Neisser, 1988).

Nelson (1989) found in little Emily's monologues, (she had analysed the three years old child's spontaneous talk), that she followed her parents' mode of story-telling generating memories.

Fivush, Haden & Reese (1994) argue that mothers teach their child the ability of reconstructing the past through the conventions of narratives. In the opinion of the authors this ability means the frame by which the child can organize his or her individual experiences and is able to share those.

This former trend does not deny the existence of personal experiences, personal memories, but claims that the frames of remembering are socially determined.

3) The third trend I would like to mention is the so called collective remembering line.

For this trend of memory research - drafting strictly - remembering appeares only in the presence of companions, the nature of remembering is discoursive. The main argument of this approach lies on the interaction - they

tend to avoid speaking about remembering outside interactive context. (Edwards & Middleton, 1990)

According to them the term memory is determined by the interaction. The distinction between a memory-representation (private or public) and fantasy is created during the discourse by the partner who strengthens the authenticity and the reality of the memory. (Harré, 1997). (I would like to express that, for them, the socialization of remembering through language socialization means the 'strengthening of authenticity' process, mentioned above.)

Memories raise in the context of a discourse and remembering is tied to interaction in some ways. Therefore,this trend explains remembering only in social context.

The trends I briefly introduced all emphasize the importance and inevitability of dealing with social factors, in the case we would like to get closer to the everyday processing and functioning of memory. In the 'proper' investigation of human memory we found rich modes of handling the social factors and we can trace among them Halbwachs' influence.

Conclusion

As a summary we can say that Halbwachs' concept of collective memory becomes central and important again because of the fact that some current researchers find the essence of the constructive nature of memory in communication, language and language conventions.

We arrived to the rediscovery of Halbwachs' notions. Nowadays we have the opportunity to evaluate them in a more subtle way in the reflection of these recently proposed theories and experimental works.

References:

Barclay, C.R-Smith,T.S. (1992): Autobiographical Remembering: Creating personal Culture, IN: Theoretical Perspectives on Autobiographical Memory, eds.: Conway, M.A.-Rubin, D.C.-Spinnler, H.-Wagenaar, W.A., Kluwer Academic Publishers

Bruner, J.-Lucairello, J. (1989): Monologue as Narrative Recreation of the World, IN: Narratives from the Crib, ed.: Katherine Nelson, Harvard University Press

Fitzgerald, J. M. (1992): Autobiographical Memory and Conceptualizations of the Self, IN: Theoretical Perspectives on Autobiographical Memory, eds.: Conway, M.A.-Rubin, D.C.-Spinnler, H.-Wagenaar, W.A., Kluwer Academic Publishers

Fitzgerald, J.M. (1995): Intersecting Meanings of Reminicence In Adult Development and Aging, IN: Rubin, D.C ed. Reconstructing our past An overview of Autobioraphical Memory, Cambridge University Press

Fivush, R.-Haden, C.-Reese, E. (1995): Remembering, recounting and reminiscing: The development of autobiographical memory in social context, IN: Rubin, D.C ed. Reconstructing our past An overview of Autobioraphical Memory, Cambridge University Press

Harré, R. (1997): Érzelem és emlékezet: a második kognit0v forradalom, *Replika 25* pp. 141-152.

Halbwachs, M. (1925):Les cadres sociaux de la mémoire, Libraire Alcan, Paris

Hirst, W.-Manier,D. (1995): Opening vistas for cognitive psychology, IN: Sociocultural Psychology, Theory and Practice of doing and knowing, eds: Martin,L.M.-Nelson, K.-Tobach, E.,Cambridge University Press

Middleton, D.-Edwards, D. (1990) (eds.): Collective Remembering, Sage Publications

Neisser, U. (1988): Five kinds of knowledge, *Philosophical Psychology 1.* pp. 35-59.

Nelson, K. (1989): Monologue as Representation of Real-Life Experience, IN: Narratives from the Crib, ed.: Katherine Nelson, Harvard University Press

Pléh Csaba(1992): Pszichológiatörténet, Gondolat, Budapes

Pléh Csaba (1996a): Szociális modellek és a megismeréskutatás-Spekulatív áttekintés, *Pszichológia (16) 3,* pp. 209-235

Rubin, D.C.- Kozin, T.S. (1984): Vivid Memories, *Cognition 16.* pp. 81-95

Robert Kramer, Ph.D.,
editor of A Psychology of Difference:
The American Lectures of Otto Rank (Princeton University Press, 1996). Department of Management,George Washington University, Monroe Hall 403,Washington D.C., United States of America.
E-mail: KRAMER_B@BLS.GOV, phone at the USA: 202-606-6857.

ABSTRACT

"Otto Rank's Contributions to Client-Centered, Gestalt, and Existential Ways of Understanding Creativity and the Relational Self"

A leading disciple and confidant of Freud, Otto Rank shocked the psychoanalytic world with The Trauma of Birth (1924). In this book, Rank proposed that the child's pre-Oedipal relationship to its mother was the prototype of the therapeutic relationship between analyst and patient. Rank had abandoned the one-person psychology of classical "neutral" analysis for a more humanistic form of psychotherapy that focused on the "here-and-now" emotional experiences of analyst and patient. A person-to-person relationship, argued Rank, was more important for healing than interpretation of intrapsychic processes.

For overturning the priority of the Oedipus complex, Rank was forced out of Freud's inner circle. In 1926 he moved to France and, later, to America. Until his death in 1939, Rank wrote profusely on art, psychotherapy, and neurosis as a failure in creativity. But as far as official psychoanalysis was oncerned, he was already dead. All of his students were required to be re-analyzed by Freudians to retain their membership in the American Psychoanalytic Association.

The last two decades, however, have seen a remarkable renewal of interest in the work of Otto Rank. The Rankian revival began in 1973 with Ernest Becker's Pulitzer-Prize winning The Denial of Death, a brilliant merger of Rank's post-Freudian writings with the thought of Kierkegaard. A return to Rank was vital, argued Becker, to afford psychoanalysis a theory of creativity as compelling as Freud's theory of sexuality. "There is no substitute for reading Rank," said Becker, "he is a mine for years of insights and pondering" (Becker, 1973, p. xii).

In 1982, Esther Menaker, a member of the board of The Psychoanalytic Review, published the first comprehensive treatment of Rank's ideas. Rank, she concluded, was the unacknowledged forerunner of ego psychology as well as the object-relations theories of W. R. D. Fairbairn, D.W. Winnicott, and Margaret Mahler (Menaker, 1982). In 1985, E. James Lieberman wrote the first full-scale biography of Rank, based on dozens of interviews with respondents who knew Rank. Following the pioneering researches of Paul Roazen (1974), Lieberman

uncovered a host of lies in Ernest Jones' treatment of Rank in Volume III of his Freud biography. "The truth about Rank himself can scarcely be found in print," said Lieberman, who was amazed at he abundance of errors concerning Rank's life and work in the literature of psychology (Lieberman, 1985, p. xv).

There are exceptions. Carl Rogers, for example, always acknowledged that the thought of Rank inspired him more than any other, early on, when he was still practicing therapy in the old-fashioned "directive" way. "I became infected with Rankian ideas," Rogers once said (Kramer, 1995, p. 77). Rollo May and Irvin Yalom, two leading existential psychologists, credit Rank as the most important precursor of existential psychotherapy (May, 1983; Schneider & May, 1995; Yalom, 1980). Paul Goodman, the major theoretician of Gestalt therapy, was deeply affected by Rank, going so far as to describe Rank's writings on art and creativity as "beyond praise" in Gestalt Therapy (Perls, Hefferline & Goodman, 1951, p. 395). "Rank hit on the creative act as psychological health itself," concluded Goodman (ibid., p. 237). While constructing the theoretical basis for Gestalt, Goodman leaned heavily on Rank, whose "formulation [of the 'here and now']," according to Goodman's biographer, "has the therapeutic moment in view more explicitly than any other" (Stoehr, 1994, p. 126).

What is it about Rank's ideas that has touched so many humanistic and existential psychologists? This talk will trace Rank's influence on such pioneers of "The Third Force" as Carl Rogers, Paul Goodman and Rollo May, and demonstrate that Otto Rank is the forgotten grandfather of humanistic psychology.

topics: stress, chronic pain
qualitative research

Introspective Psychology on Pleasure and Pain: Phenomenological Implications

Robert Kugelmann, University of Dallas

At the end of the nineteenth century, psychologists debated the nature of pain and pleasure (Dallenbach, 1939, p. 337). The terms of the debate implicated the nature of psychology itself. Two of the antagonists represented the "new psychology" and its physiological basis: the sensation theory, which held that pain is a sensation mediated by specific nerve endings; and the summation theory, which argued that pain occurs when a threshold of stimulation is passed, regardless of the nerve being stimulated. The third group of contenders included consisted of introspective psychologists; while having diverse theories, they paired pleasure with pain and viewed both as qualities of mental states, arguing from the evidence of introspection and from the philosophical tradition, which weighed in heavily in its favor.

1
2
3

The sensation and summation theories were still in the lists years later, when Melzack and Wall (1965) sought their reconciliation in the gate control theory of pain. But the pleasure-pain theory of pain had been forgotten, along with its champion, Henry Rutgers Marshall (1852-1927). But there is reason to read again the arguments of Marshall and his allies. For the sensation theory has been itself displaced, and contemporary understandings define pain as "an unpleasant experience," admitting that "pain is always subjective," (Pain terms, 1986). There are further challenges to contemporary certainties that Marshall offers. After reviewing his theory, I shall appraise it in the light of phenomenologies of pain.

Introspective Psychology and Resistance

There is no single meaning of "introspective psychology," it having had many

Marshall was ejectively an architect

variations (Danziger, 1980) in the late nineteenth century. "Introspection" ambiguously conflated self-observation with proprioception in the broad sense. However, if the term mean giving voice to experience, then introspective psychology remains indispensable, because it acknowledges the voices of persons. Even though introspection suffered suppression at the hands of the "objective approach" (Stout, 1939), it remains the animating fount and destiny of psychological theory. I thus propose that we reconsider Marshall's theory of pleasure and pain in the broader context of introspective psychology as a form of resistance to the modernization of the science of the soul.

While some introspective psychologies sought modernization, others did not. The hallmarks of this modernization are functionalism and standardization. Marshall's theory did not lend itself to a problem-solving mentality, having no ready application in industry, education or medicine. Marshall addressed pleasure and pain because he was interested in aesthetics, which was understood in his day as "the study of the useless" (James, 1892/1985, p. xxviii). Moreover, Marshall's understanding of introspection could not serve "technoscience," a term Coon (1993) uses to express psychology's assumption of the goal of the "standardization of both the process and product in manufacture" (p. 759) of knowledge in the early twentieth century. While Marshall expected his readers to test his propositions for themselves, the training necessary to do so was a philosophical education. There was no procedure or protocol that could substitute or replace it. This understanding of verification was not compatible with industrialized knowledge production.

Marshall's psychology resisted modernization in a more profound way, however. Even though he postulated a thoroughgoing psychoneurological parallelism, he denied that

physiology was the basis of psychology: "we must grant it to be true of all . . . scientific experiments, that introspection is their final determinant" (Marshall, 1908, p. 3). The reader can and must verify what Marshall wrote; the reader is an interlocutor and not the consumer of expert knowledge. Conversation rather than so-called objective data grounds psychology. And speech as the ground means that the face-to-face relationship is the final determinant. This final determinant is crucial for pain and pleasure which are, we may say, "states" of the person and thus non-objectifiable and essentially invisible. Marshall's psychology of pleasure and pain resists the visualization of pleasure and pain in the image of the nervous system and thus implicitly attends to them as the expressions of the other.

Marshall's Theory of Pleasure and Pain

Marshall presented his hedonic theory in a series of essays (1889; 1891a; 1891b; 1892; 1894a; 1895a; 1895b; 1896) and in a book (1894b). His thesis was that pain, of necessity tied to pleasure, is a quality of conscious experience. Marshall's pairing of pain with pleasure was not innovative; it seemed to have been the norm at the time (e.g., Bain, 1892; Mead, 1895; Miller, 1895; Royce, 1904). He stated that the primary quality was "pleasure-pain" or the "algedonic quality." Every conscious phenomenon must be painful, indifferent or pleasurable. By quality or quale, Marshall meant a differentiation of a mental state. Pleasure-pain belongs to the "primary quales which affect all presentation" (1889, p. 527).

Pleasure-pain was but one general quality of relation, according to Marshall, the others being intensity, manifoldness, realness and time. He distinguished between these general qualities and "special qualities," such as color or tone, that differentiate sensations. The algedonic quality affected every element within the complex of a conscious moment, such that

the resulting moment could be one of "mixed feeling. Marshall defined pleasure and pain broadly, following "common sense" (1894b, p. 3). All pains, as all pleasures, are the same, even though many types exist. Pleasure and pain had been defined by some thinkers as sensations, and by others as emotions. For Marshall, there are pleasurable and painful sensations and emotions, but pleasure and pain are neither. Being a general quality, all elements of consciousness have the algedonic quality as one of their essential attributes: "Intellectual pleasures and pains are no meaningless terms; they are as full of actual import as are the phrases sensational and emotional pleasures and pains" (1894b, pp. 36-37). He claimed that this classification was based in ordinary experience, and not in specialized knowledge of the nervous system.

Marshall did not differentiate between physical and mental pain: "I do not consider this separation of physical from other pleasures and pains fundamentally important; it is but a special form of the natural division of psychic states which for so long led to the adoption of the so-called 'faculty psychology'" (1909, p. 102). In part, this position stemmed from his insistence that pleasure and pain are aspects of the same quale: "if we separate schmerz from unlust, we should in like manner separate sense pleasures from other pleasures" (1909, p. 101). Moreover, he did not isolate physical from non-physical pains because his evidence was informed reflection upon experience rather than physiological investigation. Marshall may have been the last thinker in psychology to hold to the essential unity of all kinds of pain.

William James's psychology of consciousness was a strong influence on Marshall, who viewed each "pulse of consciousness" (1895b, p. 597) as composed of discrete elements that

are in principle distinguishable. He stressed, however, the unity of each pulse of consciousness: "it is possible to look upon all mental states as we experience them, as differentiations of some original primal form of consciousness which in truth we can only speak of theoretically because we must grasp it as presented in its differentiations; our mental fields are too late a development to appear apart from all differentiations" (Marshall, 1894b, p. 46). Each differentiation of the field of consciousness can have its algedonic quality, with the resulting state of consciousness having a mixture of pleasures and pains (1894b, p. 57).

Having eliminated the sensation theory from consideration, Marshall elaborated a theory of the physical basis of pleasure and pain, which he claimed to be "the true interpretation of the Aristotlean efficiency-theory" (1891, p. 340): "Pleasure and pain are determined by the relation between the energy given out and the energy received at any moment by the physical organs which determine the content of that moment; Pleasure resulting when the balance is on the side of the energy given out, and Pain when the balance is on the side of the energy received" (1891, pp. 470-71). To this nutrition-based theory of the algedonic he added later a "neururgic" account, according to which pleasure and pain "relate respectively to the efficiency and inefficiency of the neural elements whose activity corresponds with the pleasant or painful presentations" (1909, p. 251). Marshall's physiology drew in an important way on ancient conceptions of the body. Assuming that pleasure and pain are one phenomenon, and assuming a thoroughgoing psychophysiological parallelism, he recalled "a theory which has been persistently suggested since the days of early Greek thought, and which relates pleasure to efficiency, and pain to inefficiency, in the activity of the individual who experiences the pleasure and pain" (1909, p. 250). In other words,

Marshall's position was implicitly teleological, pleasure and pain being bound to species-specific and individual purposes.

Phenomenological Reappraisal

Marshall (1895a) realized that the sensation theory was coming to dominate ~~because~~ psychology: "too much emphasis is given to-day . . . to the physiological basis of psychology" (p. 60). For Marshall, physiological evidence had to be squared with introspective evidence. With this link to phenomenological thought, I will offer some initial reappraisals of Marshall's position, in order to suggest that contemporary psychologies of the subject, whether they be phenomenological, critical, humanistic or personalist, listen again to Marshall and his allies.

Merleau-Ponty (1945/1962) extends Marshall's complaint about the over-emphasis on physiology as a supposed ground of psychology: "Psychology and physiology are no longer, then, two parallel sciences, but two accounts of behavior, the first concrete, the second abstract" (p. 10). The first reappraisal, then, affirms Marshall in claiming that the sensation theory suffers from misplaced concreteness: "There is no physiological definition of sensation . . . because the physiological event itself obeys biological and psychological laws" (Merleau-Ponty, 1945/1962, p. 9). But Merleau-Ponty's critique is not based on the primacy of introspection, but on that of the lived body. With this vantage point, further reappraisals of Marshall are possible.

The second reappraisal concerns the nature of sensations themselves. For phenomenology, sensory experience must be understood as prepersonal being-in-the-world. This position criticizes the prejudice of the objective world present in the sensation theory and in empiricism generally, including Marshall's. Sensation thus implicates motility. The

sensation theory of pain overlooked sensorimotor unity. Pain is a "performance of the organism . . . . To the inclination towards flight which follows a painful stimulus is added the inability to escape from pain. (Buytendijk, 1943/1961, p. 115, 121). Buytendijk concludes that the essence of pain is "to disrupt man's inner 'vital' and psychic structure with incomparable force" (p. 132). Because of this, pain has the possibility of being suffered.

That pain implicates action develops Marshall's understanding that pain is "a feeling which we seek to get out of consciousness and to keep out" (Spencer, quoted in Marshall, 1920, p. 135). But Marshall's work deepens in turn the phenomenological view, and this constitutes a third reappraisal. The self "rejects and fails to assimilate what is painful" (Marshall, 1909, p. 591), and Marshall concluded that the telos of pain is unreality: "pain in connection with a given presentation involves a tendency to the failure of this presentation in attention, and this failure must tend to involve, in the moments to follow, the instability of the presentation which will give it unrealness" (1909, p. 397). Because of this tendency, pain is problematic. Pain should not be. Marshall did not claim pain is unreal, rather that we tend to de-realize it, and for consciousness, it ought not to be. Thus for Marshall as for the phenomenological tradition, pain is not only a content of experience; essential to pain is aversion or repulsion (Buytendijk, 1943/1962; Sartre, 1956; Levinas, 1988), even if the painful be cultivated.

To tie pain to action deepens Marshall's theory that pleasure-pain is a general quale of consciousness. Stripped of empiricist language, it claims that existence is drawn and repulsed, touched and wounded, by the fields of existence. In Heidegger's (1979/1992) terms, we are always attuned or disposed to the world in some way. In this direction lies a reappraisal of

algedonics or Marshall's hedonism. What Marshall contributes to this notion of Befindlichkeit is a decidedly embodied sense in his emphasis on pleasure and pain, that is absent in Heidegger's grasp of pain. Levinas (1969) criticizes Befindlichkeit by addressing enjoyment, which he calls "the very pulsation of the I" (1969, p. 113): "it [the I] acquires its own identity by this dwelling in the 'other'" (1969, p. 115). In pain, "we . . . witness this turning of the I into a thing" (Levinas, 1969, p. 238). Without developing this notion further, we see how for Levinas (to use Marshall's terms), pleasure and pain are implicated in all phases of existence. Pleasure-pain belongs alongside temporality as fundamental to existence. This way lies a fourth reappraisal of Marshall and the old introspective psychology (see Stout, 1899/1977, for another psychology of pleasure-pain).

Finally, for Scheler (1966/1973), Buytendijk (1943/1962) and Levinas (1988) pain is fundamentally an ethical question, and whatever functional value some pain may have does not exhaust its significance. Along these lines, Marshall noted: "We set for ourselves these problems as to the existence of Pain, Error, Evil and Ugliness only because of a deep seated conviction that they display marked unrealness, and that Pleasure, Truth, Goodness and Beauty are more real" (1909, pp. 399-400). Pain is primarily a privatio bonum, rather than a useful signal of tissue damage. And it is no accident, then, that Marshall, while not discounting a functional view of pain, has a more ample view, taking into account its uselessness. In addressing the ethical telos of pain, Marshall exceeds his own aesthetics, and this is the final reappraisal.

References

Bain, A. (1892). Pleasure and pain. Mind 1, 161-187.

Bonica, J. J. (1990). History of pain concepts and therapies. In J. J. Bonica (Ed.), The management of pain (2nd ed., Vol. 1, pp. 2-17). Philadelphia: Lea & Febiger.

Coon, D. (1993). Standardizing the subject: Experimental psychologists, introspection, and the quest for a technoscientific ideal. Technology and Culture, 34, 757-83.

Dallenbach, K. M. (1939). Pain: History and present status. American Journal of Psychology 52, 331-347.

Danziger, K. (1980). The history of introspection reconsidered. Journal of the History of the Behavioral Sciences 16, 241-262.

Heidegger, M. (1992). History of the concept of time (T. Kisiel, Trans.). Bloomington: Indiana University Press.

Heidegger, M. (1992). History of the concept of time (T. Kisiel, Trans.). Bloomington: Indiana University Press. (Original work published 1979)

James, W. (1985). Psychology: The briefer course. Notre Dame: University of Notre Dame Press. (Original work published 1892)

[James, W.] (1894). Marshall's pain and pleasure. Nation 59, 49-51.

Levinas, E. (1969). Totality and infinity: An essay on exteriority (A. Lingis, Trans.). Pittsburgh: Duquesne University Press.

Levinas, E. (1988). Useless suffering (R. Cohen, Trans). In R. Bernasconi & D. Wood (Eds.), The provocation of Levinas: Rethinking the other (pp. 156-67). New York: Routledge.

Marshall, H. R. (1889). The classification of pleasure and pain. Mind 14, 511-536.

Marshall, H. R. (1891a). The physical basis of pleasure and pain (I). Mind 16, 327-354.

Marshall, H. R. (1891b). The physical basis of pleasure and pain (II). Mind 16, 470-497.

Marshall, H. R. (1892). Pleasure-pain and sensation. Philosophical Review 1, 625-648.

Marshall, H. R. (1894a). Are there special nerves for pain?. Journal of Nervous and Mental Disease 19, 71-84.

Marshall, H. R. (1894b). Pain, pleasure, and aesthetics. New York: Macmillan.

Marshall, H. R. (1895a). Pleasure-pain and emotion. Psychological Review 2, 57-64.

Marshall, H. R. (1895b). Physical pain. Psychological Review 2, 594-598.

Marshall, H. R. (1907). The time quality. Mind 16, 1-26.

Marshall, H. R. (1908). The methods of the naturalist and the psychologist. Psychological Review 15, 1-24.

Marshall, H. R. (1909). Consciousness. New York: Macmillan.

Marshall, H. R. (1910). William James. Science, 32, 489-92.

Marshall, H. R. (1920). Mind and conduct. New York: Charles Scribner's Sons.

Mead, G. H. (1895). A theory of emotions from the physiological standpoint. Psychological Review, 2, 162-63.

Melzack, R., & Wall, P. D. (1965). Pain mechanisms: A new theory. Science 150, 971-979.

Miller, D. S. (1895). Desire as the essence of pleasure and pain. Psychological Review,

2, 164.

Morris, D. (1991). A culture of pain. Berkeley: University of California Press.

Pain terms: A current list with definitions and notes on usage. (1986). Pain, (Suppl. 3), S216-S221.

Royce, J. (1904). Outlines of psychology. London: Macmillan.

Sartre, J.-P. (1956). Being and nothingness (H. Barnes, Trans.). New York: Philosophical Library.

Scheler, M. (1973). Formalism in ethics and non-formal ethics of values (M. S. Frings & R. L. Funk, Trans.). Evanston: Northwestern University Press.

Stout, G. F. (1977). Manual of psychology, in D. N. Robinson (Ed.), Significant contributions to the history of psychology, 1750-1920, Series A, Orientations, Vol. IX. Washington, DC: University Publications of America. (Original work published 1899)

WHY IT IS IMPORTANT TO JOIN THE INTERNATIONAL UNION OF PSYCHOLOGICAL SCIENCE: PSYCHOLOGICAL ASSOCIATIONS IN SOUTH AFRICA AND THE GERMAN DEMOCRATIC REPUBLIC

Kitty Laerz
Department of Psychology
University of Jena
Germany

Johann Louw
Department of Psychology
University of Cape Town
South Africa

Psychology as a discipline developed quite spectacularly after the second World War in many countries. This development, however, occurred quite unevenly. A brief period of joint research between the two authors allowed us to examine the development of psychology in two societies which underwent quite dramatic social change since the end of World War II: the German Democratic Republic, and the Republic of South Africa. We examined one aspect in particular: how national psychology associations were formed, and how they oriented themselves to the international psychology community, and the International Union of Psychological Science in particular.

The South African Psychological Association

The South African Psychological Association (SAPA) was the first national psychological association in South Africa, formed in 1948. It was formed as the official organization to speak for psychologists in academic and professional matters. Until then South African psychologists were organised as part of the South African Association for the Advancement of Science: they attended its congresses and read papers there, and they published in its journal, The South African Journal of Science. However, the impetus for the formation of the SAPA did not come from the academic/scientific arena, but from the professional side of the discipline.

Psychology entered the South African mental health field after World War II in increasing numbers. The impetus for the formation of a national psychological association thus came from another profession. At the 34th South African Medical Congress, held in Durban in October 1946, the Division for Neurology and Psychiatry recommended to the South African Medical Association that a register for clinical psychologists be instituted. In February 1948 the Medical Association invited seven psychologists and five psychiatrists to a meeting to discuss this. At this meeting it became clear that it would have been very difficult to move to the registration of clinical psychologists without the existence of a national psychological association which could represent the interests of psychologists and which could set standards for training and qualifications for registration. The seven psychologists present at that meeting immediately looked into the matter (see G.P. Louw, 1990). They met on 10 February 1948, to form a

provisional Council for a psychological association, until a general meeting could be held and a proper council elected. The chairperson was Prof. A.J. la Grange of Stellenbosch University, with Prof. I. D. MacCrone of the University of the Witwatersrand as member and Dr L.J. Reyna, of the same University, as secretary.

The founding meeting of the SAPA was held from 17 to 18 July 1948 in Bloemfontein. Thirty-four people were present at this meeting. The first annual meeting of the Association was between 4 and 5 July 1949 in Kimberley. Only in 1970 did it start publication of the South African Journal of Psychology.

Although the constitution of the SAPA indicated that it combined scientific and professional concerns, numerous examples could be found of how the Association involved itself with the practice of psychology, and with the private practice of psychology in particular: the negotiations in the 1960s to include the private psychological practitioner under the protection of professional provident societies; the Association's involvement in the standardization of the New South African Group Test of Intelligence in the 1950s and 60s; its canvassing for control over psychological test material and its contribution; and its international links with test distributors, e.g. the Psychological Corporation in the USA.

The Society of Psychology of the GDR

The Society of Psychology of the GDR (Gesellschaft für Psychologie in der DDR; GfP) was founded on 19 October 1962 in Berlin (East). Before this the academic psychologists at least could represent themselves in the Scientific Council of Psychology at the Ministry of Higher Education, formed on 13 February 1959. Following German unification in 1990 the last board of the GfP took an official decision to dissolve the Society and to leave it to its members to apply for membership in the Deutschen Gesellschaft für Psychologie and/ or in the Berufsverband Deutscher Psychologen (Pawlik, 1994).

The foundation of the Society of Psychology was based on a decision taken on 13 October 1961 by the Scientific Council of Psychology at the Ministry of Higher Education. This decision included the task of establishing an initial commission to found an East German Society of Psychology. (Archiv der Parteien und Massenorganisationen der DDR im Bundesarchiv. Sign.: IV2/9.04/219). One year later, on 19 October 1962, at the founding meeting of the SfP, 54 psychologists applied to become members.

At every national congress of the Society the Board and the president of the Society were elected by its members. During the 28 years the Society existed three different presidents were choosen by vote (from 1962 to 1968, W.Straub; from 1968 to 1975, F.Klix; and A. Kossakowski from 1975 to 1989). In the last month of its existence H.-D.Roesler became the president of the Society.

Even if the Society of Psychology was considered a scientific and professional organization, the actual tasks it performed had more to do with academic than professional matters. This assumption can be supported by the fact that in the first unpublished statute the name of the new Society suggested by the intial commission was "Scientific Society of Psychology in the GDR". (Archiv der Humboldt Universität Berlin; Akten der GfP; Sign.: 1.1.-1.3.)

The International Union of Psychological Science

"Despite the fact that Psychology has received varying amounts of support in different countries of the world, no single country has established a monopoly on new and imaginative ideas in psychology. It is within this general context that the International Union of Psychological Science, IUPS, has been seeking its unique roles" (Russell, 1966, p. 66).

The International Union of Psychological Science was formally instituted in July 1951, at the Thirteenth International Congress of Psychology held in Stockholm, Sweden. Historically, the IUPS is natural growth from the International Organizing Committees of the earliest congresses, which started in 1889 by helding the First Congress of Psychology in Paris, France, and under the presidency of T.A. Ribot (Holtzman, 1976). Membership of the IUPS belongs to national psychological societies, rather than individuals. It was clearly recognized "that the basic strength of the organisation is derived from the national societies which are members" (Russell, 1966, p.66).

Since there was no category of individual membership in the Union, the various national societies had to ensure that only properly qualified psychologists could belong to them. No restriction, other than proficiency in the discipline, had to be imposed on membership (Russell, 1966). In addition, the goals and objectives of the statutes of the national psychological societies has to be consistent with those of the Union.

Membership of the International Union of Psychological Science

South Africa:

In 1958 the SAPA applied to become a member of the International Union of Psychological Science. The aims were to establish closer links with psychology in other countries, and to find out more about the work of psychologists in other countries. Otto Klineberg was then the Secretary General of the Union. In its newsletter (Psygram, July 1960, 2(7), p. 140), the Association stated quite clearly that it would apply in that year to the International Union in Bonn, because it would "mean recognition of the national status of our organisation".

In the mean time, however, an issue has been raised which impacted directly upon the Association's application. In 1956 Josephine Niadoo, who was classified as "Indian" according to South Africa's Population Registration Act of 1950, applied to join SAPA. Her application forced, for the first time, the issue of black membership of SAPA - in a country where strict separation of 'the races' was legally enforced. The Council of SAPA asked her to withdraw her application, in the light of the division of opinion within the Association on this matter. This she did (Louw, 1987). However, the Association now had to consider what its policy would be in the matter, and this raised enough conflict for the SAPA to split into two associations five years later. Various committees were appointed to investigate a suitable policy and to make proposals. In 1960, at its national congress in Durban, the matter was brought to a head when another "non-European" of Indian extraction, C. Ramfol, was nominated as a member. This time the application was not withdrawn, and had to be dealt with.

The debates divided members fairly neatly into 'supporters of apartheid policies' and 'opponents of apartheid policies'. The country's racial policy was raised quite explicitly in these debates. For example, it was pointed out that the prime minister, H. F. Verwoerd (himself an ex-professor of psychology, and an honorary member of SAPA), insisted that all professional associations be racially segregated (Louw, 1987).

In 1961 the Association met for its annual congress at Stellenbosch. The Council had resolved in the mean time to admit 'whites and non-whites' as members of SAPA, and put this resolution to the congress. After a lively and often acrimonious debate, the decision was ratified, and the chairman closed the meeting with the request that the matter not lead to bad feelings among members.

This, however, was a futile hope. Strong group formation and behind-the-scenes planning had already taken place, and an action committee was formed by those opposed to admitting black psychologists to the Association. This group of psychologists then decided to break away from SAPA. On 23 June 1962 approximately 200 people gathered in Pretoria to establish the Psychological Institute of the Republic of South Africa (PIRSA), with its membership restricted to whites only.

In July 1962 the Executive committee of SAPA read out this telegram from Otto Klineberg: "The Executive Committee of the International Union of Scientific Psychology wishes to congratulate the South African Psychological Society on its election as a member of I.U.S.P. Letter follows" (Psygram, Aug 1962, 4(8), p. 179). Thus in July 1962 the SAPA was elected as a full member of the International Union of Psychological Science, and it was something the Association should be proud of, the chairman stated (Psygram, Aug 1962, 4(8), 171).

In the debate about admitting black members to SAPA, quite a few references were made to the International Union and SAPA's membership. At the annual general meeting of July 1957 already, this issue was raised inconnection with affiliation to the International Union. "The question of establishing separate professional registers for European and Non-European psychologists was then discussed, and it was found that little could be said in favour of such separation. The points in favour of one common register were many, ... that (c) overseas psychological organizations would not recognize a register based on other criteria than professional qualifications and competence. The latter argument was also brought forward in respect of membership: the SAPA would probably not be admitted to the International Union of Scientific Psychology if it were to restrict membership on racial grounds. The counter-argument was that the Association would also have to consider the general opinion prevailing in the Union" (Psygram, 1962, 4(6), 148).

Biesheuvel, a senior South African psychologist, had this to say about the matter at the annual meeting of SAPA in Stellenbosch, September 1961 (Supplement to Psygram, 1961, 3(11), 247 - 266):

"I did discuss with Prof. Klineberg the present Chairman of the International Union of Scientific Psychology, the possibility of South Africa's affiliation to the International Body.

"As you know we applied last year and so far the Association has not been informed of the official outcome of this at all. In fact, at its last meeting it was felt that South Africa could not be affiliated if we had clauses of discrimination in our Constitution. Knowing that we were still

engaged in discussions of this matter, I asked Prof. Klineberg not to convey this decision - to hold it in abeyance, because I felt that this was a matter which we would solve amongst ourselves. I did not think, at that stage, that we were going to reach a solution as easily if there was a suspicion of external pressure. I also put to him that the type of solution we were discussing was one where the Constitution would admit members of all races but that in recognition of local problems and points of view, local centres would be left entirely free to make such ad hoc arrangements as indicated by their peculiar circumstances. I was given to believe that if that were the solution we would resolve on, that there will be no difficulty at all about affiliation to the International body" (p. 248-249).

German Democratic Republic:

The 1963 published statute, paragraph 2 "Objectives of the Society of Psychology in the GDR" point 5, stated that the Society of Psychology wanted to become a member of the International Union of Psychological Science. This objective can also be found in the unpublished statute which was formulated by the initial commission. It is therefore safe to assume that membership of the IUPS was a main goal right from the start. It was granted membership in 1966, after approval of its application had been delayed in 1964.

The reasons for why this was the case are interesting. Commonly used arguments such as the importance of exchanging information and scientific results appeared. But it was also clear that obtaining membership as a national psychological society in the IUPS, had implications in terms of being accepted as a sovereign nation. Furthermore, being accepted as a member in an international scientific association could be used as proof of the increased importance of psychology within the country, an argument which could be used to obtain the support of officials.

We already pointed out that the content of the unpublished statute suggested by the initial commission was different from the one published in the journal "Probleme und Ergebnisse in der Psychologie". One difference can be found in paragraph 2, specifying the objectives of the society. In the unpublished statute the Society of Psychology in the GDR expressed itself as fighting against unscientific attitudes within psychology concerning theories and practice. It also stated that scientific knowledge should be propagated and made popular by the Society of Psychology in the GDR. This objective was changed in the published statute. There we find the following expression: "It (the society, K.L.) is fighting against all kinds of quackery ("Scharlatanerie") in its field. It is fighting against unscientific views and one of the main tasks will be the argument with anti-humanistic and imperialistic theories. In this case the society will support especially all scientists living in West Germany who are working for humanism, democracy and social progress"(Probleme und Ergebnisse der Psychologie; 8/1963. S.97).
A second aspect must be mentioned. In both the unpublished and published statute we find under point 2 in paragraph 2, that psychology should be developed on the basis of the dialectic historical Materialism and the scientific "discussion of different opinions" should be promoted as well (Probleme und Ergebnisse der Psychologie; 8/1963. S.97). The former objective prevented pluralism in psychology from the beginning.

On September 4th, 1964, James Drever, then president of the IUPS, wrote a letter to the Secretary of the Society of Psychology making the following suggestion: "...The question

before the Committee was how best to prepare the application, so that it might be accepted by the Assembly at its meeting in Moscow in 1966. In this connection it was felt that the second paragraph of § 2 (sic!) of your Statutes would be controversial and might lead to further delay in the acceptance by the Assembly of your Society. The Soviet and Polish representatives on our Committee were in argreement with this view ..."(Archiv der Humboldt Universität Berlin, Akten der GfP, Sign.: 23.1.719-757).

Two documents in the archive of the Central Committee of the Socialist United Party show how the delay was seen by contemporaries. The first is a letter written by Helmut Kulka and addressed to Jochen Siebenbrodt (the secretary of the Society) on December 30th, 1964:

"Dear Jochen,
as you requested in your letter from November 12th, 1964 I will give you information about the talk which I had with Prof Leontjew in Ljubliana, which Dr Hacker shared as well.
Leontjew told us:
At the last meeting of the Executive Committee of the IUPS in Rome the application of the Society of Psychology in the GDR was discussed and put aside. Reason: In the statute there are expressions which can be seen as political goals and/ or which can been interpretated as judgement over another society. The IUPS is opposed to commitments of political, confessional or worldview matters as well as judgements on other societies who are members of the IUPS. Prof. Leontjew gave the recommendation to modify, i.e. to change, the statute and to apply again after some time has passed. The same arguments and recommendations were given to me by Prof Tomaszewski (Warsaw) ...." (Archiv der Parteien und Massenorganisationen der DDR im Bundesarchiv. Sign.:IVA2/9.04/213; Doc. N°10). Based on the information given by Leontjew, the main reason for turning down the application therefore was seen in the attack against West Germany.

The following interpretation of the delay by Maeder, then representive of the Central Committee of the Socialist United Party, Department for Science, responsible for psychology, will give us another picture:

"The application for membership of the IUPS was handed in one year ago. Prof Leontjew (USSR) and Prof Turski (Poland) (I assume that he meant Prof Tomaszewski from Warsaw ?, K.L.) are members of the Executive Committee of the IUPS. When the members of Executive Committee discussed our application some representatives of western countries were objecting our membership because of the following sentence included in our statute: 'It (the Society of Psychologie in the GDR; K.L.) contributes to the development of the psychology on the basis of the dialectical and historical Materialism and to the promotion of the scientific discussion of different opinions`.
Although the president of the German Society of Psychology Prof Metzger was arguing against the doubts raised and supported the application, neither Prof Leontjew nor Prof Turski supported our application actively.
Therefore it was possible that our application was put aside and this will lead to real difficulties because of the expected change in the membership of the Executive Committee (likelihood of Prof Metzger leaving the Executive Committee), or anyway the "Hallstein Doctrine" will come into force" (Archiv der Parteien und Massenorganisationen der DDR im Bundesarchiv. Sign.:IV A2 /9.04/ 213 / Doc. N°11).

It can be assumed that Maeder's interpretation is based on the information given by Drever. It becomes also clear, that he is really disappointed by the fact that Leontjew and Turski (Tomaszewski) (as representatives of the Eastern European countries) did not support the application of the Society of Psychology.

The reference to the Hallstein-Doctrine, an unwritten law in the 1950s saying that the West German government will break off diplomatic relations to states who recognise the GDR as a sovereign state, shows also the strong link between politics and science. Therefore it seems a bit ironic that Maeder can in fact regard in Metzger, as a representative of the West German Society, a supporter of the application. His view also indicates that the denied acceptance of the Society of Psychology to the IUPS was seen by Maeder as a denied acceptance of the GDR as sovereign society.

After a reformulation of these two points in paragraph 2 and the second application, the Society of Psychologie was granted membership of the IUPS during the XVIIIth International Conress of Psychology in Moscow in 1966.

In the sixties and seventies the IUPS membership of the Society of Psychology was used by the contemporaries to legitimize psychology within the GDR, rather than to participate in scientific debates within the IUPS. No psychologist from the GDR has ever published a scholarly article in the International Journal of Psychology. We found two publications by East German scientists, but those simply introduced the Society of Psychology (Klix & Siebenbrodt, 1968; Schaarschmidt, 1990). One explanation was the language barrier, which was a major obstacle to publication. Another explanation can be seen in the fact that the International Journal of Psychology was not available at any university or library in the GDR and therefore it was not known among the scientists. The Society of Psychology was the only member which did not order this Journal. The reason for not ordering must be seen in the limited financial resources at the universities in the GDR in general.

Taking activities (presenting papers and posters or organizing symposia) at international congresses of the IUPS as a second measure, we noticed that since the Society of Psychology was a member of the IUPS, scientists presented scientific work to every International Congress. The number of presentations varied depending upon in which country the Congress took place, i.e. the geographical distance and therefore the costs for the journey. Looking at the scientists who joined International Congresses we find a personal continuity and therefore a lack of the younger generation. Again one reason can be seen in the limited financial situation of sciences but we must also consider the limited trust in the younger generation's willingness and ability to represent psychology and the GDR in the accepted way.

In 1980 the XXIInd International Congress of Psychology took place in Leipzig (GDR), what was organized by the Gesellschaft für Psychologie. At this Congress F. Klix was elected to be president of the IUPS for the next 4 years - it's common practice to appoint a president from the country hosting the congress. But those events belong to another (final) chapter in the history of the GfP of the GDR, which has to stand over for another day.

Conclusions

The formation and historical development of psychological associations in South Africa and the German Democratic Republic cannot be divorced from the wider political context of these two countries. In South Africa, it was the segregation between black and white. In the GDR, it was the fact that two German states existed.

Membership of the IUPS served an important legitimating function. It was an international body, to which only associations could affiliate. Affiliation therefore served as an important argument to present the discipline inside the country as being accepted at an international level. The acceptance of the association was also seen as acceptance of the national sovereignty.

Finally, both associations went about the business of promoting the discipline in their respective countries, and membership of the IUPS was seen as a way of doing this; of showing that the local body of psychologists has received international recognition.

References

Eckardt, G. (1993). Meinungsstreite - als Mittel politisch-ideologischer Reglementierung der Psychologie in der ehemaligen DDR. *Straeuble, I. and ... Kontinutitaeten und Diskontinuitaeten.*

Hiebsch, H. & Clauss, Guenther (1958). Kinderpsychologie. *1. Auflage. Volk und Wissen Berlin.*

Hiebsch, H. (1958). Aufgaben und Situation der Paedagogischen Psychologie in der DDR. *Paedagogik. Heft 4, 1958*

Hiebsch, H. (1959). Die marxistisch-leninistische Erkenntnistheorie und das Problem des Gegenstandes der Psychologie. *Paedagogik 1. Beiheft.*

Holtzman, W. H. (1976): The XXVth anniversary of the International Union of Psychological Science. *International Journal of Psychological Science. 11(2), 121-136*

Ilefeld,U. (1958). Zur ideologischen Situation in der paedagogischen Psychologie. *Paedagogik. Heft 7, 1958*

Junge, (1962). "Einschätzung der Wirksamkeit des Wissenschaftlichen Beirates" vom 29.05.1962; *Archiv der Parteinen und Massenorganisationen der DDR im Bundesarchiv. Sign.:IV 2/9.04/217*

Klemm, H. (1958). Die Entwicklung der marxistisch-leninistischen Psychologie in der DDR. *Paedagogik. Heft 1-12, Jg. 13, 1958, 512-520*

Kulka, H. (1957). Zur Berufslage von männlichen Lehrlingen in der Metallindustrie. *Wissenschaftliche Zeitschrift der Karl-Marx-Universitaet. Heft 2, Jg. 6.*

Laerz, K. (1996). "Meinungsstreite" (conflict of opinions) in psychology of the former GDR

- in reality "mandatory scientific discussions". *The paper was presented at the 15th CHEIRON Conference in Leiden - 30/08-3/09,1996.*

Laerz, K. (1997). Menschenbilddiskussionen in der deutschen Psychologie während der 50er und 60er Jahre. *Vortrag zum Colloquium anlässlich des 60. Geburtstages von Prof. Dr. Georg Eckardt (Jena). Jena - 6/01,1997.*

Louw, J. (1986). From separation to division: The origins of two psychological associations in South Africa. *Journal of the History of the Behavioral Sciences, 23*, 341-352.

Louw, G. P. (1990). *Die professionalisering van die psigologie in Suid-Afrika: 'n Histories-vergelykende perspektief.* Unpublished Ph D thesis, University of Potchefstroom, South Africa.

Pawlik, K. (1994). Psychology in Europe: Origins and Development of Psychology in German-Speaking Countries. *International Journal of Psychology, 29 (6), 677-694.*

Rosenfeld, G. (1959). Zu einigen Grundproblemen der marxistischen Psychologie in der gegenwaertigen Situation. *Paedagogik. 1. Beiheft 1959.*

Russell, R.W. (1966): The International Union of Psychological Science. *International Journal of Psychological Science. 1(1), 65-72*

Schmidt, H.-D. (1992): Erinnerungen an Kurt Gottschaldt. *Psychologische Rundschau, 43,1992, 252-260.*

Vier, W. (1959): Vorwort des ersten Beiheftes der Zeitschrift "Paedagogik". *Paedagogik, 1. Beiheft.*

CLAPARÈDE IN SPAIN (1900-1936): HIS RECEPTION IN EARLY SPANISH 20TH CENTURY PSYCHOLOGY

Enrique Lafuente, Alejandra Ferrándiz, J. Carlos Loredo (UNED, Madrid)
& Helio Carpintero (Universidad Complutense, Madrid)

To a great extent, the origins of Spanish scientific psychology are linked to the reception of psychological ideas, methods and theories imported from abroad. This is why it is so relevant to study the reception of those psychological movements that have succeeded in shaping the characteristic features of contemporary psychology.

During the first third of the 20th century, the doctrines of Wundt, psychoanalysis, Gestalt psychology and Geneva's school of child psychology were particularly significant in Spain (Carpintero, 1994). The aim of this paper is to establish the reception of the work of Edouard Claparède (1873-1940), a leading member of the school of Geneva, as well as an essential contributor to the development of European applied psychology in the early decades of the present century.

In this paper we will examine Claparède's influence in our country as it is revealed through such indicators as the translations of his books, the references made to his intellectual and institutional achievements, the use of his tests, and the various personal contacts finally leading to the emergence of a large group of Spanish disciples.

TRANSLATED BOOKS

By the end of the 19th century and the beginning of the 20th, the knowledge of French was so extended among Spanish intellectuals that translations were practically unnecessary. The number of Claparède's translated work in this period is for this reason all the more significant, as it shows a keen interest in spreading Claparède's ideas over every social layer (Table 1).

Most of Claparède's translated books are of an applied, mostly educational nature. They include some of the basic tenets of his psychopedagogical thought: among others, the importance of the psychological study of the child and the experimental method for the foundation and practice of teaching, the use of tests for assessing individual abilities, both in the educational and the professional field, and the need to adjust the teaching to the child rather than the opposite.

It is thus noteworthy the absence of translations of other important facets of Claparède's production, such as his contributions on psychophysiology, the psychology of testimony, animal psychology or the psychology of sleep. It should be born in mind that, in those days, Spain lacked the institutional framework required for developing properly that kind of psychological contributions. On the other hand, there was a powerful movement of educational reform going on, represented by such socially important and progressive institutions as the Free Institute for Education ("Institución Libre de

Enseñanza") or the New School ("Escuela Nueva"), that was transforming the educational aspect of the country.

It is no wonder, then, that it was in such an environment where Claparède's ideas became best known.

CLAPARÈDE IN PSYCHOPEDAGOGICAL JOURNAL PUBLICATIONS

In this context, the *Bulletin of the Free Institute of Education* played an essential role. This journal was founded in 1877 by Francisco Giner de los Ríos (1839-1915). It became the organ of expression of the Free Institute of Education, a private educational centre aiming at the promotion and spread of science in every field, including psychology - particularly child and educational psychology. Very soon, the names and ideas of many eminent modern psychologists (Binet, Baldwin, Dewey, James, Romanes, Spencer, Stanley Hall, Sully, Wallon...) found their way into the *Bulletin*, together with those of relevant Spanish authors (Barnés, Besteiro, Caso, Giner, Navarro, Simarro, Viqueira and many others) (Lafuente, 1996).

Claparède's presence in the *Bulletin* is very significant, too. Between 1914 and 1933 several of his papers were there translated, and references to his work were also very frequent in papers by Spanish authors. An extremely long review of the first 17 volumes of the *Archives de Psychologie*, the journal founded by Flournoy and Claparède in 1901, included references to as many as 13 papers by Claparède, many of which were even analyzed in detail (Barnés, 1923). Also noteworthy is a paper on the Institute Rousseau where its founder Claparède had of course a central place. The author of this paper had spent some time at the Insitute and he presented here a first-hand account of the men working and the activities carried out in it (Rosselló, 1923).

Thus, by the 1920s the name and ideas of Claparède were already well known to the readers of the *Bulletin*.

At the beginning of the decade another journal publication was also to join in the task of spreading his psychopedagogical thought in Spain, the *Journal of Pedagogy* ("Revista de Pedagogía"). The *Journal of Pedagogy* was founded in 1922 by Lorenzo Luzuriaga (1889-1965), a pedagogue closely linked to the Free Institute of Education and the intellectual group that was led by the philosopher Ortega y Gasset. The journal frequently included papers on psychological issues, thus providing its readers with an accurate information on recent developments in psychology. Original contributions by such relevant psychologists as Claparède, Piaget, Adler or the Spanish authors Lafora, Mira and Germain, among others, were published (Alfaro & Carpintero, 1983).

Apart from the translation of some of Claparède's papers and the many references made to his work in other authors' papers, the *Journal* very frequently included Claparède's name in its "Books"

and "General Information" sections, where his books where announced and, in some cases, extensively reviewed. At times, even his contributions to other journals and conferences were reproduced or extracted (Anonymous, 1922 and 1924). The journal also reported on Claparède's trip to Madrid in May 1923, where he came invited and gave several lectures (Claparède, 1923).

However, it is in the attention paid to the Institute Rousseau, Claparède's work *par excellence*, where the presence of the Swiss psychologist in the *Journal* may be best shown. The Institute Rousseau had been founded by Claparède in 1912, and soon became a well-known centre for the research of child psychology and for the development of progressive methods of teaching. Many Spanish educators concerned with pedological and educational issues travelled to Geneva for training. The *Journal* minutely reported on different events affecting the life of the Institute. It reflected the various activities and courses carried out, but also the changes of address and even the number of students registered.

The *Journal* included papers by the men and women working at the Institute, and also their books were frequently reviewed (Table 3). Particularly relevant in this connection were the names of Pierre Bovet, director of the Institute; Adolphe Ferrière, director of the Bureau International des Écoles Nouvelles; and, of course, Jean Piaget. Other authors linked to the Institute, whose books were also noted in the *Journal*, were M. Audemars, C. Baudouin, A. Descoeudres, R. Dottrens and L. Lafendel.

Thus, also through the work of his collaborators at the Rousseau Institute was Claparede's voice heard in the Spanish psychological and educational scene.

CLAPARÈDE'S TESTS APPLIED TO MADRILENIAN CHILDREN

An interesting paper published by M. Rodrigo and P. Rosselló shows that Claparède's influence in Spain went far beyond the printed page (Rodrigo & Rosselló, 1923). Both these authors had been trained by Claparède in Geneva. In this paper they reported on the results obtained from subjecting more than 1000 Madrilenian children from 7 to 14 years old to a number of tests designed by Claparède for the selection and orientation of Swiss schoolboys and girls (Table 2).

When comparing their results with those obtained in Geneva and Zürich by Claparede himself, Rodrigo and Rosselló found that the Swiss children scored much higher than the Spanish sample. In their opinion, it is the difference in social extraction that accounts for such results: while public school children in Switzerland might belong to either a high or a low social class, in Spain only low class children were likely to attend to such institutions. Therefore, a new sample of 53 Spanish children, coming now from wealthy families, was then examined with the same tests, the results being now similar or even higher than those of the Swiss children, thus confirming the authors' assumption.

Thus, Claparède's tests were used by the authors as a means to denounce the "spiritual indigence" resulting from the low material life conditions of working class Madrilenian children.

PERSONAL CONTACTS

Therefore, the Spanish reception of Claparède seems to have been carried out through several different though not unconnected lines. In the first place, of course, there were the translations, comments and reviews of his books. Secondly, his work came to be known through the references and publications of his collaborators and the activities carried out by the Institute he had founded. The use of his tests on the Madrilenian school population shows a third line of acquaintance with another facet of his production. But there were also, in the fourth place, close personal contacts that surely enhanced the extent of his influence.

Claparède's trip to Madrid in 1923 has already been recalled. This was not, however, his first visit to Spain. He had already been invited to Barcelona several years before, in 1920, when he lectured at the Summer School organized by Alejandro Galí, the main representative of the Catalonian movement of educational reform. Claparède then visited the Institute of Professional Guidance, and he was so impressed by its functioning that he suggested that the 2nd International Conference of Applied Psychology were held in Barcelona (1921). Some years later, in 1930, Barcelona was again the site of the 6th Conference with Claparède as President of Honour (Siguán, 1981; Saiz *et al.*, 1994).

At least in one more occasion travelled Claparède to Spain. In 1935, the University of Santander, at the northern coast of Spain, invited a number of internationally well-known psychologists (Claparède, Piéron, Janet, Myers, Bühler, Langfeld, Ponzo, Gemelli) to lecture on various applied psychological subjects - particularly medical, industrial and educational. It was meant to facilitate a meeting where the main decisions on the organization of the XI International Congress of Psychology could be taken. The Congress should have been held in Madrid in 1936, but the outburst of the Spanish Civil War (1936-1939) prevented it from taking place.

The closest personal contacts, however, those resulting in most fruitful consequences, took place in Geneva, at the Rousseau Institute, where very many Spanish students went to complete their training. This process of getting in touch with the most advanced psychopedagogical trends of the time through the most prestigious European centre for psychology as applied to educational issues, was to a high extent promoted by an institution of an extraordinary importance in the development of Spanish science in the early decades of the 20th century: the "Junta para Ampliación the Estudios".

The "Junta" was a national committee for the promotion of higher training and scientific research. It was founded in 1907 and was headed by Cajal. The Junta supported the Spanish movement of educational reform, and facilitated the training of Spanish educators in European centres - mainly French, Belgian and Swiss centres. At that time, the Institute Rousseau was playing a fundamental role

in the dissemination, sistematization and coordination of the European "New Schools", and it was the Swiss centre most visited by Spanish students (Carpintero, 1989; Herrero, García & Carpintero, 1995).

The Spaniard Pablo Vila was the first student ever to apply for acceptance at the Rousseau Institute, in 1912. He was later followed by many others. Particularly noteworthy are the names of Mercedes Rodrigo and Pedro Rosselló, the promoters of professional guidance in Spain; Luis de Zulueta, who was in charge of pedagogical studies at the Higher School for Teachers; Juan Comas, a translator of Claparède and other authors of the School of Geneva; and Domingo Barnés, the introducer of pedology in Spain, a man very strongly influenced by Claparède, many of whose works he translated.

The powerful impact exerted by the teaching of Claparède and his collaborators at the Institute Rousseau on Spanish students may be clearly seen in the foundation of a Spanish Association of Former Students and Friends of the Institute Rousseau in 1922. The aim of such an Association was to contribute to the development of the Institute and the spreading of its doctrines. The first activity ever carried out by the Association was to invite Claparède to his 1923 trip to Madrid. The Association also promoted the publication of pedagogical works and the creation of a Department of psychology applied to professional guidance within an Institute of Professional Reeducation for Handicapped Workers (Carda & Carpintero, 1993).

CONCLUDING REMARKS

To sum up, through the first third of the 20th century, Claparède's thought became very well known in our country through the translations of his books and papers, the various accounts of his ideas and activities, and the personal contacts made possible both by the trips of the Swiss psychologist to Spain and by those made to Geneva by a high number of Spanish psychologists and educators willing to become his disciples.

The reception of his ideas had a clearly psychopedagogical character. It was grounded on a wide movement of national renewal aiming to transform Spanish society through education, and to provide both education and society with a firm, scientific foundation.

It was not, however, just a passive reception consisting in a mere acquaintance with his views. Claparède's contribution was of a basically applied nature, and so was understood by his Spanish followers, who tried to use his ideas and tests for coping with some of the social and educational problems raised in their own country.

Neither was it an isolated reception. Claparède's figure was often seen on the ground provided by the Rousseau Institute, his great institutional achievement. Thus, his name frequently appeared in connection with other names of the Geneva School. Their reception in Spain, in turn, was likely favored

by the prestige they borrowed from the great Swiss master. The existence of a high number of disciples grouped in a Spanish Association of Former Students and Friends of the Rousseau Institute evidences the influence exerted by this remarkable Claparedian institutional realization.

Finally, it should be noted that Claparède's psychopedagogical presence in Spain was maintained through those topics that, thanks to his legacy, were later developed by Piaget (Carpintero, 1985; Caparrós, 1982; Peiró y Grau, 1991), whose name stands together with Claparède's as a key reference for a new way of understanding education in our country in the first decades of the present century.

REFERENCES

- Alfaro, I. & Carpintero, H. (1983), "La psicología en la educación. Un análisis a través de la Revista de Pedagogía". Revista de Historia de la Psicología, 4, 197-223.

- Anonymous (1922), "Informaciones. Los métodos de educación y la psicología aplicada". Revista de Pedagogía, 8, 351-353.

- Anonymous (1924), "Informaciones. La psicología y la escuela activa". Revista de Pedagogía, 26, 65-66.

- Barnés, D. (1923), "Suiza. Los Archivos de Psicología de Ginebra". BILE, 47, 280-282, 311-315, 338-344.

- Caparrós, A. (1982), "Piaget y sus orígenes científicos y filosóficos". Revista de Psicología General y Aplicada, 37, 285-307.

- Carda, M.R. & Carpintero, H. (1993), Domingo Barnés. Psicología y educación. Alicante: Instituto de Cultura Juan Gil Albert.

- Carpintero, H. (1985), "Algunos antecedentes de la obra de Piaget. Un capítulo de psicología cognitiva antes del cognitivismo". In J. Mayor (Ed.), Actividad humana y procesos cognitivos. Madrid: Alhambra.

- Carpintero, H. (1989), "La psicología y la Junta para Ampliación de Estudios: Una primera aproximación". In J.M. Sánchez Ron, 1907-1987: la Junta para Ampliación de Estudios e Investigaciones Científicas 80 años después. Madrid: C.S.I.C., 2 vols.

- Carpintero, H. (1994), Historia de la psicología en España. Madrid: Eudema.

- Claparède, E. (1923), "La psicología de las aptitudes". Revista de Pedagogía, 18, 223-227.

- Herrero, F. Garcia, E. & Carpintero, H. (1995), "Psicopedagogía en España (1900-1936). Becarios españoles en centros europeos". Revista de Historia de la Psicología, 16, 181-200.

- Lafuente, E. (1996), "El pensamiento psicológico de Francisco Giner de los Ríos". In M. Saiz & D. Saiz (eds.), Personajes para una historia de la psicología en España. Madrid: Pirámide.

- Peiró, J.M. & Grau, R. (1991), "La presencia de la obra de E. Claparède en la psicología de Jean Piaget". Revista de Historia de la Psicología, 12, 87-121.

- Rodrigo, M. & Rosselló, P. (1923), "Revisión española de los tests de Claparède (Primera Serie)". Revista de Pedagogía, 15, 81-92.

- Rosselló, P. (1923), "El Instituto J.J. Rousseau. Sus hombres. Su obra. Sugestiones que para nuestro país pueden derivarse de su funcionamiento". BILE, 47, 139-145, 169-174.

- Saiz, M., Saiz, D., Mülberger, A., Alfaro, M., del Blanco, R., Capdevila, A., Peralta, A. & Trujillo, J.L. (1994), "La II y VI Conferencias Internacionales de Psicotecnia celebradas en Barcelona. Algunas de sus repercusiones en el ámbito social e institucional". Revista de Historia de la Psicología, 15, 227-237.

- Siguán, M. (1981), La psicologia à Catalunya. Barcelona: Edicions 62

TABLE 1
CLAPARÈDE'S BOOKS TRANSLATED INTO SPANISH

- 1907. La asociación de ideas. Trad. D. Barnés. Madrid: Jorro.
- 1911. Psicología del niño y pedagogía experimental. Trad. D. Barnés. Madrid: Beltrán.
- 1921. Psicología del niño y pedagogía experimental. Trad. y estudio preliminar D. Barnés. Madrid: Beltrán.
- 1923. La escuela a la medida. Trad. M. Rodrigo. Madrid: Espasa-Calpe.
- 1924. La orientación profesional. Madrid: La Lectura.
- 1926. La escuela y la psicología experimental. Trad. y estudio preliminar de L. Luzuriaga. Madrid: Pub. Revista de Pedagogía.
- 1927. Cómo diagnosticar las aptitudes de los escolares. Trad. J. Xandri Pich. Madrid: M. Aguilar.
- 1927. La educación funcional. Trad. M. Rodrigo. Madrid: Espasa-Calpe.
- 1933. La psicología y la nueva educación. Trad. y prólogo de Juan Comas. Madrid: Pub. Revista de Pedagogía.
- 1936. El sentimiento de inferioridad en el niño. Trad. M.L. Navarro. Madrid: Publicaciones de la Revista de Pedagogía.

TABLE 2
CLAPARÈDE'S TESTS APPLIED BY M. RODRIGO Y P. ROSSELLÓ

1. **MEMORY FOR WORDS**: 15 words are read to the subject, who is then asked to write down as many as he can recall in 1 minute.

2. **DRAWING**: the subject is given 3 minutes for drawing a cat running after a mouse, a bottle and a coin-sized circle.

3. **SPEED OF WRITING**: the subject is given 1 minute for writing the sentence "el sol nos alumbra" as many times as he can.

4. **COMBINING LETTERS**: the subject is given 1 minute for making as many combinations as possible with the letters a, b, c, d.

TABLE 3
BOOKS BY AUTHORS LINKED TO THE ROUSSEAU INSTITUTE, REVIEWED IN THE JOURNAL OF PEDAGOGY

M. AUDEMARS Y L. LAFENDEL
- La Maison des Petits de l'Institut J.J. Rousseau, 1923 (M.L. Navarro)

C. BAUDOUIN
- El alma infantil y el psicoanálisis, 1934 (V. Aranda)

P. BOVET
- El instinto luchador, 1921 (M.L. Navarro)
- El psicoanálisis y la educación, 1922 (M.L. Navarro)
- La psicología y la educación por la paz, 1928 (L. Serrano)
- L'instinct combatif, 1928 (J. Comas)
- Le sentiment religieux et la psychologie de l'enfant, 1929 (J. Comas)
- La paz por la escuela, 1932 (R. Lago)
- La obra del Instituto Rousseau (Veinte años de vida, 1912-1932), 1934 (M. Medina Bravo)

E. CLAPARÈDE
- La escuela a la medida, 1923 (M.P. Oñate)
- La orientación profesional, 1924 (E. Mira)
- L'éducation fonctionelle, 1931 (M.L. Navarro)
- Le sentiment d'inferiorité chez l'enfant, 1934 (J. Comas)

A. DESCOEUDRES
- Le developpement de l'enfant de deux à sept ans, 1921 (M.L. Navarro)

R. DOTTRENS
- Les études pédagogiques à Genève, 1835.1933, 1933 (J. Comas)

A. FERRIÈRE
- L'autonomie des escoliers, 1921 (L. Luzuriaga)
- Les tendances actuelles de l'éducation en Suisse, 1921
- L'école active, 1922 (M.L. Navarro)
- Transformemos la escuela, 1924 (M.L. Navarro)
- La educación autónoma, 1926 (L. Luzuriaga)
- La coéducation des sexes dans ses rapports avec la crise de la famille et la transformation de l'école, 1926 (A. Ballesteros)
- El alma del niño a la luz de la ciencia, 1928 (F. Sáiz)
- L'école sur mesure à la mesure du maitre, 1931 (L. Santullano)
- L'Amérique Latine adopte l'école active, 1931 (L. Luzuriaga)
- La educación constructiva. El progreso espiritual, 1932 (A. Ballesteros)

J. PIAGET
- Notes sur les types de description d'images chez l'enfant (con P. Rosselló), 1922 (M. Rodrigo)
- La representation du monde chez l'enfant, 1929 (C. Sáiz-Amor)
- Le jugement morale chez l'enfant, 1932 (J. Comas)
- La causalidad fisica en el niño, 1934 (H. Almendros)
- El juicio moral en el niño, 1935

Oskar Pfister's transatlantic psychoanalytic connection.

David D. Lee
Dept. of History, UCLA
davelee@ucla.edu

Historians make clear the myriad of ways in which the origins, growth and development of psychology and psychiatry as independent disciplines were conditioned by the political and social life of their national origins. Our comprehension of the very nature of the psychological enterprise has been reshaped in recent years by authors such as Mark Micale, Roy Porter, and Hannah Decker each of whom have argued for an appreciably more nuanced and contextualized narrative of this past. A quarter of a century ago Carl Schorske analyzed Freud's Interpretation of Dreams for what he termed its "counterpolitical ingredient in the origins of psychoanalysis." These authors, and others like them, believe that political life, artistic production, and philosophical discussions fundamentally shape related intellectual products. In the nineteenth and twentieth centuries culture has developed primarily around the nation-state. In this light the interaction between national cultures is of great interest to historians of psychology, psychiatry, and psychoanalysis.

National schools of psychology are defined not only by those who led them (e.g., Kraepelin or Charcot), but we claim also to comprehend a dynamic within each nation as they contributed to the monumental task of grasping human emotional and behavioral dynamics. We accept as a truism that each nation has made a definable contribution to modern psychology, yet we simultaneously realize the interwoven nature of all intellectual endeavors. This paper seeks to identify some threads of transatlantic psychoanalytic history using published and archival materials in the expectation that such endeavors help clarify national contributions as much as they impress upon us the psychology's scientific, international character.

The history of psychoanalysis provides a particularly useful example for such a study. Written initially by practitioners and followers of Freud, this hagiographic narrative was seriously critiqued in the 1960s by men such as Henry Ellenberger. Subsequent generations of historians such as Han Israels and Ernst Falzeder (not to mention Peter Swales) have increased the pressure on traditional psychoanalytic history by uncovering deception, half-truths, and convenient omissions. This re-evaluation of the origins and nature of Freud's psychology is an important part of our comprehension of the history of psychology. This paper seeks to further our comprehension of the nature of international cross-fertilization and identify the most accurate narrative of psychoanalytic history.

Scholars in many fields struggle to comprehend the nature and degree of international intercourse that contributed to each nation's development. Most remarkable in this regard is the rapid and substantial contributions of U.S. researchers and thinkers from the end of the Civil War. From William James to Oliver Sachs the rise of the United States' economic and political power in the last third of the nineteenth century led most directly to her astounding psychological contributions and her prominent position today. This process did not eclipse European research and practice, but rather stimulated further Old World activity as we shall shortly see.

This story includes a varied cast of characters featuring Sigmund Freud and his follower and friend Oskar Pfister, as well as a host of European and American psychiatrists and psychoanalysts. Pfister (1873-1956), for those unfamiliar with him, was a Swiss Protestant pastor who published dozens of books and articles on psychoanalytic pedagogy, religious psychology, and psychoanalytic theory and practice during the first half of this century. His work was shaped in part by the work of William James and his students, James Leuba and Edwin Starbuck.

Pfister in turn provided a bridge between the two continents during his career with his formative textbooks and research publications. As the author of the first popular psychoanalytic textbook, Die psychanalytische Methode (1913, 1921, 1924, 1927), issued in English as The psychoanalytic method (1915, 1917, 1919), Pfister's interpretive views spread beyond his pastoral and pedagogical fields to medical practitioners. Influential psychologists such as William A. White in the United States and Donald Winnicott in Great Britain credit Pfister with drawing them into the psychoanalytic fold. In his 1917 *Journal of the American Medical Association* article "Psychanalysis and the practice of medicine," White strongly relied upon Pfister's presentation and interpretation of Freud's work to justify his use of analytic technique in medical situations. White, and other Americans such as Smith Ely Jelliffe, found Pfister's lack of emphasis on sexuality, his friendliness toward religious belief, his clear presentation of Freudian principles, and his bold, suggestive applications of analytic thought to everyday psychiatric problems and the schoolroom compelling. In Britain, Donald Winnicott also found his way to psychoanalysis through Pfister. In a rare auto-biographical piece from 1961 Winnicott recalled that as a student he "found that from being a good dreamer, he had ceased to be able to recall his dreams." When the popular French philosopher Henri Bergson proved "quite irrelevant" he tried Pfister's Psychoanalytic Method. Winnicott describes the revelation in Biblical terms: "It must have been like the butler introducing Pharaoh to Joseph." Deeply impressed with the book, Winnicott began his analysis with James

Strachey shortly thereafter and launched his pioneering career in child psychology.

In the decade after the First World War Pfister's authority in psychoanalytic pastoral and pedagogical psychology was unquestioned. Patients from the United States traveled to Zürich especially for treatment with him. And it is with one of these patients that our story begins. Known in the Freud-Pfister by the pseudonym "A.B.", I shall here preserve this patient's anonymity at the request of those at McLean Hospital (Belmont, Massachusetts, USA) kind enough to grant me access to his medical records.

A.B. was born in a major U.S. city at the turn of the century to wealthy German-Jewish parents. Mental illness was not unknown within the family. Though it is not known whether his parents emigrated to the United States, the family business involved the manufacture of goods commonly associated with central Europe; family trips to Europe were made with some frequency. A.B. was a bright child who exhibited few symptoms of mental disorder outside of a particular asociability. In his mid-teens he began to develop a "marked hypersensitiveness to the opinions of others along with a distinct feeling of inferiority." After graduating from a prestigious American university in 1922 A.B.'s illness began to take a turn for the worse. He believed with increasing certitude that he was a coward and that his cowardly nature manifested itself in all his actions. His family physician consequently referred him to Dr. Pierce Clark.

At that time Clark was best known for his pioneering psychiatric works such as Neurological and mental diagnosis: a manual of methods (1908) and Clinical studies in epilepsy (1917). Later Clark translated Anna Freud's Introduction to the technique of child analysis and published several pioneering psychohistorical works. In 1922, however, Clark was one of a small group of American psychiatrists who endeavored to make use of Freud's clinical theories at a time when they were quite exciting, but equally unproved. Thus began A.B.'s first psychoanalytic experience.

Like William White, Pierce Clark relied on the English editions of Pfister's The psychoanalytic Method (1915, 1917, 1919) and Psychoanalysis in the service of education (1922) for analytic guidance and evidently suggested that A.B. also read these works. A.B. seems to have been a man concerned with religious issues, though I believe that Pfister's interest in the psychology of art may have attracted A.B.'s attention as he was planning, and in fact eventually pursued, university studies in art.

Under this guise he traveled to Zürich, studied at the university, and undertook an analysis with Oskar Pfister. A non-medical, or lay, analyst, Pfister understood the importance of consulting psychiatrists in matters concerning mental illness. It is therefore no surprise that he asked his friend and colleague Eugen Bleuler to assess A.B.'s

mental condition and analyzability. Bleuler, as many of you may know, was Chief of the famous state psychiatric institute Burghölzli in Zürich where, after coining and defining the term *schizophrenia*, he hired Carl Jung as his assistant and transformed the psychiatric landscape by supporting and practicing Freud's theories in the years before the First World War. Bleuler's expert opinion was that A.B. was seriously disturbed, yet could well benefit from analytic treatment by Pfister. As the examination concluded, Bleuler wrote to Pfister, "He then asked me what I would write [you]. I told him that he was indeed disturbed and needed a proper education, which was still be possible if he would acquiesce to you in all matters."

For two years Pfister worked with A.B. though evidently with little success. Pfister corresponded with Freud about the case and eventually Pfister convinced Freud to take A.B. as his own patient. Freud had previously stated that schizophrenia was not amenable to psychoanalytic treatment for the most basic reason that the delusional nature of the illness prevented the kind of transferential bond necessary for analytic progress. Freud was intrigued and/or charmed by A.B. sufficiently to take him into treatment. In December of 1924 Freud wrote to Pfister, "Do not worry about your young American. The man can be helped." Yet about a year later Freud reported that, "things are going very strangely." He began to worry that A.B. was beyond any kind of psychiatric assistance and he would have to end treatment, "but there is something touching about him which deters me from doing so; the threat of breaking off the treatment has made him gentle and amenable again, with the result that at present a good understanding prevails between us." This display of force by Freud is evident also in his directive to A.B. to end his compulsive masturbation and is not altogether uncharacteristic of his technique.

Freud worked with A.B. from 1925 until 1930, during which time he corresponded regularly with A.B.'s parents as well as Pfister. To A.B.'s mother Freud wrote in 1928:

> I have no right to withhold from you that the diagnosis in [your son's] case is paranoid schizophrenia. You have every right to emphasize that such a diagnosis doesn't mean much and doesn't help clarify the uncertainty of his future. Jean Jacques Rousseau was also such a case and only slightly less abnormal sexually.

By dismissing the diagnostic value of psychiatric terminology, emphasizing his investment in A.B.'s cure, and comparing this mother's disturbed son to Rousseau, Freud sought to reassure, but also demonstrates some of his affection for the youth. Like so many of his patients, A.B. subsequently appears in several of Freud's papers on technique, most notably his 1927 paper on fetishism. Freud uses A.B.'s case to demonstrate that "the disavowal and the

affirmation of the castration have found their way into the construction of the fetish itself." Freud eventually did end treatment, passing him on to his close friend and colleague Ruth Mack Brunswick, an American psychiatrist who looked after many of Freud's American patients in Vienna. Brunswick later supported psychoanalytic work suggesting that certain schizophrenics could benefit from analysis. This analysis, however, was aborted within a year and although A.B. abandoned Europe for the U.S. in the fall of 1932, he did not abandon his ties to psychoanalysis as documentary evidence indicates that he worked with A.A. Brill in New York as well as other American analysts.

A.B.'s condition seems to have been stable upon his return to the U.S., but within a year he had regressed to the point of attempting suicide. As early as 1926 Freud expressed to Pfister his fear of just this outcome:

> What weighs on me in his case is my belief that, unless the outcome is very good indeed, it will be very bad indeed; what I mean is that he would commit suicide without hesitation. I shall therefore do all in my power to avert that eventuality.

In April of 1933 A.B. plunged a knife into his breast missing his heart by less than an inch. He actively fought those who tried to save his life. His wounds took eight weeks to heal. For some time thereafter he continued to work psychoanalytically with Brill and later Herman Nunberg, but his worsening condition made this increasingly difficult. Consequently he was institutionalized in February of 1935.

At this date A.B.'s psychoanalytic treatment ends, though its influence on his character and his interpretation of his illness continued until his death. Many years later a McLean psychiatrist noted that A.B. "is also fond of quoting from his personal conversations with Freud, whom he venerates exceedingly." Of interest to historians of psychiatry, however, is his continued treatment. Despite A.B.'s faith in Freud, even analysts could no longer hold out the hope of a cure. Brill wrote to A.B.'s father in March of 1935: "There is no possibility that analysis will do him any more good, as much as I think of analysis and believe in its efficacy in other cases."

At McLean A.B. was given several treatments, including electro convulsive therapy (ECT). Shortly before the outbreak of the Second World War, an attending psychiatrist observed:

> He was always trying to find the real source of his neurosis. Now he has the impression it would be like somebody trying to find the oil lamp in a burning house, which caused the fire. He thinks even if he could find the lamp - the real source - the house still would burn.

At McLean A.B. was one of the first participants in a new Insulin Coma Therapy program. These treatments provided "temporary, slight improvement." In 1949 he was subjected to a frontal lobotomy (topectomy) which seems not to have improved his condition appreciably. It is remarkable to find a case history in which the patient has participated in each of the major psychiatric revolutions of our era, psychotherapy, chemical treatment, electrotherapy, and surgical intervention.

A.B.'s psychiatric journey from Freud's couch to an operating table in a Boston suburb is not a happy one. By all accounts he seems to have generated a great deal of sympathy for his condition. Anna Freud recalled in 1972, "I remember this patient very well, and also my father's struggle to understand and help him." Oskar Pfister's role in this drama was that of catalyst. As a popularizer of psychoanalytic thought, particularly psychoanalytic pedagogy, Pfister's published writings reached a much wider audience than psychiatric literature. For example, Pfister's 1913 psychoanalytic textbook Die psychanalytische Methode was not the first such textbook; Eduard Hitschmann's Freuds Neurosenlehre, more technical manual than textbook, appeared in 1911. But it was Pfister's tome, and others like it, that helped linked psychoanalysis to mainstream U.S. intellectual thought and culture. Pfister also toured the United States from coast to coast in the 1930s lecturing to psychoanalytic societies, church groups, and teacher's associations. Pfister's psychoanalysis, perceived to be remarkably compatible with American (i.e., utilitarian) notions of psychology, struck a responsive chord.

English translations of Pfister's publications, such as Psychoanalysis in the service of education (1922), Some applications of psychoanalysis (1923), Love in children and its aberrations (1924), and Christianity and Fear (1948), kept Pfister's religiously-influenced psychoanalytic views in American and British discussions of religious psychology, pedagogy, and psychoanalysis. Pfister's legacy can be seen in the numerous doctoral dissertations and theses written on his life and work. The Oskar-Pfister-Tagung held in Zürich in 1973 to commemorate the centenary of his birth was organized and funded largely by Americans, rather than Swiss. His influence is most manifest in the annual American Psychiatric Association's Oskar-Pfister-Prize awarded to individuals such as Oliver Sachs and Peter Gay who research the psychology of religious experience and its history.

The great transatlantic dynamic in psychoanalytic history is the forced immigration of analysts from Nazi Germany and conquered Europe before and during the Second World War. Here we find an earlier dynamic at work in this history as well. Oskar Pfister's efforts to popularize and clarify Freud's thought bore fruit in the product of his own work, and in that of Freud's as well.

SÁNDOR LISZNYAI

THE AFTER-WAR HISTORY OF THE HUNGARIAN INDIVIDUAL PSYCHOLOGY MOVEMENT

THE AFTER-WAR HISTORY OF THE HUNGARIAN ASSOCIATION FOR INDIVIDUAL PSYCHOLOGY

This paper is concerned in the history of the Hungarian "Association for Individual Psychology" (henceforth abbreviated "MIPE" according to the Hungarian abbreviation) - focusing on the history of the association after the Second World War until its dissolution in 1950 and with especial regard to the outcomes, impacts on the present Hungarian life of scientific psychology caused by its activities.

There are very reliable source materials and writings in Hungarian on the topic, but the most important is the histography by Emma Gráber, who was an active member of the association until its dissolution and then wrote down the story of the movement which was completed by 1968. She could not publish her histography, of course, due to the actual political regime, but in her heritage it was found and now it is in the property of the in 1988-89 reorganised MIPE.

The other important documents of the association are available as archivalia in the National Record Office of Hungary.

INDIVIDUAL PSYCHOLOGY IN HUNGARY - INTRODUCTION

The active period of the Hungarian individual psychology movement was a very short one, lasted only two and a half decades from the foundation of the association in 2 Nov. 1927 to the "closing of the cash book" in 31 May 1950.

This short term was not enough to achieve spectacular scientific successes, but on some domain of psychology - e.g. educational guidance - it created the scope for applied psychology in a larger, society scale. First a few words about the most important personalities and dates of MIPE.

The president of the association was *István Máday*, from its foundation until its dissolution. He was a doctor graduated at Prague German University and attended several Monarchic universities - he was acquainted personally with Alfred Adler, and he was a member of the "Wednesday seminars" - then joined the Psychoanalytic Society in Vienna, and later, after the rending between Adler and Freud he joined the intellectual society organised around Adler.

When he moved back to Hungary he get a scholarship at the University of Debrecen, and when Adler requested Jenô Rácz to establish the Hungarian Association for Individual Psychology in 1927 he was elected to be the president of the organisation.

Máday was an apolitical psychologist and this attitude was a strict principle not to take sides in political issues. This lead to an interesting situation after the 1948-er Hungarian changeover, where politics infiltrated the civil spheres of life either.

The general secretary of the association was *Jenô Rácz*, Dr attended lectures of Alfred Adler in Vienna and when back to Budapest he joined the readers' society on individual psychology. This 'readers society' conducted by Máday was the predecessor of the MIPE of which foundation Rácz put forward a proposal. His motion was adopted and he become the general secretary of the association and held that position until his early death in Dec. 1933.

His activities was important not only for his official function but his 'institutionalisation' - he run the first educational guidance centre in Hungary with the aims of prevention and therapy established in 1928. He founded and edited the Hungarian journal for individual psychology called "Lélek és Jellem" (Soul and Character) in 1933 - but with his early death this journal also terminated its functioning.

Sources and their criticism

The most important resource material is the histography written by Emma Gráber, who was an active member of the association until its dismissal in 1950. Her material contains referrals upon itself, and the activities of Emma Gráber - and as a result of this, it can help to compose the criticism either.
She wrote portraits (retrospectively, as the whole material itself, in 1960) of the more prominent MIPE members, including herself. The portrait of Emma Gráber is the most confused of all the others, the narrative line can be hardly followed in it. Considering the format it is full of errata, inserts. The other individualpsychologists are described as less contradictory or ambiguous personalities than herself. The question that whether it is due to the process of self-reflection or to the folkloring, stereotyping, selecting way of remembering contemporaries, career fellows is out of the scope of this paper. Looking for "weak points" in the self-portrait and the biography of the other prominent MIPE members (Prof. Máday, Takács, Dr Rácz) is a sort of inspection I can not take on, and the relevant issues to the present project are the issues of existence of historic events in psychological science rather than personal evaluation, truth content of them. The information presented below is based in a great part on her work.

INDIVIDUAL PSYCHOLOGY AND EDUCATIONAL GUIDANCE

The network of educational guidance services and IP

In Hungary the institutions of educational and vocational guidance are still the most important base and intellectual forum for the national psychological ideas and experts. These offices, centres for psychological counselling are similar to one another in some respect, but the origin of them is surprisingly distinct - there were parallel existing starting points in the history of international psychology.

Nowadays in Hungary - and before the 1989 changeover of the social and political system - the most important impacts of the functioning of these organisations are not from the domain of IP but from psychoanalysis, the Rogersian approaches and eclectic, pedagogy-oriented psychological theories function as the base of professional work. Before the Second World War, however, and a few years after the "deliberation of the country" the IP Association had the most important impact upon educational psychology and counselling.

One might think that the extra-school services and help for parents to manage special stress and study concerns are the inevitable consequences of the problems of "modern man" and his/her children. But the social context of the age of the after-world war I Hungary carried in itself the need for such services.

There were only a few children in a family - especially in big towns - so the mother could obtain less experience with children than before, and the children themselves were more vulnerable and did not attain the implicit rules of social interactions *in vivo* - comparing to the situation when seven or eight children had lived in the same living space. Most mothers went to work so they had a smaller educative impact on their children. As Máday mentioned in his book titled Individuálpszichológia, "Due to the present existential problems the parents manage their children in a nervous and impatient way which may cause behaviour problems or illness even in the early childhood. The spreading social problems around children - alcoholism, prostitution and other immoral manners - can push children towards maladaptive models of coping."

Máday's opinion on educational psychology: the most important social problem is the changing role of the school as institution. It may help acquire basic or even sophisticated knowledge, but not the skills about how to integrate into human community. This side of education remains a task of the parents: who are inexperienced and short of time to manage this function. Teachers at schools are also unable to handle the too large number of students and even not qualified enough (i.e. before the Second World War) to undertake on this task (Arató, Kiss, 1991).

The first mentioned different origins of educational guidance services were present in Hungary, too. The first types of such services were guidance offices being concerned in the choices of career - e.g. at the Budapest Pedagogical Seminar conducted by László Nagy.

The first preventive and therapy oriented counselling service of the country was organised by Jenô Rácz at one of the centres of the National Child Prevention League. Máday himself lead another counselling centre established in 1929 also in the framework of the work of the National Child Prevention League.

After 1930 the number of the educational counselling offices increased - as a result of some well identifiable reasons.

The preventive and therapeutic - 'helping' - methods were well elaborated and worked out by those years. Testified by the works of Máday and other IP representatives, the work in these centres was a very modern and unique psychological approach in international relations, either.

The other reason was the circuit letter of the "Department VII". This was the Public Education Department of Budapest, and these letter (coded 17/1931) was delivered to every school and emphasised the importance of such services, and one of Máday's papers was attached to it describing the process and possible benefits of the process. So this opportunity providing these free helping services was well known by both parents and teachers, and could become a popular way of solving frequent educational problems.

THE DISSOLUTION OF THE ASSOCIATION AND ITS SCENARIOS

Through misunderstandings - hidden cross-purposes?

Another parallel between the lot of the MIPE and the Psychoanalitic Society is the quality of the debates on their legitimacy. In relation with psychoanalysis there is a good analysis in Hungarian (Harmat, 1992).

The conclusion of these observations is that the offences did not allowed the psychoanalytic experts to defend their ideas on their "home ground"; the scientific reference was substituted by the "directives". The statements of the psychological research were surprisingly misunderstood - a deformed argument, not free from extremes came into existence, with the participation - voluntary or forced - of both sides. These irrealistic, virtual pseudo-scientific context was the reference and the normative base of science that days.

The historians do not miss the opportunity to state that these "debates" were only means to create causes to dismiss these institutions. This can be true - but the psychoanalytic experts defended themselves and their ideas on the same ground and in the context as the politicians on the other hand. In this false - or, at least, not compatible with the original psychoanalytic concepts - system of concepts was the framework of the arguments also for the aligned psychoanalysts.

This situation is easier to elaborate in the case of the MIPE. This organisation was not so well-known as the psychoanalytic one, so it was more complicated to place in the map of the actual political life, and its conceptual framework was not as obvious, either. It was a more difficult task to describe an organisation as a hostile one when it was not clearly defined. Due to this conditions characteristic misunderstandings appeared: after the name of the founder, Adler (as a way of "free association") the organisation was mentioned to be connected to Max Adler, the reputed Wiener social-democrat politician - who was a really interesting historical person as well, but not in any relation with Alfred Adler and the Individual Psychology movement. Another similar misunderstanding is reported in the histography of Bíróné:

"Máday's another assumption was a wrong one, however. He believed that we are pushed into the background because the socialists misunderstand the name of the organisation: they translate the first half of the name [i.e. individual] as "private", and assume it as opposed to the system (social contra individual)."

Emma Gráber believed Máday's idea to be wrong because she thought that "the feeling of community could have been more dangerous to a dictatorial system than the privacy itself". This argument is at least wrong in a logical sense. However, when it is examined by the above mentioned historical approach, and a distinct, virtual context of the debates is postulated, this argument cannot be

eliminated. Máday's "mistake" can be elaborated as a cognitively reasoned contradiction: the name of the organisation is absolutely irrelevant and - when it is a real scientific issue - may not be criticised, but in this mixed, at the same time political and scientific context it can play an important role.

As the arguments and offences against the IP Association were "routinish and apathetic side-blows of the ones against the psychoanalysts" (Harmat, 1992), the public debates in scientific journals and at other intellectual forums showed this phenomenon. Since the offence against the psychoanalysts and the psychoanalytic concepts in general were launched in a concentrated way and in one of the most powerful mouthpiece of the Communist Party and were executed by two trained experts (Tariska and Pálóczi-Horváth, see below), the attack against the IP ideas was done *á propos,* by joining an existing debate of quite scientific characteristics and hijacked it towards a political, ideological struggle.

Education For Peace

This existing debate was begun by Maximilian Bing, who published an article in the first after-war number of *Orvosok Lapja* (Doctors Magazine), and raised the topic then cultivated by American psychologists called 'Education For Peace' - referring to the possible and demanding functions of psychology in developing and maintaining a peaceful and not hostile society.

He stressed that the Hungarian psychologists should take into consideration this aspect of their work. István Máday answered to this question had come up, and stated that the IP Association at one of its first sessions resolved working out such a program.

Máday's train of thought in this context was an original psychological one though it contained wider, social influences. The interpretation of the early after-war Hungarian society was in intellectual circles a notion of disappointment and irritated promises that such things that destroyed the nation as the Second World War could not happen again (see for example the first introductory after-war article of *Világosság* titled "Without Lies").

He argued that the psychological ideas the American psychologists stated to be the base of an educational program to train peaceful people were a strange intellectual shift: for example, the 'Education For Peace' was built up on the concepts of S. Freud, among others. Freud himself saw no possibility to establish a society without *compulsives*, and considered the destructive instinct as one of the basic determinant of Man. Máday believed that the IP framework of concepts would be a more suitable setting for such an educational program.

At this point joined the debate the authors with political intentions. Characteristically, and Helfmann et al. (in the same magazine, *Orvosok Lapja*) did not spare either sides of the disputers: stated that neither Máday and the authors he cited, nor Bing having mentioned the American psychologists could not catch the point - moreover, they spread dangerous and misleading ideas. Peace and war are not questions of psychology and education but economic means - warned the authors.

The hijacked orientation of the argument could not been reversed. Máday and Bing (this time jointly) tried to show that the notion of war is a complex problem - should be eliminated only with complex measures, utilising economic, psychological and other approaches.

The standard source book on the history of the Hungarian psychoanalytic movements bears a grudge against Máday and his collaborator in connection with this story (Harmat, 1992). It mentioned that it was at least "not with style" to adduce sentences from Stalin in defending IP and psychology in general. However, at that phase of the attempted transition into an ideological dispute from a real scientific one, some compromise could seem useful to revert the orientation *back* to the ground of science. Although it was a witty move, the debate simply terminated - the IP movement and its educational implications did not merit attention from the standpoint of political circles.

Direct actions - the psychoanalytic movement

"Freudism... is the focus of bacilles of all the bourgeois fallacious ideas; a possible breeding ground for fascism; it bears and spreads bourgeois idealism; clothing with the illusion of a scientific character the irrationalism of the disintegrating capitalism."

This was the introductory quotation of an article appeared in the Forum 1948 Vol. 3, the author was Vilmos Tariska. The quotation is from an author published another article in the same journal a few weeks before, and Tariska added some criticism (!) to it.

While the IP authors and association was offended by an accidental way only, psychoanalysis was insinuated in a volume of the Forum, where a conscious and well organised action was lashed out against the psychoanalytic world of science. This action was "clothed with the illusion" of a philosophical debate: two authors and other contributors "argued" on the nature and importance of psychoanalysis. In these articles the authors reflected to one another's opinion - while stressing that psychoanalysis is a defected and irrealistic trend of thought. The differences of the attitudes of the two above mentioned authors, for example, are quite slight considering that they were declared philosophical treatises: Tariska stated that psychoanalysis is really a secondary product of the capitalist nihilism and social context (as Pálóczy-Nagy concluded first), but in addition he thinks that its idealism plays an active role maintaining that social context.

Mixing ideology with psychological concepts: resulting a new psylosophy of psychology

"The official Hungarian scientific life in some fields has not reached the level of the bourgeois stage - so representatives of the bourgeois psychology - for example freudism - were avoided successfully from universities. (At several other scientific areas more closely connected to the power structure of the society such as sociology the Hungarian way of development leaped at once to the fascist level omitting bourgeois approaches).[...] From the analytic psychological trends the Jungian psychology was the one, considering its Weltanschauung, general approach, that contained the most fascist-like and reactionist elements."

The classic Marxian historicism postulated the progress containing the feudal--bourgeois--fascist stages, and the above mentioned author (in Társadalmi Szemle [Social Review] 1948 January, by György Nádor who was the permanent recensist of that journal) designed another parallel development stage sequence, i.e. "ancient psychology"--freudism--jungism. Ancient psychology (expression quoted from the author) here meant the classic empirical approaches based on empiricist philosophical theories.

This historicist way of interpretation shows one of the most difficult problem when defending psychology those days. It can be elaborated as a strong demand for defining social functions and roles - because historicism means a contented and predeterminated sequence of events, where all players have to choose a role. Psychology - by definition - is a social enterprise: its function in society is not defined a priory, as for philosophy or health sciences, for example. There were no strong reference points to be connected: most of the psychological trends had came from Western countries. However, one could find evidence that the enterprise of psychology - especially IP - had been a bonafide social movement before the war: but this kind of legitimisation was not enough even in the case of Communist Party (MDP) members.

Psychoanalysis and Individual Psychology

The common history of the two above named movement is remarkable from the approach of this paper i.e. from a political point of view.
The historical data of the psychoanalysis is not used here only as a heuristic model but in this case (and in general when two related object are impacted upon the same effect) through interpretation it is an essential thing to analyse parallel these institutionally connected scientific movement.

The beginning of their common story is well-known in the standard literature of psychology: individual psychology is a renegade movement, a "stepchild" of psychoanalysis - Freud himself had never respected the activities and the competency at psychology of the "Adlerian Gang". These terms emerged in Hungarian psychological life in a mellowed way.
Individual psychology took part in Hungarian psychology with a delay of one or two decades comparing to psychoanalysis. Due to this, and the fact that at the beginning of the IP participation the psychoanalytical practice was a well-profitable opportunity, a few of the individualpsychologists changed sides for psychoanalysis. This mainly financial reason resulted that IP as a therapeutic method did not achieved spectacular successes. In the domain of pedagogic issues, however, and in educational counselling it played a pioneering and important role and made remarkable successes.

The dynamic of their mutual relations after the Second World War is a modelling story for social sciences. In the opening years of the era the relationship between the communists and the psychoanalytic institutions was quite good and manageable. Even after when the first offensives were taken against the IP Association, the Psychoanalytic Society enjoyed a "partial" trust by the ruling powers. In some cases, unfortunately, it appeared as a negative discrimination towards the IP. After this it is surprising that the Individual Psychological Association held out longer than the Psychoanalytic Society: the PS dismissed unasked in 1949, the IP Association was forced to be closed in 1950.
Taking a look at the situation after the "deliberation" of the country, the differences also can be seen in the two movements' predicament.
In the case of the psychoanalysis it was a good recommendation that the members were representatives of a popular psychological tendency - and gradually, through debates between experts of distinction and "hirelings" of politicians the society become banned.
In a paradox way, after the political changeover in 1948 the pressures did not reached the more prominent psychoanalytic movement first. The psychoanalysts were better defended against attacks "since the members were communists themselves in a great part".

AFTER THE 1950-ER DISMISSAL - FUNCTIONING WITHOUT INSTITUTIONALISING

After the dissolution of the association, even in the most depressing days of the dictatorship the taxpaying private practice of psychiatrists remained tolerated by the regime. Thus, individual psychology oriented psychotherapy was also permitted, and in the institutions of OTI (National Institute of Social Insurance) free psychotherapy was provided by connecting it to the neurological treatment (O. Arató, 1995). After the partial liberalisation of cultural policy in Hungary in the '60s and in the '70s, psychologists and psychiatrists interested in IP could attend conferences and a few books were published with some respect to IP ideas. After the 1988-89er changeover the IP Association started to be reorganised.

The functioning of IP theory and practice before the 'silent revolution' in 1989, however, left its enduring mark on the present activities of institutional psychology in Hungary. The practice of IP after the dissolution of its association still contained the possibility of making use of and improve professional knowledge and skills, the experts could hold meetings, introduce the techniques of supervision. But, on the other hand, they could not organise conferences, undertake on publishing activity and - perhaps most importantly - IP was not integrated into official university curriculum.
The present problems of the Hungarian educational and vocational guidance and counselling come at least in part from the disadvantages described above. The today's Hunarian professionals of applied psychology are highly qualified and have update knowledge on their professional area at which they work but the frameworks and institutions in which they work are only now being developed. The activities of these institutions, however, do not work in a concerted way but separately from one another, sometimes with redundancies and seldom rivalling and acting capriciously. Representatives of each element of this system take a lot of effort but their actions, due to the above mentioned reasons, are not quite efficient. This disunited system needs integration and orientation.

Bibliography

Arató O., Kiss Gy.: Az individuálpszichológia és hatása Magyarországon. Tankönyvkiadó, Budapest, 991

Arató O.: Az individuálpszichológia... Az Individuálpszichológia _tjain, I/1.

Fejtö, F.: Le sort des juifs dans les pays d'Europe orientale. Paris, 1971.

Fejtö, F. Judentum und Kommunismus. Antisemitismus in Osteuropa. Wien-Frankfurt-Zürich, 1967.

Gráber B. Emma: A magyar individuálpszichológiai mozgalom története (Manuscript).

Harmat, P.: Freud, Ferenczi és a magyarországi pszichoanalízis. Bethlen Kiadó, Budapest, 1994.

Nádor Gy.: C. G. Jung: Bevezetés a Tudattalan pszichológiájába. Társadalmi Szemle, 1948 jan. III/1.

Pálóczi-Horváth Gy.: Freud, avagy egy illúyió eredete. Forum, 3, 25-38, 1948.

Tariska I.: A freudizmus mint az imperializmus házi pszichológiája. Forum, 3, 799-804, 1948.

Tariska I.: És ami a freudi illúziókból következik. Forum, 3, 899-903, 1948.

THE SYMBOLICS OF PSYCHOLOGY UNDER A TOTALITARIAN SYSTEM: THE CASE OF HUNGARY IN THE 1960S[1]

Csaba Pléh
Department of General Psychology
Eötvös Loránd University, Budapest 64
P.O. Box 4, 1378 Hungary pleh@izabell.elte.hu

Social symbolics and social determination in science studies

In the recent articulation of the strong program in science studies a special attention is paid to the issue of the separability of the scientific content on the one hand and the symbolic role of scientific theories on the other. Most of the proponents of the strong program (the Edinburgh program, if you like) take a rather clear stance here. Scientific content and social symbolism cannot be separated in the causal models of the development of science. Scientists are certainly looking for truth (which is itself a socially conditioned category in this view, but we can ignore this aspect for the moment) but they do this as full social beings. Scientists of the modern times participate in different networks. The intellectual network, the world of the "invisible colleges" is the most visible out of these, but the everyday private network (the personal life of the

[1] While working on this paper the author had enjoyed the hospitality of the Center for Advanced Studies in the Behavioral Sciences, Stanford University, California. His work was supported by a Gardner Lindzey Fellowship of the Mellon Foundation. The author would like to thank Zsuzsa Vajda for letting him express his ideas at the conference.

supposedly removed scholar), and the social network, including the political one are all penetrable to each other as Bruno Latour (1993) claims it most clearly. The different networks are competitive and sometimes cooperative determiners of the growth of science.

There are certain trivial aspects of this social determination. Regarding the "socialist Europe" it is hard to forget a rather direct social determination.[2] Just think about the immediate power related social determination of the fate of genetics, and for that matter, psychology as well. (Joravsky, 1989 gives a detailed account of these practices regarding psychology.) "Progressive" science that promised sudden and immediate changes, both in agriculture, and in peoples's mind, in education, was not only symbolically related to a political "voluntarism", but through direct political control as well.

> To counterbalance the genuine science, an agricultural science of a different style was being created, one which cynically used the weapons of promise and deceit, an opportunistic science that accepted the paragraphs of countless decrees as axioms of its logical structure. Medvedev, 1969, p. 248

There are, however, less transparent determinations. Scientific theories with their symbolic aspects, but also with their directly claimed causal mechanisms enter the social world. They are cultivated and developed not merely for their merits but due to this underlying determination by interests. Let us take an example close enough to psychology. Steven Shapin (1975, 1979) in his studies on the fate of phrenology in Edinburgh society claims for a social explanation for the spread of the multiplicity view of the human mind. Not only were people

---

[2] Incidentally, this "social determination" based on political directives and expectations makes it hard for Central and East European scholars to deal without personal emotional involvement with the whole issue of social determination in science.

different from each other in this view, but some of these important differences were also observable on the basis of external signs (see of this semiotic aspect Lanteri-Laura, 1970). This was a new discipline. We should not forget that prior to phrenology there was no serious previous teaching about brain localization. The new discipline was cultivated by the new industrial and commercial classes of Edinburgh society, while the aristocracy together with official academia was motivated to claim a unified view of the mind, and therefore a unified vision of social power. Goldstein (1994) showed a similar distribution over a larger time scale. In nineteenth century France, throughout the whole century, there was a tension between unified and multiple views of the mind according to his interpretation. The multiple views were of different varieties: empiricist (Condillac), phrenological (Gall), or based on the clinical evidence of dissociation as in Charcot, Ribot, and Janet. This vision of the mind corresponded to a multiplicity vision of the world, and in its civil variety of the architecture of the mind to a claim that there are many different types of excellence. These approaches were in constant tension and debate with the centralized government related official philosophies that were lay versions of the view of the Catholic church regarding the soul, and symbolically, of centralized power.

According to the new strong proposals about the social determination of science, science should not be interpreted as the equivalent of a religious sacred realm that is not connected to profane and mundane issues (Bloor, 1991). It should be tied by its sociological study to its social background, and in this regard, not only an institutional history is in place, but a careful positioning of the theories and their social meaning as well, be them true or false from a later perspective (Shapin, 1992).

One can, of course, always raise the charge or challenge of hermeneutics here. It is questionable whether by showing the symbolic associations we really do uncover causal relations, which is the real intention of the strong program (Bloor, 1991) or do we merely reconstruct the workings of a semantic engine, i.e. the human mind that sees meaning, in this case social meaning, in all possible patterns. I sympathize with this latter view, with a serious restriction. We should not forget that not only are the people reconstructing the social interests behind a theory hermeneuticians, but the actors themselves whom they characterize also had been lay hermeneuticians. Therefore, in this symbolic domain one should not expect a simple

linear determination.

Take a trivial example. The social situation of the ethnically and linguistically divided Austro-Hungarian monarchy with the dissolution of the empire following World War I can be interpreted, as this has been done many times (see e.g. Janik and Toulmin, 1973) as the social explanatory background for the different deconstructionist views, be it regarding the mind and personality (Mach and Freud), or novel writing (Musil), language (Wittgenstein), and the whole world. Fair enough. As Nyíri (1992) pointed out, Austria can be seen as the first intellectual source and terrain of the "postmodern condition". But let us not forget that the same social setting was responsible for the flourishing of one of the strongest integrative attempts of modernity in the Vienna School, for theories that tried to reduce or model everything in a common language of a unified science. As a sensitive psychologist, you could of course claim that this unifying attempt was also related to the dissolution background. Certainly, as Toulmin (1990) indicates it for earlier versions of the unified views, for the Cartesianism of early modernity, unifying notions can be born due to the hopeless division and fragmentation of society, as a "compensatory reaction". (In the special case of early modernity, unifying conceptions appeared due to the devastation in the religious frictions in the Thirty Years War). All of this shows the complexity of the symbolic-social determination. Humans are agents in their social field, they act in it, but within the given circumstances. [3] There are a series of consequences of this for the symbolic relations that interest us here. As a first step, the acts of the human agents do not have a meaning fixed for ever: they have only contextual meaning. E.g. social progressivist movements tend to be tied to intellectual movements that question the dominant ideas in academia, whatever they be. Thus, there is no eternal social meaning to the different world views, their functional meaning depends on context. Second, the field does not simply determine the action of the people , but it does motivate them. Therefore, what we can reconstruct in the best case is "only" this chain of motivation, and not a clear determination.

This long preparation sets the tone to look for some similar symbolic determinations in

[3] This is a hidden Marx reference, by the way. "Men make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given, and transmitted from the past." (Marx, 1852/1963, p. 15).

the unfolding of psychology in the 1960s of socialist Hungary.

Passive organism versus activity

The issue of activity has become a central one in Central-Eastern Europe during the sixties. As a matter of fact, a loosely defined, fuzzy, "cloud like" opposition was setting up between two approaches to behavior and mind. They corresponded to two views on human nature, and, in fact, to two views of social organization, as shown in Table 1.

	FIXED VIEW	DYNAMIC, ACTIVE VIEW
Behavior science	Pavlovian conditioning passive sensation (mirror) one channel pathways learning and reflection	Instrumental learning active and motor perceptual models orientation and selection multiple pathways motivated learning
Corresponding social organization and philosophy	top-down organization historical relativism individuals are passive subjects fixed rewards closed world	bottom-up organization human nature as given individuals are active initiating agents changing rewards open world

Table 1: Some features of the opposition between two views on man in behavior science and social organization

The non-orthodox visions of human behavior were united in a feeling of looking for

MORE ACTIVITY IN HUMANS. In the debates characterizing active and passive views of perception, regarding the importance of instrumental and Pavlovian conditioning and the like, there was a hidden underlying social issue: namely the issue of how far are we as subjects of the Big Brother, indeed, merely instances of large scale social laws, or are we ourselves agents, with intentions and an active self-determination. One could even say, that open minded psychologists were looking for more "agency". This latter issue, however, is rather a differentiating feature. "Agency" at the time also had an activist reform-Marxist connotation. Therefore some professional psychologists were happy with the idea of activity, "agency" being too speculative, and, too Marxist for many of them.

Due to the underlying factors and the social symbolism associated with them is easier to understand after three decades some of the fierce oppositions and also the centrality and emotional interpretation of some ideas that would have been considered to be as "mere" scientific issues.

The case of conditioning

One of the most clearcut opposition was between PAVLOVIAN AND INSTRUMENTAL LEARNING, as summarized in Table 2. The good guys, of course, stood for instrumental learning. One factor in this was, of course, institutional. In Hungary, the Pavlovization of much of biology and psychology was a rather drastic and fast process in the 1950s, and the self-awakening psychologists in the sixties were reacting to that heritage. Pavlov was, so to say, the officialdom. Some of the experimental psychologists were trying to overcome it, or live with it by showing that it was possible to reconcile Pavlov with experimental (*horribile dictu*, American) psychology (Kardos, 1960). For the majority, however, a new road was open by emphasizing the importance of instrumental conditioning. Pavlovian conditioning had several features that predestined it to become "classical" conditioning. The very situation of the animal in the experimental setting is rather symbolic. Pavlov's dogs are constrained on the experimental podium. They are tied with scratches. The animal cannot move, typically it's only possible action is to modify its salivation. (Or, to move its one untied leg.)

The instrumental learning situation is, of course, opposed to this on a trivial symbolic

level as Russian versus American. There is, however, a further, semantically more rich symbolic opposition as well. The animal in a Skinner-box seems to have much more initiative. "Cats in the puzzle box", to use Thorndike's expression (Thorndike, 1898), try several movements, and one is selected due to the consequences. Thus, in instrumental conditioning there is a role for chance.

In a way, the precursor of instrumental conditioning, trial and error learning corresponded to that constantly moving and pragmatic Darwinian image of man the new canonizers, like Rorty (1979) see in John Dewey (1910). Instrumental learning indeed corresponds to an "instrumental vision" of knowledge. And in the framework of this image, an active, crucial and not merely background role is played by motivation. Learning only happens if their is reinforcement. In some interpretations of the Pavlovian case, however, learning happens merely by contiguity. Again, there is a social implication that can be easily projected to this image: on one image, you need to make people interested in what they do, in the other image, you do not need immediate rewards for any social activity. In the instrumental view of knowledge and in instrumental learning you need direct motivation and also the self initiated activity of the animal. Table 2 summarizes these contrasts.

CLASSICAL CONDITIONING	INSTRUMENTAL CONDITIONING
constrained animal	freely moving animal
learning by association	learning from consequences
motivation not required	motivational essential
role of chance reduced	chance is essential

Table 2. Some contrasts of the two views of learning

All of these features made for a strange position, if I dare to say, a strange "ideological

position" of INSTRUMENTAL LEARNING in a strictly restrictive society. The same Skinnerian model of learning that has become in the late sixties the symbol of control, manipulation, and a lack for freedom, a deterministic view of man in American society, and in the high intellectual circles, a symbol for the over ambitious reductionism of Skinner (Chomsky, 1959), in Eastern Europe BECAME A SYMBOL FOR ACTIVITY AND FREEDOM as contrasted to Pavlov's dogs who were merely subjected to interventions and were undergoing learning without doing too much. The small textbook by Barkóczi and Putnoky (1968), and the neurophysiological theory of reinforcement elaborated by Endre Grastyán from the early sixties on, and presented to broader audiences as well (Grastyán, 1967) were clear examples for this interpretation. The troubled fate of some Pavlov followers in Russia like Beritashvili who dared to use more naturalistic settings with freely moving animals clearly shows that the symbolic aspect of the Pavlov orthodoxy was extremely strong (see Joravsky, 1989 for details). The symbolic side of experimentation there became a moving of social reality. Conditions in the sixties were not as trivial in Hungary. People were not persecuted for taking Skinner or Konorski seriously, but there certainly was a symbolic side to their preferences.

Perception and activity: The mirror revived

The same underlying issue, the role of activity, showed up in perceptual research and theory. Both in physiology and in philosophy (and of course, psychology) there was an implied or *de facto* passive view of perception that would take perception to be a mere information intake. This was the clearest example of the "mind as mirror of the world" image of modernity criticized so sharply by Rorty (1979). One could even say that the combination in official ideology of the Leninist version of epistemology where the mind "mirrors the world" and Pavlovian physiology with the "two signaling systems" was a sad caricature of scientific modernity and its representational view of the mind. The social symbolics of this official image had to do with passivity again: mirrors and signals do not do too much, things happen to them.

In contrast to this official passive vision THE GOOD GUYS WERE CLAIMING THAT PERCEPTION WAS TO BE AN ACTIVE PROCESS. There were several rival varieties of these claims

even within Hungary. First of all, there were attempts within Marxist theory for a change towards a more active image of man, including a concept of "active mirroring". That meant, among other things, a return and a cultivation of the anthropology of the young Marx (Márkus, 1968a), and an in depth philosophical analysis of perceptual research from the point of view of "activity theory", social categorization in perception, and concepts coming from analytic philosophy (Márkus, 1968b). This was heretic enough for traditional "Leninist theories of mirroring", but was not appealing to all psychologists.

On the other end of the scale, experimental psychologists were mainly busy treating the motor components of perception as essential, and at the same time campaigned for some version of a template based view of perception where perception would be infiltrated by background knowledge. Experimental programs were initiated by Zsolt Tánczos (for a late review see Tánczos, 1984) for the explanation of the fine role of motor components in compensating retinal image distortions. The Innsbruck studies of Kohler belonged to the popular issues of the time. Motor theories were combined in this view with a Brunerian New Look approach. The emphasis on perceptual learning carried the implication of a nonrigid world that is not predetermined, neither by nature nor by society.

The reader edited by Magda Marton (1975) was a clear and trend setting example for this approach. At the same time, some other psychologists took up the "neomarxist" interpretations of the issue of activity and the Soviet work towards an active view of perception and human "agency" (Leontev,1978). Again, a reader this time edited by Ibolya Váriné-Szilágyi (1974) was a clear summary of this attitude.

The two lines were rivals in a sense due to some of their ideological connotations. The latter group thought that it would form a perceptual theory that is reconcilable with a view of man that treats man as more "active", more agent-like in the sense of the early writings of Marx. The more experiment oriented group thought, on the other hand, without spending to much time to spell it out dangerously, that psychology was an issue for the psychologists and should not be messed up with a reinterpretation of Marxism along more action theoretic and activist lines. It should to be the least possible "infected" by philosophical considerations whatsoever. This should not be taken as an aversion towards philosophy as such, but, rather as an experience based attitude that showed that association with politically interpreted philosophies

already lead several times during this decade to later politically based professional discrimination in Hungarian psychology (see about this László and Pléh, 1992, Pléh, 1997). Though experimental psychologists had a symbolically motivated preference for an active view of the mind, they did not see any need to ally this to a reform Marxist orientation. For them, the battle for more "activity" in models of the subject was also a battle for more autonomy and independence of the whole field. In the official jargon of the time that corresponded to the idea that PSYCHOLOGY WAS A "NATURAL SCIENCE", THEREFORE NOT PART OF THE "SUPERSTRUCTURE", THEREFORE IT IS IDEOLOGICALLY NOT SENSITIVE TO "CLASS INTERESTS" or what not.

Thus, for both directions anything that was "active" was supposed to be good and progressive by the psychologists. Notice, that those were times when the ideological debates were going on for a proper interpretation of "progress". "Progress" was not yet an unwelcome four-letter word. Everybody still believed in the idea of progress. But some thought progress entailed a more natural science view of man, with a deterministic flavor, while others thought progress entailed a more social, or even a more voluntaristic and undeterministic image of man.

Motivation

The issue of CURIOSITY, ORIENTATION REACTION, COGNITIVE MOTIVATION AND SPONTANEOUS ACTIVITY ALSO PLAYED A CRUCIAL ROLE IN THIS SELF-DEFINITION OF MODERN HUNGARIAN PSYCHOLOGY. That appeared in several forms. In psychophysiology, Moruzzi and Magoun (1949) became the bibles, and the most intensive research unfolded regarding the importance of activation in learning, their connection to sensory reinforcement, and relationship to play and self initiated activity. The work of Endre Grastyán from the fifties well into the eighties was the clearest example for this trend (Grastyán, 1961, 1985). Indeed, he was the first to propose a model about the role of hippocampus in learning through the regulation of orientation.

In human psychology as well, activation mechanisms were presented as crucial (Marton, 1964) and they were even put into the center of research on modern experimental typology (Marton and Urbán, 1965). The importance of "manipulative behavior" and the central role of

self based sort of actively searching cognitive motivation was also central to studies on infant development (Barkóczi, 1970). This was coupled with an emphasis on less restraint in infant education (the so called Loczi method of institutional infant care). All of this implied a view of man where man is not only a passive information and knowledge intake unit under Prussian control, but is actively seeking knowledge and the truth. Elicited behavior was contrasted with spontaneity. Parallel to this there was an emphasis on the role of non homeostatic elements in motivation (Barkóczi and Putnoky, 1968, Grastyán, 1967, 1985). The underlying was again there: strict homeostatic mechanisms were equivalent to a closed world, while curiosity, activation and so on represented the idea of an open universe. Interestingly enough, there were frictions between neomarxist trends and the "naturalist' psychologists regarding motivation as well. Ágnes Heller (1979) campaigned for a reduced role of "natural" moments in human emotions and motivation, and argued for a constructivist theory of motivation, not unlike the one proposed by Garai in a philosophical psychology inspired largely by the activity theory of the Vygotsky school (1969, 1993). Meanwhile, the "naturalists", referring to ethology for support argued for specific human instincts and a biological explanation of the non-homeostatic motivation systems.

Group organization

In the revival of social psychology in Hungary in the sixties there was a clear trend towards showing the **SUPERIORITY OF THE SPONTANEOUS AND EMOTION OR ATTRACTION BASED GROUPINGS VERSUS THE FORMAL ONES**. This happened in a society where the official ideology paid an enormous amount of lip service to "communal organization" and to the idea of an abstract predominance of the social over the individual. The *de facto* society was based on strong hierarchies (forget about the egalitarian slogans). Societal organization was bureaucratic in the sense of being formal, not in the sense of being efficient. This was accompanied by an open emphasis on the importance of class, class interest and so on.

The good guys contrasted with this an emphasis on **SPONTANEOUS STRUCTURES**. The sixties were the prime time of sociometric research and activism in Hungary. The clear

implication being that primary groups should be based on real affinity and as Ferenc Mérei's (1989) extensions of Moreno showed, on efficiency or competence based organization, rather then the official one. There was a constant undertone suggesting that official groupings were simply bad. The officialdom was inefficient in selecting leaders: we are in fact the alternative, the "real leaders". That is what any vote, be it a sociometric vote, would show. Thus sociometry in a way was a substitute for politics: it implied an organization outside politics, but at the same time it was based on VOTING AND CHOICE that did not really exist in Hungarian official politics at all. With its emphasis on emotionality, immediate social power, and on choice, sociometry had a hidden threatening message. Even more threatening than the mere idea of social engineering was. Remember that the communist credo in its early forms had a clear social engineering commitment.

Ferenc Mérei, the leader of the sociometry movement was the archetypical network guru. His entire life was defined and fulfilled through the networks he not only belonged to, but brought to life. At the same time his main scientific contributions also had to do with the issue of the relationships between the group and the individual, between good and bad networking from the perspective of democracy and individual happiness. His early paper (Mérei, 1947, 1949) pointed out that group interaction can create an "experiential surplus" that is different from the mere sum of the individual experiences. Later on, he developed this notions into several directions: elaborated the notion of "allusion" as a semiotic way to remind us of our group belongingness (see e.g. Mérei, 1994) and also worked out a theory of the relationships between leaders and groups where efficient leaders always take over the values of the group.

Mérei's life and work later on can be seen as an example of the implications of some of his early insights. His life was also a living witness for the intervention of politics into the life of the scholar and the other way around. As Erôs (1995) recently pointed out, the active political leader of educational reform of the forties, when fallen from grace and even put into prison, learned from his own example two important things for a Central European scholar. First, the shaky nature of life and power, the constant shift between inner and outer circles, which lead to a reflective consideration of the relationships between power and real human groups. A theory and a practice with hundreds of followers claimed a central place for spontaneity and for spontaneous group formation on the scientific level. Second, a *de facto*

practice of unofficial groups followed where togetherness, training, and the supportive value of group relations against the power structure of society was constantly reexperienced. Primary groups and their emotional aspects had become for Mérei both the cementing factors of human life at large and the keys to survival and protection of individual integrity against officialdom.

Another central feature of early Hungarian social psychology is the constant emphasis on **ANTI-AUTHORITARIAN ATTITUDES AND ON THE IMPORTANCE OF DEMOCRATIC GROUP LEADERSHIP**. While the Lewin and Adorno inspired and mediated notions retained their original antifascist meaning, at the same time they transmitted a more general anti-authoritarianism (see about this Erôs, 1979). I.e. they carried an implication, though openly not spelled out, but tacitly assumed that our own society also showed signs of the illness of authoritarianism, and cannot really face democratic leadership practices on any level.

Motherhood and the state

The seemingly absolutely innocent issue of mother-child relationship was also not an easy and trivial one. Comparative psychologists like Magda Marton and Ilona Barkóczi, as well as developmentalists in textbooks (Mérei and Binét, 1970), psychoanalysts dealing with attachment problems, and even the openly not psychoanalytic case study literature constantly reemphasized **THE IMPORTANCE OF MOTHERHOOD AND MATERNAL LOVE IN INFANT DEVELOPMENT.** One has to understand the symbolic undertones of this, again, not in the context say of present day American feminism but in the Hungary of the fifties and sixties. One of the "party lines" suggested an idealized version of Makarenko based communal education. It suggested that as a matter of fact, it is the state that has to care about children, in all levels of their development. The state should have thereby, of course, control over the moral development of children. This way, one of the great issues of educational publicity a the time, "double education" (home being religious, school being materialistic-atheistic) would be overcome.

Seen from this perspective reemphasizing of maternal roles, and the arguments pulled for it from research on hospitalism and the Harlows, was not a return to traditional role models.

rather it was an attempt to use the cultivation of scientific facts to protect children's rights against attempted organized hospitalization. To phrase it anachronistically, it was an early children's right movement.

The issue of knowledge and truth

There is an interesting underlying problem all over Central and East European intellectual history in the times after Khrushchev. Scientists always believed that they were standing on the right side, in the sense that they were in the side of **REAL PROGRESS**. Society and power might have biased the notion of progress, but there was a belief in real progress. For science, this implied that there is going to be more freedom of research, and a clear stance against obscurantism. Truth will be victorious. Truth cannot be oppressed on the long run, and there is an affinity between reformist social changes and the truth as delivered by science itself. Belief in "positivistic truth" as hard as it may to accept it now, in the context ot the time was an act of moral an intellectual revolt, and not a comfortable stance. It would of course be very difficult not to believe in the objectivity of the truth in a social organization where you constantly experience not the unconscious but the planned and manipulative distortion of truth.

Present day hermeneutically based relativistic views on truth challenge this enlightenment version of belief in progress in the former socialist part of Europe well as in the "educated West". Both the scientist and the hemeneutician believe in the need to increase human freedom. But they diverge in the fact that the hemeneutician would extend his flight for freedom towards a total freedom of interpretation as well. In this view, it would be an unfounded reification to believe in the objectivity of truth. Truth itself is a construction. The scienticist scientist, on the other hand, believes that his freedom of interpretation is constrained. He fights society in the very name of these constraints on freedom, while the hermeneutician challenges the notion of truth in the name of the freedom as well.

A version of these different revolts against authority was true for the non-existing dialogue between Neomarxists and experimental scientists. Both groups were looking for more freedom but each one suspected the other in compromising freedom for new constraints. The

data oriented social and behavioral scientist was supposed to be too much involved in building a deterministic image of man which would counterbalance the socially deterministic but the same time factually voluntaristic official view. The Neomarxist at the same time overemphasized the "constructed character" of social life and social determination. In the eyes of the scientist working in the direction of providing an intellectual sanctuary from the voluntaristic politics at the top, this latter one seemed to be a rather threatening perspective.

One can only hope that in the politically clearer perspectives of today a more open dialogue will develop between the naturalist and the constructive images of man, and small intellectual communities like the one existing in Hungary can even become interesting in these dialogues.

References

Barkóczi, I. (1970): A megismerési motíváció ontogenezise. [The ontogenesis of cognitive motivation] *Magyar Pszichológiai Szemle, 27*, 26-58

Barkóczi, I. and Putnoky, J. (1968): *Tanulás és motíváció.* [Learning and motivation.] Budapest: Tankönyvkiadó

Bloor, D. (1983): *Wittgenstein: A social theory of knowledge.* New York: Columbia University Press

Bloor, D. (1991): *Knowledge and social imagery. 2nd edition.* Chicago: University of Chicago Press

Chomsky, N. (1959): Review of B.F. Skinner's Verbal Behavior. *Language, 35*, 26-58

Dewey, J. (1910): *The Influence of Darwin on Philosophy.* New York: Holt

Erôs, F. (1979): Szociálpszichológia és kritikai elmélete. [Social psychology and its critical theory] *Pszichológiai Tanulmányok XV*, 347-374

Erôs, F.(1995): Mérei Ferenc életmûve és a magyar szociálpszichológia. [The ouevre of Ferenc Mérei and Hungarian social psychology.] In: Kiss, 1995a, pp. 123-136

Garai, László (1969): *Személyiségdinamika és társadalmi lét.* [Personality dynamics and social being] Budapest: Akadémiai Kiadó.

Garai, L. (1993): The brain and the mechanism of psychosocial phenomena. *Journal of Russian & East European Psychology, 31*, 71-91.

Goldstein, J. (1994): The advent of psychological modernism in France: An alternative narrative. In: Ross, D. (1994, ed.): *Modernist Impulses in the Human Sciences.1870-1930.* Baltimore: Johns Hopkins University Press,190-209

Grastyán, E. (1961): The significance of the earliest manifestations of conditioning in the mechanism of learning. In: Delafresnaye, J. F. (ed., 1961): *Brain Mechanisms and Learning.* Oxford: Blackwell, 243-263

Grastyán, Endre (1967): A tanulás alapvetô paradoxonai és idegélettani feloldásuk. [Basic paradoxes of learning and their resolution by neurophysiology.] *Pedagógiai Szemle,*

Grastyán, Endre (1985): *A játék neurobiológiája.* [The neurobiology of play]. Budapest: Akadémiai

Heller, A. (1979): *A Theory of Feelings.* Assen : Van Gorcum

Janik, A. and Toulmin, S. (1973) : *Wittgenstein's Vienna.* New York, Simon and Schuster

Joravsky, D. (1989): *Russian Psychology: A Critical History.* Oxford: Blackwell

Kardos, Ludwig (1960): *Die Grudfragen der Psychologie und die Forshungem Pawlow's.* Budapest: Akadémiai

Kiss, Gy. (1995a, ed.): *Pszichológia Magyarországon.* [Psychology in Hungary]. Budapest: National Library and Museum of Education

Lanteri-Laura, G. (1970): *Histoire de la phrénologie.* Paris: Presses Universitaires de France

László, J and Pléh, Cs. (1992): Hungary. In: Sexton, V. S. (eds..): *International psychology.* Lincoln: University of Nebraska Press, pp. 196-205.

Latour, B. (1987): *Science in the making.* Cambridge, Mass.: Harvard University Press

Latour, B. (1993): *We have never been modern.* New York: Harvester

Leontev, A. A. (1978): *Activity, Consciousness, and Personality.* Englewood Cliffs, N.J. : Prentice-Hall

Márkus Gy. (1968a): *Marxizmus és antropológia.* [Marxism and anthropology]. Budapest: Akadémiai

Márkus Gy. (1968b): Az észlelés és a pszichofizikai probléma. *Magyar Filozófiai Szemle, 12,* 217-299

Marx, K. (1852/1963): *The Eighteenth Brumaire of Louis Bonaparte.* New York: International Publishers

Marton, L. M. (1964): Az aktívációs és a tájékozódási reakció fôbb mûködési elvei és jelentôségük a pszichikus folyamatok megismerésében. [Basic principles of activation and orientation reactions and their role in the understanding of psychological processes]. *Pszichológiai Tanulmányok, VI.*, 37-50

Marton, L. Magda (ed.,1975) : *A Tanulás szerepe az emberi észlelésben.* [The role of learning in perception]. Budapest: Gondolat

Marton, L. M. and Urbán, J. (1965): Pszichofiziológiai módszerek alkalmazása a személyiségvizsgálatokban. [Psychophysiological methods in personality studies.] *Pszichológiai Tanulmányok. VII.*, 199-206

Medvedev, Z. A. (1969): *The rise and Fall of T.D. Lysenko.* New York: Columbia University Press

Mérei, F. (1947): *Az együttes élmény.* (The joint experience.) Budapest: Officina

Mérei, F. (1949): Group leadership and institutionalization. *Human Relations, 2,* 23-29

Mérei, F. (1989): *Társ és csoport.* (Companion and group.) Budapest: Akadémiai

Mérei, F. (1994): Social relationships in manifest dream content. *Journal of Russian & East European Psychology. 32,* 46-68.

Mérei, F. and V. Binét, Å. (1970): *Gyermeklélektan.* (Child psychology.) Budapest: Gondolat

Moruzzi, G. és Magoun, H. W. (1949): Brain stem reticular formation and activation of the EEG. *Electroencephal. Clin. Neurophysiol., 1,* 455-473

Nyíri J. K. (1992): *Tradition and individuality.* Dordrecht: Kluwer

Pléh, Cs. (1997): Hungarian Contributions to Modern Psychology. Paper presented at the conference "Hungarian Contributions to Scholarship". Indiana University, Bloomington, April 5th, 1997.

Rorty, R. (1979): *Philosophy and the Mirror of Nature.* Princeton: Princeton University Press

Shapin, S (1975): Phrenological knowledge and the social structure of early 19th-century Edinburgh. *Annals of Science 32,* 219-243

Shapin, S. (1979): Homo phrenologicus: Anthropological perspective on a historical problem. In:Barnes, B. and Shapin, S. (1979, eds.): *Natural Order: Historical studies of Scientific Culture.* London: Sage, 41-71

Shapin, S. (1987): Shapin, S. (1992): The externalism-internalism debate. *History of Science, 30,* 334-369

Tánczos, Zs. (1984): *A látás alapfolyamatai.* [The basic processes of vision]. Budapest: Akadémiai

Thorndike, E.L. (1898): *Animal Intelligence.* New York and London: MacMillan Company

Toulmin, S. (1990): *Cosmopolis: The Hidden Agenda of Modernity.* New York: Free Press

Váriné, Sz. I. (1974, ed.) : *A pszichikum és a tevékenység a mai szovjet pszichológiában.* [Mind and activity in present day Soviet psychology.] Budapest: Gondolat

ON HISTORICAL CONTINUITY

Hans van Rappard

Netherlands Center for Theoretical Psychology

at

Department of Psychonomics

Vrije Universiteit

1111 De Boelelaan

1081 HV Amsterdam

jfh.van.rappard@psy.vu.nl

Abstract

The paper begins with a brief review of the early beginnings of the Lockean Way of Ideas. Some key-characteristics are linked up to what various authors have called, the Lockean Tradition. Next, it is pointed out that such a tradition entails a long historical continuity at the foundational level of psychological thinking, and that here one source of evidence may be found for the contemporary relevance of historical studies. It is argued that continuity at the level of fundamental issues may entail a limited historiography, which, however, in virtue of its limitations does not run the risk of disciplinary cross-dressing, that is, the tendency of scientist-historians to adapt to the methods and issues of professional history.

HISTORICAL CONTINUITY IN THE LOCKEAN TRADITION

THE WAY OF IDEAS

A fundamental concept in seventeenth and eighteenth century thought is Idea. What John Locke called, the Way of Ideas governed philosophy for well over a century. So central were ideas that the authors of a widely adapted text could write that, "Some words are so clear that they cannot be explained by others, for none are more clear or more simple. 'Idea' is such a word." (Arnauld & Nicole, quoted by Ariew & Grene, 1995, p. 87).

The term Idea in its modern sense is attributed to Descartes, who made a new start in philosophy with it. But his thoughts on the topic did not fall from the sky. Descartes shaped his concept by drawing on ancient as well as contemporary meanings. Ariew and Grene (1995) reviewed the use of Idea in the seventeenth century before Descartes in a number of contemporary philosophical writers whose terminology may be assumed to have been familiar to any scholar, whether to Descartes himself or to those in his circle. In one of these texts the new meaning exploited by Descartes is found. Idea is defined as an image, "expressive of something, something which the mind comtemplates. It is both something I do - an act - and something I 'see'" (Ariew & Grene, 1995, p. 95). From this it appears that ideas could be conceived by early seventeenth century scholars as both **activities** of the mind and the **objects** of those activities. In the terminology of the time, ideas are modes or activities of the mind when taken

'formally' or 'materially'. But ideas are also representatives of (mental or material) objects and this is what they are 'objectively'. Thus, taken materially an idea of the sun is a mode of the mind but objectively it is the sun in so far as it occurs in the understanding. Descartes was aware that there might be a difficulty lurking. As he acknowledges in the Preface to his **Meditations** in discussing an objection to his proof for the existence of God, "... there is an ambiguity here in the word 'idea'. 'Idea' can be taken materially, as an operation of the intellect, in which case it cannot be said to be more perfect than me. Alternatively, it can be taken objectively, as the thing represented by that operation; and this thing, even if it is not regarded as existing outside the intellect, can still, in virtue of its essence, be more perfect than myself" (Descartes, quoted by Wahl, 1988, p. 560).

The ambiguity about the mind as an activity on the one hand, or an object, representation or mental content on the other, is not the only source of the difficulties that the idea concept brought in its wake. But since the present paper is not concerned with an exhaustive analysis of its origins but with continuity in the history of psychology, the above may suffice to appreciate that in spite of the optimistic Arnauld and Nicole quotation on the clarity of the Idea-idea, in fact a lot remained unclear about it. In due course, vehement discussions erupted on the nature of ideas. One of these discussions is the controversy in the mid 1680s between Arnauld and Malebranche (cf. Wahl, 1988). Although the

controversy took place within the framework of a broader dispute over theological matters, its source is found in the ambiguity noted earlier by Descartes himself.

Arnauld and Malebranche focused on the question whether there are **two** kinds of ideas: mental activities, and the objects of these activities - or just **one**: an activity of the mind which itself is an awareness of an object. Arnauld held the latter position: it is the activity of the mind which does the representing, whereas its objects are not ideas but ,generally speaking, things. Against this, Malebranche upheld the former view according to which mental acts could not be representational, but the immediate object of the mind is a representational entity. Such representations Malebranche called Ideas, and he maintained that they were in the mind of God. That is, when perceiving an object, God sees to it that the appropriate idea is placed in the mind, along with the sensory impression caused by the object. This seemingly odd twist in the controversy harks back to a Medieval dispute over divine illumination. Malebranche concurred here with those who took the originally Platonic view that the only proper objects of knowledge are the uncreated ideas or "exemplars" in God's mind. Arnauld however, interpreted ideas psychologically, as Descartes had also done, as activities or modes of the human mind.

John Locke was well acquainted with Descartes's writings and some of the controversies among his followers. In his analysis of consciousness a central role is given to the concepts of

activity and content or object - the key-concepts of Arnauld and Malebranche, whose exchange he read when completing his **Essay Concerning Human Understanding**. On a Lockean analysis, Yolton (1984, p. 101) notes, "it is easy to slip into talking of the content as if it is an object", and this is precisely what seems to have happened to the Idea concept of Arnauld and Locke.

As we have just seen, mental content as object or entity was probably not what Arnauld had meant by Idea. But accordingly to the so-called Old View of his work that was advanced in the eighteenth century by Thomas Reid, Arnauld is a representationalist, that is, he believes that we (mediately) see external objects only by (immediately) seeing ideas, which represent them in the mind. Cook (1991), along with Yolton (1984) rejects this standard interpretation. Seen in terms of the Old View, Arnauld is an easy target for the Stock Objection (Cook, 1991, p. 185) against representationalism: the claim that ideas form a veil between us and the external world, so that all we ever see are ideas.

Clearly, Yolton also rejects the representationalist interpretation of Locke and the ontologization of the ideas. He prefers to read 'ideas' not as 'things' but as 'conscious mental contents', and to translate 'exist in the mind' simply as 'understand', thus reducing the ontological overtones (Yolton, 1984, p. 102). On this view, perceiving an object is having or receiving ideas. Locke and Malebranche would not disagree here, Yolton says, but they would most certainly disagree on the nature of these ideas. As has already been

mentioned, for Malebranche an idea is God-given and thing-like; for Locke it is neither.

Regarding the nature of ideas Locke seems to have followed Arnauld, or rather, what is now called the New View on this philosopher (Cook, 1991). Yolton even speaks of the "Locke-Arnauld concept of ideas", which does not involve ideas as entities standing between us and the external world. Hence, as Yolton sees it, Locke is not to be called a representationalist. However,

> From the very beginning readers of Locke's **Essay concerning Human Understanding** viewed his account of ideas as making our knowledge of the world indirect, representative, and uncertain. Many of the critics of his **Essay** charged that knowledge of the world was even rendered impossible by the new 'way of ideas' (...) If ideas and their relations constitute knowledge and what is present with or to the mind, what are the guarantees that those ideas and relations do inform us about the world? A realm of ideas threatened to supplant, in our knowledge, the world of physical objects and events (Yolton, 1984, p. 4).

THE LOCKEAN TRADITION

In current historiography, continuity is not a popular topic. To many in the field it has a definite essentialist ring about it. Way back in the sixties however, things were often seen in a different light. This was expressed by, for instance, Helson

(1972) according to whom "(f)undamental issues are always with us and we must be prepared to face them. The history of psychology serves to focus such issues better than any other approach in our subject" (p. 116).

Historical continuity emerged rather strikingly from a number of approaches to the then fundamental issue of psychology's pre-paradigmatic status. In an attempt to make up for the lacking paradigm, Watson (1967), quickly followed by others, isolated various numbers of prescriptions, factors, and other presumably fundamental dimensions of psychology-through-the-ages. It is not difficult to recognize in these shopping lists the characteristics of the new science that was being developed in Locke's days and that spilled into the work of most would-be "Newtons of the mind".

The two dichotomous traditions that some of the early amateur historians of psycholgy perceived were coined, the tough and the tender minded ones. A number of authors have pointed to such developmental lines, which, even if they were approached from different perspectives tended to come out remarkably similar. Historical amateurs, such as Allport (1955), Watson (1967) and, more recently, Gergen (1982) all traced their traditions to seventeenth century philosophy and hence, these may perhaps be thought of as historical constants (cf. Strickland, 1991). In the context of this paper it is interesting to note that the tough-minded scientist culture - characterized by, e.g, objectivism, elementarism, and mechanicism - is sometimes called, the Lockean tradition (for

fuller treatment, see Van Rappard, 1993a). As summarized by Rorty (1980),

> The notion that there is a problem about mind and body originated in the seventeenth century's attempt to make 'the mind' a self-contained sphere of inquiry. The idea was to offer a para-mechanical account of mental processes which, somehow, would underwrite some claims to knowledge and disallow other claims. The paradigm of the 'epistemological turn' taken by philosophy was what Kant called 'the physiology of the understanding of the celebrated Mr. Locke ...' (p. 126).

CONTINUITY AND THE CASE FOR LIMITED HISTORY

It is a stark continuity that we see emerging here. A continuity of foundational concepts that somehow have been preserved over a period of some three hundred years in spite of incisive change in every domain of life and society. It must be in the striking trans-contextuality implied by such continuity - which sits so uneasily with historiography as currently recommended, even if not always practiced (Coleman, Cole & Webster, 1993) - that an important reason may be found for the impopularity of the topic.

However, the significance of history in psychology does not only show in long lasting tracks of fundamental concepts and problems in the discipline. Rather the opposite: the continuity perspective would seem to barely touch the

historicity of the field. From the historiographical point of view it entails a history of ideas or problem-oriented approach, which Krueger (1984) thinks is a rather a-historical kind of history - and rightly so. I feel that the matter might be put even stronger and that the kind of historiography implied by the continuity perspective should be called, **limited history**. 'Limited' because the present point of view is restricted in at least two ways. It is to be called limited because, firstly, emphasizing foundational matters it covers a limited **domain** and, secondly and closely related to this, it entails a **historiography** that according to current standards must be judged limited.

However, when talking about the limitations of limited history it should immediately be asked **whose** limitations are meant; that is, from whose point of view may the limitations appear problematical? It seems to me that in history of psychology, or history of science at large for that matter, inevitably two parties are involved. On the one hand, there are the scientists whose discipline is studied, and on the other hand, there are the scientists who study that discipline, that is, the (professional) historians. In the early stages of the history of a particular science the two parties tend to coincide but in the course of its development they may diverge. As Rachel Laudan (1993) has told us, this is what happened in the natural sciences. In our field things are still different but as Danziger (1994, p. 468) wrote, "increasingly, professional historians are also making contributions in th(e) area (of psychology)". Nevertheless,

the role of history still seems different in psychology than in the natural sciences. And in my view, it is only natural that history should have a different place in the two disciplines, especially with regard to foundational issues.

It should be noted that when mentioning 'history' I do emphatically **not** mean to say that the natural sciences, qua science, are not historical, or less historical than psychology. What I do mean to say is that in the former, in contradistinction to the latter, history cannot be seen to have any contemporary relevance; but in our discipline it has (cf. Danziger, 1994, pp. 471 - 472). At an earlier ISTP conference I had an opportunity to summarize this view by stating that in psychology "the past is part and parcel of the present" (Van Rappard, 1990). This can easily be gathered from the fact that in the spate of studies that has been and still is being published on the foundational problems of the field, history - often a limited history indeed - has an essential role to play. Again this may be summarized in a quotation - a combined quotation from Danziger (1994, p. 472) and the present author (Van Rappard, 1997, p. 102) according to whom, "in marked distinction to Galileo and Newton in physics - Weber and Durkheim in sociology, Adam Smith and Ricardo in economics, and Wundt, James and Vygotsky in psychology are still studied".

Let me get back to what was said earlier on the two parties involved in the historiography of any science: the (insider)

scientist-historian and the (outsider) professional historian. As biologist-historian Ernst Mayr (1990, p. 304) observed, "(m)ost scientists have had considerable interest in the history of science. This is not surprising, because 'science without its history is like a man without a memory'. The interest of the scientist, however, is quite specific and in many respects different from that of the historian trained in the humanities".

If it can be argued, as I have done (Van Rappard, 1993b, 1996b) that history has a role to play **within** psychology, it would seem imperative that the issues taken up for historical scrutiny and the concepts used for presenting and communicating the answers fit in with the discipline. That is why I argue for Limited History, that is, a history that does not work the field of psychology using the conceptual and methodological tools wrought for a different field. Hence, I contend that with regard to foundational issues, the scientist-scholar does not necessarily need the full sophistication of the professional historian. As Mayr (1990, p. 304) says, continuing the quotation above, "(t)he foremost interest of the modern scientist-historiographer is the development of ideas, from their origin through all their permutations up to the present day. The reason for this interest is that it is impossible to understand many of the current controversies and prevailing concepts without studying their history". It is doubtful if the skills of the professional historian are essential to such an admittedly limited history. In view of the historiographical

recommendations issued since the 'social turn' (Ash, 1987) the question is: are the new approaches and perspectives mandatory? Should the scientist-scholar dress up as a professional historian? Should she cross-dress in the methodological garment of another discipline? Should she cross over to another, presumably greener field?

Disciplinary cross-dressing has been spotted and declared unfit for psychology by Brock (1996), Danziger (1994), and Van Rappard & Van Strien (1993). And even in the natural sciences professional history and its aftermath does not always meet with approval. Swerdlow (1993) surveying developments in the history of the exact sciences noted that increasingly, work is seen that requires no knowledge of science, or even treats such knowledge with disdain. The impact on the field is deemed disastrous, "an increasing and intentional ignorance of science, precluding serious work and reducing the history of science to amateurism." (p. 326). What is to be done? Swerdlow wonders and exclaims in exasperation,

> Enough of rhetoric and discourse and power and patronage and all other fashionable banalities (...) for God's sake let us learn our sciences (p. 326).

REFERENCES

Allport, G.W. (1955). **Becoming**. New Haven: Yale U.P.

Ariew, R. & Greene, M. (1995). Ideas, in and before Descartes. **Journal of the History of Ideas, 56**, 87 - 106

Ash, M.G. (1987). Introduction. In M.G. Ash & W.R. Woodward (Eds.), **Psychology in twentieth century thought and society** (pp. 1 - 11). Cambridge: Cambridge U P

Brock, A. (1996). In praise of the mixed model: History of psychology as science-history and history of science. **Paper, 15th Cheiron Conference,** Leiden (Netherlands), 3008 - 0309/96

Coleman, S.R, Cola, Ph. & Webster, S. (1993). Empirical assessment of methodology in the history-of-psychology literature, 1975 - 1986. **American Journal of Psychology, 106**, 253 - 272

Cook, M. (1991). Malebranche versus Arnauld. **Journal of the History of Philosophy, 29**, 183 - 199

Danziger, K. (1994). Does the history of psychology have a future? **Theory & Psychology, 4**, 467 - 484

Gergen, K.J. (1982). **Toward transformation in social knowledge.** New York: Springer

Helson, H. (1972). What can we learn from the history of psychology? **Journal of the History of the Behavioral Sciences, VIII**, 115 119

Krueger, L. (1984). Why do we study the history of philosophy? In R. Rorty, J.B. Schneewind & Q. Skinner (Eds.).

Philosophy in history (pp. 77 - 101). Cambridge: Cambridge U P

Laudan, R. (1993). Histories of the sciences and their uses: A review to 1913. **History of Science, xxxi,** 1 - 34

Mayr, E. (1990). When is historiography whiggish. **Journal of the history of ideas, 51,** 301 - 309

Rappard, J.F.H. van (1990). In praise of problemgeschichte. In W.J. Baker, M.E. Hyland, R. van Hezewijk & S. Terwee (Eds), **Recent trends in theoretical psychology, II** (pp. 317 - 325). New York: Springer

Rappard, J.F.H. van (1993a). History & System. In J.F.H. van Rappard, P.J. van Strien, L.P. Mos & W.J. Baker (Eds.), **Annals of theoretical psychology, 9,** 1 - 40. New York: Plenum

Rappard, J.F.H. van (1993b). History in psychology. **Psychologie und Geschichte, 4,** 187 - 196

Rappard, J.F.H. van (1996a). Presentation and presentism. **Paper, 15th Cheiron Conference**, Leiden, Netherlands, 3008-0309

Rappard, J.F.H. van (1996b). Meta matters: A comment on Agatti's proposal on the identity of theoretical psychology. **Theory & Psychology, 6,** 293 - 300

Rappard, J.F.H. van (1997). History of psychology turned inside(r) out: A comment on Danziger. **Theory & Psychology, 7,** 101 - 105

Rappard, J.F.H. van & Strien, P.J. van. (1993). History and theory: Introduction. In J.F.H. van Rappard, P.J. van

Strien, L.P. Mos & W.J. Baker (Eds). **Annals of theoretical psychology, 8,** 1 - 14

Rorty, R (1980). **Philosophy and the mirror of nature.** Princeton: Princeton U P

Strickland, L.H. (1991). Russian and Soviet social psychology. **Canadian Psychology, 32,** 580 - 593

Swerdlow, N.M. (1993). Montucla's legacy: The history of the exact sciences. **Journal of the History of Ideas, 54,** 992 - 328

Wahl, R. (1988). The Arnauld - Malebranche controversy and Descartes' ideas. **The Monist, 71,** 560 - 572

Watson, R.I. (1967). Psychology: A prescriptive science. **American Psychologist, 22,** 435 - 443

Yolton, J.W. (1984). **Perceptual acquaintance from Descartes to Reid.** Oxford: Blackwell

TENDENCIES OF DIFFERENTIATION OF COUNSELLING ACTIVITY IN HUNGARY IN THE POSTWAR PERIOD

by Magda Ritoók
Eötvös Loránd University, Budapest

Counselling work in the traditional sense has been done mostly by psychologists for decades, and it was focused on educational and career selection counselling for the young. Considering its theoretical background and methodological culture, this meant a high-standard and internationally appreciated activity that has had an important founding role in allowing the present-day counselling work to make significant progress within a short period of time.

Counselling as a profession is one of the most dynamically developing activities in Hungary. The number of counsellor specialist has been increased by some new forms of activity and professional training.

To mention just the most important fields, the tasks, institutions and professional types of counselling in Hungary include consultations at general school, secondary school and higher education levels, counselling for high ability children and their parents, family and partner counselling, educational counselling, rehabilitation counselling, crisis-intervention, as well as counselling for refugees, and intercultural, pastoral, and organizational counselling. Peer-counselling is also beginning among secondary school pupils and college and university students.

Earlier counselling focused only on direct interventions at points of need. Counselling activity now is supplemented with prevention and promotion as well. Besides individual consultations, group counselling has also become widespread, and an ever more important role is played by consultation, training and media as supportive tools. High-standard, well-organized supervision is also developing at a speedy rate.

Counselling is no longer been the activity solely of psychologists. Following specific, post-graduate training, professional experts now work in many places in Hungary.

The tendencies listed above are more or less present, not only in Hungary, but also in all Eastern European countries. Besides, in all of these countries, the attitude and the philosophy of counselling have changed: instead of the paternalistic care-taking that once prevailed, counselling today offers help so that people will be able to plan and guide their own lives more independently and with more competence.

1st ESHHS Conference, Szeged, 29 August - 3 September 1997
Roger Smith
Department of History, Lancaster University, Lancaster LA1 4YG, UK.

THE LARGE-SCALE HISTORY: VALUES, CHOICES AND NARRATIVE

The paper will consider the ideal of a 'synthesis', writing which, in the words of the Fontana History of Science series will '[synthesize] detailed research and [bring] out its wider significance'. This sounds straightforward. Yet the goal raises far-reaching questions about contemporary academic culture and its capacity, in epistemological and rhetorical senses, to write general and synthetic as opposed to particular and analytic studies. What theory of knowledge can make possible a historical synthesis, and in what rhetorical terms can a synthesis lay claim to authority? The questions are huge, but I will draw on my recent Fontana History of the Human Sciences as a practical if not necessarily consistent response.

Let me first comment on the negative aspect of the conditions for writing large-scale history: the pressures against it being written at all. In the 1960s and 1970s, the history of science, like the history of ideas generally, moved from its position as handmaiden to natural science and philosophy to autonomous history. The consequence was what may be called 'professional' pressure on historians of science to write about the particulars of knowledge and events in context. Meanwhile, wider intellectual changes fostered scepticism about the authorial stance in terms of which it is possible to write a generalized synthesis. All the explanatory grand narratives to which historians were once attracted have been shown to be inadequate on empirical grounds, or to be conceptually confused, and, as a consequence, many historians now believe that explanation can be undertaken only at the level of particulars. There has also been antagonism on political as well as epistemological grounds to the omniscient author. A grand narrative was likely to be accused of personal bias, ideology or worse, whereas the language of particulars seemed to reflect objective scholarship. Further, it was argued, at least for the subject matter of the humanities, that deconstruction could reveal that language has itself, not something beyond itself, as its subject. 'The linguistic turn' in history appeared to imply that the historical voice cannot but be particular, that we should admire the playful and ironic as opposed to the all-seeing author, and that disruption and discontinuity should displace grand narratives.

These remarks apply to any form of history writing. I now turn to three marked difficulties for writing the history of the human sciences. At an abstract level of analysis, these difficulties are all aspects of the first, 'reflexivity', the irreducible intuition that we are at one and the same time agent and object of knowledge. The philosophical issues are fearsome, and I therefore need to make clear that my purposes are limited, to comment on the choices faced by a synthetic historian of the human sciences. By 'reflexivity' I refer to the way in which consciousness is itself both subject and object as a condition of knowledge of human beings. 'The human sciences', as I understand the term, denotes our attempts to articulate systematic knowledge in terms of that condition..

When knowledge is articulated about the human, the human who possesses that knowledge is a different human from what he or she was before. The philosopher Alasdair MacIntyre wrote: 'Psychology is not only the study of human thinking, feeling, acting, and interacting: it has itself - like the other human sciences - brought into being new ways of thinking, feeling, acting, and interacting.' The human subject will not stay still. In the seventeenth century, there are many texts on the passions - Descartes' Passions de l'âme (1649) is an eminent example - which seek to advance and spread knowledge in order to assist what, since the work of Stephen Greenblatt, is known as the 'self-fashioning' of a virtuous life. The literature of the passions was not, in our terms, about either 'the mind' or 'the body' or 'psychology'; rather, conduct books and guides to a healthy regimen built knowledge of the passions into a circle of reflections. In dialogue with such texts, authors and readers shaped their self-identity and conduct as passionate beings, and hence they changed the subject matter of what the texts were about.

If ways of life, and specifically ways in which identity and subjectivity are constructed, are bound into a circle with knowledge of those ways of life, it appears to follow that the history of ways of life is an essential part of the history of the human sciences. This conclusion then requires us to consider what possibilities there are for historical psychology, the history of subjectivity and self-identity, the history of mentalite and the history of family, childhood, sexuality and all the other headings under which being human and representation of the human have been studied together. There is a vast literature. My response, in order to be practical, was to include only a few areas in my book, with the hope that this would at least show how the history of the human sciences is also a history of the world of subjective identity and feeling. It is a weakness, however, that I have not fully thought through this argument in the way I have structured the topics included in my book.

The history of belief about human nature, the self and the social is bound up with the history of how life is lived. This explains the protean nature of the history of the human sciences. This history merges with history in general, and a distinct history of the human sciences appears impossible.

This arrives at what I list as the second difficulty for a general history of the human sciences. We cannot make decisions about how to delimit the dimensions and scope of the human sciences in a way which everyone will find satisfactory. This may sound a trivial point, but I think it goes deep because it denies to us the possibility of either a unified human science or an agreed history of the human sciences. It is a major practical and intellectual problem of synthetic work on the history of the human sciences that there can be no clear boundaries to the area. This problem is intrinsic to an area of scholarship with a reflexive subject, and it is not only a historical consequence of the elaborate development of specialization and insitutionalised division of scholarly labour between what are now very many disciplines. If what is denoted by 'human' is at one and the same time the subject that knows and the object that is known, any expression of reflexive consciousness, that is, any aspect of culture, is in some sense part of the knowledge which a history of the human sciences might be said to be about. Hence the field of the human sciences, ultimately, cannot be said to be bounded. We can, however, trace historically the social boundaries which are drawn, most obviously in discipline formation and maintenance.

In practical terms, a historical synthesis must chose what to do, and there can be no 'right' answer. I tried to cover a wide range of areas before the present century, in order to argue that the areas now encompassed within the disciplines which we recognize are socially constructed areas. Then, for the twentieth century, overwhelmed by the idea of covering all contenders to the pantheon of the human sciences disciplines (?accountancy), I structured arguments about the debate over what sort of knowledge a human science should achieve around psychology and around the great emphasis on the psychological dimension in modern life.

It is also necessary for historians to see (to see reflexively) that historical scholarship is itself a way of life, a form of life that accepts some rather than other views about being human, and as such historical scholarship takes a stance in the human sciences which is not one which others (natural scientists, for example) who live life in a different way, will accept. A synthetic history must therefore face politics and values.

The writing of history is evaluative. It is necessary to make choices in writing a history of the human sciences because its scope is so vast; but, more critically, these choices will be exposed to criticism because the values behind the selection of what to write about will be exposed to view. All historical writing, like conscious reflection itself, is a selective and evaluative act. In the history of the human sciences, however, the presence of judgments is more conspicuous than elsewhere, more obviously representative of one way of life rather than another.

In some sense, everything bears a relation to us - even knowledge of distant galaxies - through our agency as human subjects, and everything might be said to tell us something about ourselves. But in practical and concrete terms, the historian of the human sciences must ask such questions as whether the human sciences include the management sciences, literary theory, jurisprudence, along with sociology, linguistics, psychology and so forth. Answers to the question are going to be very strongly structured by present interests and purposes. If we try to be liberal and inclusive in our sympathies, the potential subject matter to be covered quickly becomes uncontainable. Even a historian of great liberality must obviously select, that is, include and exclude on the basis of values. On the largest scale, it is necessary to decide how far back in time to go - the Greeks? - and whether the history should include non-Western cultures, since all peoples may in some sense be said to express systematic views about human nature. Selection can be an emotive matter.

It might be thought that the problem lies in the phrase 'the human sciences', the usage of which is recent and unconsolidated, and that it is possible to define the scope of the history of established specialist areas such as psychology and anthropology. This, however, does not help. The historical emergence of the social entities called disciplines - political science, sociology and so forth - is a very uneven process with diverse contents in different countries in the twentieth century, and it began only in the second half of the nineteenth century. Even in the modern period, the disciplines that collectively form the human sciences are diverse indeed. Before this, it is hard to say in contextual and non-anachronistic terms what psychology or sociology was. It was precisely this difficulty which led me, when asked to write a history of psychology by the Fontana Press, to refuse and to propose instead a history of the human sciences. I thought, rightly or wrongly, that the category 'the human sciences' is serviceable because it is an anachronistic umbrella term which will cover whatever areas historians

decide do form part of the historical background to the relevant modern disciplines or subject areas. It will also permit the historian to take on board the reflexive questions about the proper subject matter of a history of the human sciences just outlined.

The term 'the human sciences' is evaluatively loaded in one striking regard, since it is not a term, like 'psychology', which is identified primarily with an area of natural science. As I use the term, 'the human sciences' deliberately leaves open the question of whether or not the knowledge described historically is part of the history of natural science. My history includes such topics as the European encounter with the peoples of the Americas in the sixteenth century, natural law theories of jurisprudence in the seventeenth century and Staatswissenschaft (the science of the state) in the eighteenth century, topics not normally thought to belong to the history 'of psychology', though, as I suggest, such topics contributed much to the conceptualization of human nature. My argument is that if such topics are not included in a history of psychology, the history becomes a projection back of our conceptual frameworks and disciplinary divisions into a time when they did not apply. The general argument holds even if my own agenda of topics to be included is judged unsatisfactory. I find 'the human sciences' a useful term also because I do not know what other term in the English language can be used to group together earlier subject areas such as 'moral philosophy', 'the science of man', 'mental science' and 'the philosophy of mankind', all of which refer to systematic attempts to achieve knowledge of the human.

Whatever the practical and philosophical arguments about the possibility of unification of an area of science called psychology in the twentieth century, reference to the modern occupational diversity of psychologies enhances a historian's imagination about the choices which must be made. During the eighteenth and nineteenth centuries, a number of areas of learning (including experimental physiology and moral philosophy), practical occupations (including psychological medicine and pedagogy), social movements (like evangelicalism and phrenology) and forms of cultural expression (like 'the rise of the novel' and the diary) all contributed to what we identify as the psychological dimensions of human life. This should hardly come as a surprise in a world where people from statistical methodologists to Jungians and from observers of elephants on the African savannah to providers of special aids in schools call themselves psychologists.

Over the eighteenth and nineteenth centuries, it gradually became common to use the word 'psychology' to describe a subject area. The synthetic historian, then, should describe how very many topics or activities, grouped for convenience under the label 'the human sciences', were refashioned in the late nineteenth century into the psychologies of our own century. As one example, I cite the subject of pneumatology, which was taught in the Scottish universities into the second half of the eighteenth century. It was at first the part of the curriculum which covered knowledge of non-material, spiritual substances, and this included both angels and the spirituous substances of men's character. By the time of Thomas Reid's lectures at Aberdeen in 1752, pneumatology was redefined as 'the history of the Human Mind and its Operations & Powers'. The task for the historian is to find a way to describe how pneumatology, and the teaching in moral philosophy concerned with the active and passive mental powers to which it contributed, was reshaped - metaphorically, we might say translated - into the form of knowledge, called psychology, of which Alexander Bain was an eminent representative before he too came to Aberdeen, in 1860.

The third substantial difficulty for the history of the human sciences which I will discuss requires comment on the word 'science'. It is the status of some knowledge as science that has, for many historians as well as scientists, marked out the special province of the historian of science. Whatever the issues which confront the history of the natural sciences, when it comes to the human sciences we cannot avoid asking whether history in this area should cover the long-standing and multi-faceted debate about how the word 'science' is to be understood when the subject of science is man. The historian who does not include an account of this debate takes his or her stance - as a matter of fact, usually the position that psychology is a natural science - about the correctness of one side of what is in dispute. Not to accept that psychology is a natural-scientific domain would seem obtuse or perverse to most academic psychologists, but such an acceptance nonetheless involves values. Once again, reflexivity is the philosophical heart of the debate. What has been at issue is whether there is continuity between the explanatory concepts and methods of the natural and the human sciences, or whether the reflexive character of human existence - expressed as language, culture and history - requires distinct explanations and methods in the human sciences. The debate, thus described abstractly, has taken many concrete historical forms, which my book attempts to describe, and it is still a major contemporary focus of differences of view (e.g., in the philosophy of mind).

Any manner of writing historically on the debate about the relation of the natural and human sciences is an intervention in current debate. The intellectual historian John Burrow put the point simply: 'To write the history of a discipline is to state what the discipline is, and this, in the social sciences, is often highly contentious.' Thus, to write historically about the disputed explanatory forms of the human sciences cannot but at least question whether the history of psychology (or other human science) is a history of the achievement of a natural science of psychology.. I take my stand, even if I do not ground it in an elaborated philosophical argument, on what many will judge an anti-naturalist position. This also creates problems of narrative sturucture, since the achievement of objective natural science is the story line which has given order, direction and manageable content to existing histories of psychology. I must find a different story line, and I attempt to do this by making the debate itself the story.

I want to state this question about history and the relation between the natural sciences and the human sciences in another way. In a natural science mode of explanation, we use language in which we confront our 'nature', even - as natural scientists and common opinion often state - a common 'human nature'. People frequently see themselves as having a given nature. It is clearly possible to write history about what this given nature has been understood to be, from the humoral theories of the Renaissance, through the literature of sensibility, the moral sense and pleasure-pain associations of the eighteenth century, to the drives, traits and cognitive functions of recent times. This 'nature', however, has been, and in the humanities and cultural studies disciplines is, often understood differently: our 'nature' is thought of as self-created, an achievement of reflective consciousness articulated through symbol systems, an achievement of time and of human history. This is the way of thought which the Neapolitan scholar Vico expressed in the first half of the eighteenth century, which was taken up by the German philosophers of history from Herder to Marx, and which - transmuted - returned in the post-Nietzschean, post-structuralist permutations of recent decades.

What sustains argument in favour of an approach to human nature as a self-creation is, once again, reflexivity: the argument that the way we live creates beliefs about what we are, while beliefs create the way we live. It follows from this position, I believe, though it is a strong claim, that the history of the human sciences is knowledge of what we are, that it is a contribution to the human sciences themselves. Of course, it is not a contribution to a natural science of the human. The argument, perhaps I should add, does not exclude the possibility of a natural science of the human, but it does exclude claims that would give natural-scientific knowledge exclusive status.

I have outlined three difficulties which face a synthetic history of the human sciences: reflexivity and hence the need to make clear the relationship between the history of knowledge and the history of human experience and identity; the protean, boundary-less nature of the domain; and the centrality of debate about the nature of 'science' in relation to the human subject. These formidable issues might be thought sufficient to deter anyone from writing a work of history to which they are intrinsic. Yet I am not sure that they are any more severe than those which mainstream historians face all the time, as when, for example, they account for the sources of the Bolshevik Revolution or describe the changing world of childhood. If there is a difference, it is that many other areas of history have established conventions and models, even if they are subject to criticism, for the large-scale narrative. Whatever the standards prevailing in history departments about the requirement for scholarship comprehensively to satisfy the correspondence criterion of truth (that is, to be congruent with the available primary source evidence), narrative conventions are on hand to help with selection, generalization, causal attribution, moral judgment, and so on. By contrast, there are no histories of the human sciences, not surprisingly, since the term is new and contestable. There are histories of fields or disciplines (like psychology), of philosophical themes (like the normative contribution of the concept of 'function' to social science) and of practices (like the prison). And of course there is a rapidly expanding body of scholarship on specific topics. The challenge, then, is to find a narrative form in which all of this can be brought together. Rhetoric and philosophy, form and content, must consciously be articulated together.

Note: This paper draws upon Roger Smith, The Fontana History of the Human Sciences (London: HarperCollins, 1997), published in the U.S. as The Norton History of the Human Sciences (New York: W. W. Norton, 1997); the volume is a title in 'The Fontana (or Norton) History of Science Series' (general editor, Roy Porter). The book contains an extensive 'Bibliographic Essay' in which further references may be found.

16th Conference European Society for the History of the Human Sciences (ESHHS)
Budapest - Szeged 1997

Abstract

"Postcolonial" constructions of "cultural others" in the human sciences

Irmingard Staeuble, Freie Universität Berlin

So far, in my historiographic studies of the constructions of non-European "others" in colonial discourse and of subsequent postcolonial revisions, I have been concerned with three interrelated questions: what has been the contribution of the human sciences to imperial construals of non-European "others"? how, since decolonization, have these construals been revised, and what do the revisions contribute to an an "intelligible discourse" (Geertz) on an international scale.

In this paper, I will first discuss some historiographic problems involved in attempts at a critical historiography of post-/neocolonialism and the human sciences. I will argue that, much as the historiography of colonialism and the human sciences is becoming more self-reflective, one hardly finds a hint at appropriate strategies for tackling the reinventions of scholarship concerning "others" that have marked the scene since decolonization. Second, I will provide evidence of "postcolonial" constructions of cultural "others" in order to specify the problems outlined in the first part and to indicate my skepticism with regard to progress in the "decolonization" of the human sciences.

Paper prepared for
16th Conference European Society for the History of the Human Sciences (ESHHS)
Budapest - Szeged 1997

"Postcolonial" constructions of "cultural others" in the human sciences

Irmingard Staeuble, Freie Universität Berlin

So far, in my historiographic studies of constructions of non-European "others" in colonial discourse and postcolonial revisions in theorizing others (Staeuble 1992; 1993; 1995a; 1995b), I have been concerned with three interrelated questions

1 - what has been the contribution of the human sciences to imperial construals of non-European "others"?

2 - how, since decolonization, have these construals been revised, and

3 - what revisions may enable us to explore the horizons of the present with regard to "intelligible discourse between people quite different from one another in interest, outlook, wealth, and power, and yet contained in a world where ... it is increasingly difficult to get out of each other's way" (Geertz 1988, 147).

In this paper, I will first discuss some historiographic problems involved in attempts at a critical historiography of colonialism, post-/neocolonialism, and the human sciences. Second, I will provide a sketch of "postcolonial" constructions of cultural "others".

I. Toward a critical historiography of (neo)colonialism and the human sciences

As to the question of imperial construals of non-European "others", critical historiography of colonialism and the human sciences has provided ample evidence of a continuing text produced by the West about its "other" in a way that justifies taking possession of non-Western life worlds. "Imperial Eyes" (Pratt 1992) set upon foreign worlds saw them in need of European culture, Orientalist accounts stated their lack of "civil society" (Said 1978), anthropology hastened to fill in the cartography of "vanishing primitives", and ethnopsychological devices for testing the mental capacities of "primitives" served to sort indigenous labour force for colonial administrators (Probst 1992).

A selfreflective glance at the strategies used in writing about colonial history, meaning, and agency has found colonial historiograph itself in need of amendment or radical revision. Thus Martha Kaplan noted a strategy that finds "any scholarship concerning >others< so intricately implicated in western categories or in the mechanisms of colonial domination that concepts of >culture< and >cultural difference< themselves become artifacts of colonial categorizing", and an opposite strategy, widespread in anthropology, that "insists on the priority of cultural difference" and "produces narratives which insist on local categories of meaning and local agency for an understanding of encounters with the world system or colonizing peoples" (Kaplan 1995, 2). Her suggested amendment, in case studies of "cargo cults" that are *neither cargo nor cult*, emphasizes the polyphonous ambiguity of colonial situations.

Diversity of "colonial situations" is also stressed by Stocking (1991), though in the afterword to Stocking's collection Talal Asad refocuses "the story of anthropology and colonialism" as "part of a larger narrative which has a rich array of characters and situations but a simple plot":

> When Europe conquered and ruled the world, its inhabitants went out to engage with innumerable peoples and places. European merchants, soldiers, missionaries, settlers, and administrators - together with men of power who stayed at home, they helped transform their non-European subjects, with varying degrees of violence, in a "modern" direction. And of course, these subjects were not passive. The story recounts how they understood initial encounters with Europeans in indigenous cultural terms, how they resisted, adapted to, cooperated with, or challenged their new masters, and how they attempted to reinvent their disrupted lives. But it also tells of how the conditions of reinvention were increasingly defined by a new scheme of things - new forms of power, work, and knowledge" (1991, 314). → Asad

With the hint at a "new scheme of things" Asad pleads for shifting the focus toward an attempt at understanding "the radically altered form and terrain of conflict inaugurated by" Western hegemony - "new political languages, new powers, new social groups, new desires and fears, new subjectivities", suggesting closer inquiry of "the role of Western technologies in transforming colonial subjects" and of "Western techniques for governing subjects" (Asad 1991, 323).

Much as critical historiography of colonialism and the human sciences is being revised, one hardly finds a hint at what strategies are considered appropriate for tackling the revisions and reinventions of scholarship concerning "others" that have marked the scene since decolonization. If critical historiography of the human sciences is to enable us "to think *against* the present, in the sense of exploring its horizons and its conditions of possibility" (Rose 1996, 122), postcolonial theorizing of cultural selves and agency is still in need of historiographic rethinking.

Considering that the "end of empire" has not marked an end of economic dependency nor of imperialism, talk about postcolonial scholarship obviously need qualification. As to scholars, their attempts at disentangling theorizing from the webs of colonial discourse or Eurocentrism

may not suffice to qualify as "postcolonial" as long as neocolonial or imperialist features of the context of knowledge production are overlooked. As to "scholarship concerning others" several questions arise concerning the sociocultural constitution and reconstitution of colonized subjectivities, the configurations of talking and listening in communication between Euro-Americans and non-Euro-Americans, and the very choice of making cultural others or the interpretation of cultures an object of scholarship. Would not equal chances of participation in international production of knowledge concerning mutual selves and others be the ultimate criterion of any scholarship deserving to be called "postcolonial"?

II. Colonialism and its aftermath: Revised constructions of "cultural others"

It is by now a well-known story that decolonization brought about a crisis of anthropology (Stocking 1983), discrediting the inventions of "primitive society" (Kuper 1988), "primitive mentality", and "people without history" (Wolf 1984), and casting doubt upon the innocence of ethnographic writing itself. Subsequent reinventions of anthropology tried to reconceptualize object, goal, and method. At the same time, new chapters of developmental sociology, globalism, and hyphenated disciplines like ethno-science or ethno-history have been opened. Likewise, the contrast of "tradition" and "modernity" has been deconstructed (Lutz & El-Shaks 1982), and orientalist and occidentalist modes of representation have given way to the production of interlocking stories of "intersecting territories" (Said 1993) and "intersecting lives" (Gewertz & Errington 1991).

Yet is it possible to read this combination of revisions as a discernible project of decolonization of the human sciences? I am afraid I do not think so. With my account of some episodes of the changing constructions of "cultural others" I would like to indicate the reasons for my increasing skepticism.

"Mental colonisation" versus "individual modernity": non-European others in social macrotheory

When around 1960 the struggles for independence had resulted in the creation of about 150 new states which soon became referred to as the "Third World", social macrotheory moved to the fore revising Orientalist "othering" in favour of the "master ideology" (Horowitz) of **modernization and development.**

In the liberal model of "modernization" development was seen as a linear path toward the Euroamerican model of modernity, and modernity as a consistent whole comprising similar patterns of economy, government, value systems and stratification. The requisites of the

functioning of Western society as conceptualized in goal-directed terms of social integration, political stability and economic growth, were turned into prerequisites of development elsewhere.

This model continues to provide the basis for UN developmental politics and also, though increasingly deliberalized for the sake of gearing the Third World toward Western economic interests, for World Bank and International Monetary Funds directives.

Studies of the subjective dimension based on both the modernization model and David McClellands theory of achievement society were carried out in 1960s in the Harvard-Stanford project on "Social and Cultural Aspects of Modernization" as documented in the comparative study Becoming Modern (Inkeles & Smith 1974) and Alex Inkeles' sequel Exploring Individual Modernity (1983). An analytical model of the psychosocial characteristics of individual modernity, "derived from a theoretical consideration of the requirements of factory life" (Inkeles 1983, 39) and translated into attitude scales, provided the measuring rod for a comparison of people in developing societies (Argentina, Chile, India, Israel, Nigeria, and East Pakistan), in terms of their sense of efficacy, openness to new experience, respect for science and technology, acceptance of time discipline, and an interest in planning.

For social scientists and development workers interested in the "human potentialities" of "men in crisis", the modernization model was unsuitable. As observed by Gerrit Huizer, it implied that people in South America or Africa were poor mainly because they lacked "achievement motivation" or were irrationally "fatalistic" or "apathetic" (1991, 43).

In a substantial critique of the concept of "individual modernization", Hans Bosse argued that it conceals the destructive effects of the global process of "internal colonization", i.e. the mental expropriation of collective life histories by means of educational politics, modern mass media and communication technologies. Having analyzed the function of educational developmental politics in the transnationalization of capitalist culture (Bosse 1978), he developed an ethnohermeneutic approach to the study of the subjective dimension of "internal colonization" (1979; 1981). Ethnohermeneutics draws on the ethnopsychoanalytic approach developed by the Zurich group of Paul Parin, Goldy Parin-Matthey, and Fritz Morgenthaler and was designed to reveal the psychological dynamic of human agency in the inferiorized's reinventions of their disrupted collective biographies.

Liberating "the oppressed": the Marxist and feminist impact

In response to the growing sense of crisis in anthropology, radical critics called into question its institutionalization within the academy, "turning toward Marxist theory for the first time in its

(=anthropology's) history" (Stocking) and advocating an active political involvement on behalf of its subjects. Their option for an unconcealed political project - as against the concealed politics of "scientific neutralism" - and the claim to speak from the viewpoint of the oppressed - rather than from the viewpoint of objective rationality - brought human agency center stage. If human agency made history, and if the world was to be geared toward socialism, emancipation of the oppressed people of the world was of central importance. Committed scholars turned field research into action research trying to figure out traces of resistance against social class formation, to support local groups in projects of their own design, or to do participatory analysis of the alienating effects of schooling in Third World countries.

When radical actionism began to falter, due to both limited access to the field and objections from those to be liberated - for instance, black women resisting "white middle class ideas of emancipation", Marxist and feminist debates in the academy took on a more epistemological turn. From hindsight, Pels & Nencel (1991, 19) observed that the project of the emancipation of the oppressed implied an untenable epistemological assumption of a universal object - both the subordination of the peoples of the world under capitalism and the universal subordination of women. In Foucauldian terms, the "claim to speak from the viewpoint of the oppressed did conceal a politics of knowledge, a claim to authority which gave Western academics the power to define problems and solutions" (Pels & Nencel, 10-11).

Monita voiced by Third-World scholars

Since the late 1970s, indigenous Third World scholars have occasionally managed to articulate their problems with anthropological constructions of cultural others. The Tonganese anthropologist Epeli Hau'ofa, for instance, criticized that

> ...we have come up only with pictures of people who fight, compete, trade, pay bride-prices, engage in rituals, invent cargo cults, copulate and sorcerise each other. There is hardly anything in our literature to indicate whether these people have any such sentiments as love, kindness, consideration, altruism and so on. We cannot tell from our ethnographic writings whether they have any sense of humour. We know little about their systems of morality, specifically their ideas of the good and the bad ... We have ignored their physical gestures, their deportment, and their patterns of non-verbal communication. By presenting incomplete and distorted representations of Melanesians we have bastardised our discipline, denied people important aspects of their humanity in our literature, and we have thereby unwittingly contributed to the perpetuation of the outrageous stereotypes of them made by ignorant outsiders who lived in their midst" (1975, 286).

Most complaints, however, concerned both, problems of indigenous research in a disadvantaged academic surrounding (cf. Morauta 1979, 563; Chilungu 1984), and the structures of international knowledge production that prevent their participation (Fahim 1979).

In 1984, the Kenian anthropologist Simeon Chilungu provided a list of Third World scholars contributing to international journals that includes 1 author each from China, Indonesia, Papua New Guinea, Egypt, Ghana, and Mexico; 2 from India, Uganda, South Africa and Nigeria; and 3 from Kenia.

It is yet to be systematically researched what efforts have been made by critical human scientists to alter this situation. But the fact that the situation of Third World scholars is, up to today, hardly ever referred to by Western scholars, casts doubt on any awareness of the essential role of the international conditions of knowledge production.

What flourished instead, were reinventions of anthropologyby Western scholars.

Culturally diverse constructions of person, self, and feeling

Since the 1980s, new approaches to the **cultural diversity of person, self, and feeling** have made a noticeable impact on cultural anthropology (M. Rosaldo 1980, 1984; White & Kirkpatrick 1985; Lutz 1988). Inspired by the linguistic turn, their ethnographies were based on a discursive concept of culture which suggests that notions of persons or feelings are not words for some universal natural things that can be easily translated from language into another, but rather sociocultural construals closely interwoven with everyday social life.

For instance, Catherine Lutz's study of Ifaluk emotions and everyday life conveys how the emotional mind with which the Ifaluk endow themselves ideally serves not simply to understand the world but to act in it; how Ifaluk thought and motivation, word and deed form relatively seamless units; and how their cultural values are explicitly included in their views of persons, selves, and others.

Unlike emotion words in the US, which relate to inner states of feeling, Ifaluk emotion words relate to interactions between persons or between persons and events. Cultural diversity - thus the general message - exists not only in the contents of self-awareness and person concepts but also in the degree to which this awareness is itself monitored, emphasized as salient, and explicitly discussed in everyday discourse (36).

In such studies, the habit of taking Western encodings of subjectivity and social interaction to be "natural givens" is explicitly made a topic of reflection. The scholars articulate their cultural heritage of psychological models in order to beware of using them unreflectedly.

This is certainly a substantial move toard de-Eurocentricizing constructions of "cultural others". On the other hand, however, no attempt is made to tackle the impact of rapid social transformation on people's notions of self. Thus the ethnographies of self and feeling convey a nearly timeless image of the societies portrayed - an image that oddly reminds of classical

ethnography's essentializations of culture.

Social change, cultural selves and agency

Among the few studies that put the impact of rapid transformation of the life worlds in Third World countries like Papua New Guinea centre stage, the contributions from urban ethnologists like Paula Brown (1988) and Florence Weiss (1991), the accounts of both Chambri "twisted histories" and "altered contexts" by Deborah Gewertz & Frederick Errington (1991) and coping strategies with "hard Times on Kairiru Island" by Michael French Smith are especially noteworthy. For instance, Gewertz & Errington (1991), after twenty years of field research trying to represent the Chambri in a world system, write a sociohistorically contextualized "collective biography" to convey the particuliarities of Chambri lives in transition, of their transactions with tourists, of the extension of their life world to the city and its impact upon their views of the wantok system of mutual support, of their relationships to neighbour communities and their attempts to protect themselves from the state.

Studies like these display sensitivity to discursive interactions, versatility in social and historical contextualisation, and a thoughtful stance vis-à-vis neocolonial inequalities of power. One can at least imagine that the people portrayed could gain from reading their portrayals.

Still, "the chance of intelligible discourse between people quite different from one another in interest, outlook, wealth, and power" will at last depend on more dialogic exchange between scholars at home and abroad. Renewed efforts of analyzing the international structures of knowledge production would thus seem essential - in order to instigate change.

Asad, T. (1991) Afterword: From the History of Colonial Anthropology to the Anthropology of Western Hegemony. In G.W. Stocking (ed.) Colonial Situations. Madison: The University of Wisconsin Press.

Bosse, H. (1979) Diebe, Lügner, Faulenzer. Zur Ethno-Hermeneutik von Abhängigkeit und Verweigerung in der Dritten Welt. Frankfurt: Syndikat

Bosse, H. (1981) Zur Kolonisierung der inneren Natur. In Schülein/ Rammstedt/ Horn/ Leithäuser/ Wacker/ Bosse, Politische Psychologie. Entwürfe zu einer historisch-materialistischen Theorie des Subekts. Frankfurt: Syndikat.

Bosse, H. (mit Werner Knauss) (1994) Der fremde Mann. Jugend, Männlichkeit, Macht. Eine Ethnoanalyse. Frankfurt: Fischer

Brown, P. (1988) Gender and Social Change: New Forms of Independence for Simbu Women. Oceania 59: 123-142.

Chilungu, S. W. (1984) Alternative Ethnologie aus der Dritten Welt. In E. W. Müller, R. König et al. (Hg) Ethnologie als Sozialwissenschaft. Köln: Westdeutscher Verlag.

Errington, F. & D. Gewertz (1991) Articulating Change in the "Last Unknown". Boulder: Westview Press.

Fahim, Hussein (ed) (1982) Indigenous Anthropology in Non-Western Countries. Proceedings of a Burg Wartenstein Symposium. Durham: Carolina Academic Press.

Geertz, C. (1988) Works and Lives, the Anthropologist as Author. Stanford: Stanford University Press.

Gewertz, D. & F. Errington (1991) Twisted Histories, Altered Contexts: Representing the Chambri in a World System. Cambridge: Cambridge University Press.

Hau'ofa, E. (1982) An anthropologist at home. In F. Hussein (ed) Indigenous anthropology in non-Western countries. Durham, N.C.

Hau'ofa, Epeli (1975) Anthropology and Pacific Islanders. Oceania 45(4): 283-289.

Healey C. J. (1989) Introduction: The anthropology of development in Papua New Guinea. Canberra Anthropology 12 (1&2). Special Volume Culture and development in Papua New Guinea.

Huizer, G. (1991) Anthropology of Crisis. In L. Nencel & P. Pels (eds) Constructing Knowledge: Authority and Critique in Social Science. London: Sage.

Inkeles, Alex & D.H. Smith (1974) Becoming Modern: Individual Change in Six Developing Countries. Cambridge: Harvard University Press.

Inkeles, Alex (1983) Exploring Individual Modernity. New York: Columbia University Press.

Kaplan, M. (1995) Neither Cargo nor Cult: Ritual Politics and the Colonial Imagination in Fiji. Durham: Duke University Press.

Kuper, A. (1988) The Invention of Primitive Society. London: Allen Lane.

Leclerc, Gérard (1973) *Anthropologie und Kolonialismus*. München: Hanser.

Lutz, C. A. (1988) Unnatural Emotions. Everyday Sentiments on a Micronesian Atoll & Their Challenge to Western Theory. Chicago: The University of Chicago Press

Lutz, J.G. & El-Shaks, S. (eds.) (1982) Tradition and Modernity. The role of traditionalism in the modernization process. University Press of America

Morauta, L. (1979) Indigenous Anthropology in Papua New Guinea. Current Anthropology 20, 561-576

Pels, P. & L. Nencel (1991) Introduction: Critique and the Deconstruction of Anthropological Authority. In Nencel & Pels (eds) Constructing Knowledge: Authority and Critique in Social Science. London: Sage.

Pratt, M. L. (1992) Imperial Eyes. Travel Writing and Transculturation. London: Routledge.

Probst, P. (1992) Angewandte Ethnopsychologie während der Epoche des Deutschen Kolonialismus. Psychologieund Geschichte 3 (3/4): 67-80.

Rosaldo M. Z. (1984) Toward an anthropology of self and feeling. In R. Shweder & R.A. Le Vine (eds) Culture theory: Essays on mind, self, and emotion. New York: Cambrisge University Press.

Rosaldo, M. Z. (1980) Knowledge and Passion: Ilongot Notions of Self and Social Life. Cambridge: Cambridge University Press.

Rose, N. (1996) Power and subjectivity: Critical history and psychology. In C.G.Graumann & K. Gergen (eds.) Historical Dimensions of Psychological Discourse. Cambridge University Press.

Said, E. (1993) Culture and Imperialism. New York: Alfred A. Knopf.

Smith, Michael French (1994) Hard Times on Kairiru Island: Poverty, Development, and Morality in a Papuan New Guinean Village. Honolulu: University of Hawaii Press.

Staeuble, I. (1992) Wir und die anderen - ethnopsychologische Konzepte im Wandel. Geschichte und Psychologie 4: 140-157.

Staeuble, I. (1993). Kolonialismus, Ethno-Psychologie, und die schwierige Vorstellung eines Miteinander auf der Welt. In: H. Givsan & W.Schmied-Kowarzik (Hg.) Reflexionen zur geschichtlichen Praxis, 65-91. Würzburg: Königshausen & Neumann.

Staeuble, I. (1995) Subjektivität im post-/neokolonialen Wandel. Eine Herausforderung an Kulturanthropologie und Ethnopsychologie. In: Jaeger/Staeuble/Sprung/Brauns (Hg.) Psychologie im soziokulturellen Wandel - Kontinuitäten und Diskontinuitäten. Peter Lang Verlag

Staeuble, I. (1995) Becoming Modern amidst Global Change: Postcolonial cultural transformations as viewed by Western and non-Western social scientists. In: R. Stachowski & A. Pankalla (eds) Studies in the History of Psychology and the Social Sciences, 172-178. Poznan: Impressions.

Stocking, George W. Jr. (ed) (1983) Observers Observed. Madison: The University of Wisconsin Press.

Stocking, George W. Jr. (ed) (1991) Colonial Situations. Madison: The University of Wisconsin Press. (History of Anthropology, vol. 7)

Weiss F. (1991) Frauen in der urbanethnologischen Forschung. In B. Hauser-Schäublin (Hg) Ethnologische Frauenforschung. Berlin: Dietrich Reimer Verlag.

White, G. & J. P. Kirkpatrick (eds) Person, Self, and Experience. Exploring Pacific Ethnopsychologies. Berkeley: University of California Press.

Wolf, E. (1984) Europe and the People without History. German Transl. Frankurt: Campus 1986.

Herbart´s views on the freedom of the personality

Renate Topel

Renowned philosophers from time immemorial have dealt with theories of freedom: ideals and utopias of freedom were and are leitmotives of countless revolutionary movements; growing children and young people strive for freedom. But what does the term "freedom" really imply? Is the independence of thinking and action at all possible and how can it be attained? Questions of this sort concern us just as much as Herbart, who experienced the stormy period of the French Revolution, the foreign rule of Napoleon, the war of liberation and the restoration of princely hegemony.

Herbart`s doctrine on the freedom of the act of will was developed in particular by having a critical look at the theories of Spinoza, Fichte and Kant.

According to Spinoza a human isn´t free but subjected to the laws of nature. Acting instinctively is to him synonymous with acting with reason. Herbart also advocates a deterministic concept of human action, nonetheless he is decidedly against being called a follower of Spinoza. „Spinoza had a natural view and wrote a set of moral principles." (Herbart 1989b, p.475). Herbart´s judgement is that this can only be the "aimless chatter of a blind man about colours" (1989c, p.258). Because there is no good and no evil in nature he is against all attempts to apply concepts of nature at random onto the human act of will. Only the human ability to make a judgement makes it possible to think and to act in a way corresponding to a particular set of values.

Fichte demands that an independent being act creatively and be able to form and to control his environment. Independent decisions require a particular ability for thought, a lack of freedom arises through unsatisfactory use of this capability. Herbart sharply observed that Fichte acknowledged a "malleability of intellectual ability" through which "acts of thinking are determined by an external causality" (1989b, p.477). However he is able to acknowledge inactive thought as being free. In his opinion determining what is good and evil is up to the human being according to the situation and it cannot be reduced to questions of causality. In real life varied, dark and illuminating degrees of thinking can be found. Therefore the freedom of the will should not only be limited to advanced thought-processes.

Herbart is also a strong critic of Kant´s term of transcendental freedom. Kant had compared the physical world controlled by the laws of nature with the mental world , the realm of intelligible freedom. He thus defined the categorical imperative as the foremost maxim of free action, the willingness to consider every rational being as an end in itself and not to abuse others as a means to one´s own ends. The severity of the moral law is incontestable to Herbart. However Herbart is a realist and he asks himself whether a "general legislation is something so comprehensible, so obvious to general intelligence, that the term and its´ application can be adopted everywhere where moral behaviour is called for" (1989b, p.489). His doubt is not only raised through the formulation of Kant´s maxims, he is also dissatisfied with the lack of time reference." Kant´s timeless transcendental freedom floats through time and space" (Herbart 1889c, p.212), and thus the malleability of individuals , a change in the way of thinking gained from past experience, remains unconsidered in Kant´s picture of the human being.

Herbart´s concept of the freedom of the will (1989a,b,c,d) takes as its´ starting point the question of why the word "freedom" exercises such a power of attraction on every individual? In everyday life the human being is faced with many burdensome demands

which are sometimes difficult to overcome. Under the pressure of these obligations freedom appears very often in fantastic images . Herbart gives us something to think about when he talks about common everyday views. Actually the human being who " considers himself not free... is really not free and if he takes his freedom it doesn't mean that he is really free"(1989c, p.224).

Young people make many decisions which later prove to be false. An inexperienced child possesses only a very narrow time perspective as well in relation to adults and can only estimate small results independently from more refined alternatives to actions. This in no way means that Herbart advocates an external determination of the act of will. In spite of the limitation of external freedom there is an inner freedom, a resolved individual will which doesn't take account of external attitudes (Herbart 1989, p.222). This inner realm, the transcendental freedom, was to Kant an objectless freedom, since every object connection leads to the heteronomy of the will. This view in no way wins Herbart's approval. It's clear to him that a will had to be first formulated in order to become a will. Superior mammals, and even more so children, display a promising agility and ease when in contact with changing objects which can be recognised as being a precondition for later freedom of choice and decision. Dependent on the educational level, experience gained, the strength of character, the personality submits itself to objects or uses the object world incresingly according to its own needs. „The richest mind has the best prospects of reaching a future possible goal , he changes objects most easily, deals with difficulties most deftly, overcomes obstacles most cleverly, he would call himself especially free. And we won't accuse him of shortsightedness since shortsightedness otherwise creates an assumption of freedom where someone isn't aware of his limitations" (Herbart 1989c, p.226).

The human being lives in a community; the actual freedom of an individual dare not limit the freedom of others. "The moral law is the law of freedom and so to speak the essence of it" (Herbart 1989b, S.496). The dignity of the individual is shown first of all in virtuous and moral freedom. Herbart considers virtue as the ideal of moral freedom. In real life we find many simple expressions of this virtue, for example, fundamental resistance to affectation and inclinations, foolish hopes and unrestrained fantasies. In spite of good principles, apathy, dislikes, prejudices and unauthorised partisanship gain the upper hand over our behaviour. Vice arises "where freedom expresses itself without asking virtue for permission" This is Herbart's judgement on such incidents (1989c, p.234). He pays particular attention to the development of virtuous ways of thinking.

Something which Herbart dislikes is an inherent ability for desire, just as a given, a priori intelligble will which controls the desires like a tyrant; a familiar consideration in Wollf's psychology. The demanding will is also not a slave to emotion, it gains its authority rather more through being able to make aesthetic judgements, original value definitions concerning existing circumstances without having a will. Even a child can tell good and evil, usefulness and worthlessness, beauty and ugliness, truth and falseness apart, it has aesthetic or moral judgements at its' disposal in order to form its relationship to the concrete and social environment. Moral insight grows if moral principles gradually develop out of individual judgements in the process of growing up. However in order to act in a virtuous way it's not enough in the end to concentrate on Kants'categorical imperative. The practical principles hidden in this imperative must rather become the main driving force behind the ability to judge morally. Herbart formulates five practical ideas following Plato and the Stoic School : the ideas of inner

freedom, of perfection, of goodwill, of law and of reprisal, which as formal principles in their entirety should form the basis of all moral decisions in intra- and interindividual situations of conflict (Herbart 1989a; Benner & Schmied-Kowarzik 1986).

With the *idea of inner freedom*, of the individual remaining loyal to his convictions (Flügel 1907, p.79), a harmonious relationship is referred to between insights which have been gained through making judgements and the attitude of the will. Mere obedience without developing individual judgement damages the personality just as much as acts which contradict insights already gained.

The *idea of perfection* has the human being in mind who is steadily working at his moral development, who pursues his goals energetically, stubbornly and consistently, who has a variety of alternative actions at his disposal and who is capable of concentrating on a goal by finding different ways of gaining this goal. It presupposes a variety of interests which first enable all-embracing and practical judgements to be made on existing circumstances (cf. Herbart 1976).

The *idea of goodwill* provides guidelines for relationships to be formed interpersonally. Every human being should meet other people without having any prejudices and also give them support without expecting thanks, admiration or personal gain. Since goodness is „directly good and good to a strange will without having a motive"(Herbart, 1989a, p.363).

In the *idea of law* conduct in conflict situations is addressed. Because „disputes cause discontent" (Herbart, 1989a, p.366), agreements should be reached in good time which don't obviously recognise the law of the stronger but consider the parity of conflicting interests.

The *idea of reprisal* is devoted to acts which are carried out intentionally against the rules, which result in acts of charity or misdeeds. The reprisal for an injustice begins with the sufferer who approves the correct measure of punishment if he acts accordingly to all of the practical ideas or principles (Herbarts 1989a; Flügel 1907).

The practical ideas determine each other. So the term of goodwill as well as inner freedom and the stiving towards self-perfection are composed out of each other. A virtuous way of thinking is shown when all of Herbart's postulated practical principles merge together into an inseparable whole in the character of the personality.

Moral basic convictions only express themselves incompletely in actions. Nonetheless the attempt to control the behaviour of adolescents primarily through strict rules and lists of duties is taking the wrong track. In Herbart's understanding the inner freedom should alone restrict the external freedom. "The wise man thinks himself king but not because of a power which he doesn't possess but because he wants nothing that he can't reach (Herbart 1889c, p.279). The independance of action is then reached when the use of the practical ideas is adopted in narrow contact with a resolved will.

The area within which the practical ideas are operative are limited by Herbart. to intrapersonal activities and interpersonal relationship between two people. However he is convinced that their analyses are enough to draw far-reaching conclusions on larger communities. Social conditions are however subjected to their own lawfulness, therefore there remains in Herbart's concept many questions which are unexplained which concern the free possibilities of development of humans in society. A concrete example of this is an extremely trying experience which happened in Herbart's last working years when King Ernst August of Hannover expelled the Grimm brothers and five further prominent professors from Göttingen who protested against the abolition

of the liberal constitution from 1833. While Herbart's friends praised the integrity of his moral position, also in this complicated situation, the students boycotted his lectures because he refused to give his support to the Göttingen Seven.
In the traditional argument about freedom and lack of freedom of the human act of will Herbart holds both extreme positions to be unacceptable. He demands a determinism which is compatible with freedom. Determinism and freedom have to conclude peace he demands (Herbart 1989c, p.305). In this sense a passive determination of behaviour is refused which postulates an inherent ability for desire and an intelligble will controlled by it. Disposition and environment may form the framework of the development of the personality, but the primary driving force of the development is the subject itself according to Herbart. Every developed character is determined through his activity. This activity is shown in the moral character through an increasing ability to self-autonomy. The independence of action is tied to a life long development of the ability to make a moral judgement.

References:

Benner, D. & Schmied-Kowarzik W. (1986). Theoriegeschichtliche Quellentexte zur Pädagogik. Johann Friedrich Herbart. Systematische Pädagogik. eingeleitet, ausgewählt und interpretiert von D. Benner. Stuttgart: Klett.
Flügel, O. (1907). Herbarts Lehren und Leben. Leipzig: B.G.Teubner.
Herbart, J.F. (1976). Ausgewählte Schriften zur Pädagogik, ausgewählt, eingeleitet unde erläutert von f. Hofmann unter Mitarbeit von Berthold Ebert. Berlin (DDR): Akademie.
Herbart, J. F. (1989a). Allgemeine praktische Philosophie. In: Johann Friedrich Herbart - Sämtliche Werke (Band 2, S.329-458). Hrsg. von K. Kehrbach u. O. Flügel. Aalen: Scientia.
Herbart, J.F. (1989b). Gespräche über das Böse. In: Johann Friedrich Herbart - Sämtliche Werke (Band 4, S.449-510). Hrsg. von K. Kehrbach u. O. Flügel. Aalen: Scientia.
Herbart, J.F.(1989c). Zur Lehre von der Freyheyt des menschlichen Willens. In: Johann Friedrich Herbart - Sämtliche Werke (Band 10, S.207-313). Hrsg. von J.K. Kehrbach u. O. Flügel. Aalen: Scientia.
Herbart, J.F.(1989d). Analytische Beleuchtung des Naturrechts und der Moral. In: Johann Friedrich Herbart - Sämtliche Werke (Band 10, S.315-460). Hrsg. von J.K. Kehrbach u. O. Flügel. Aalen: Scientia.

Zsuzsanna Vajda dr
University of Attila József, Szeged
Hungary

A psychoanalitically oriented psychologist`s views on education in the totalitarian regime (A case study of Alice Hermann) 1895-1975 X Imre Hermann

My paper is a sort of a case study: I am going to present works of a member of the Budapest School of psychoanalysis, Alice Hermann, in the fifties. The era is the "darkest" time of totalitarianism. It is not easy to decide, can we speak at all about any kind of progress in the science or spiritual life. It is wellknown that political ideology intervened directly into all kinds of professional fields; psychology and psychoanalysis became unacceptable, and soon strictly restricted.

But it is undoubtedly not without interest, how these truly extreme conditons influenced thinking and professional role taking of experts and professionals in human sciences, who remained from the vively intellectual life of the interwar period. A part of former Budapest School of psychoanalysis also came back to Hungary after emigration and deportation. Most of them became immediately members of Communist Party. In spite of all these facts Hungarian psychoanalitical society was dissolved in 1948. /Harmat, 1986/

We must to take into account that in the interwar period marxism and psychoanalitical theory did not seem to be contrasted to each other, and even several followers of both school of thought tried to integrate them. On the other hand members of Budapest School of psychoanalysis acquired high reputation and popularity among intellectuals, mostly left oriented ones, who partly became participants of the new regime - true, not for a long time. Some members were even participants of illegal communist movement before and during the war.

Since psychoanalysis was the subject of complete refusal and prohibition in Sovietunion, formally it could not be exist in Hungary as well. But its representatives were not pursuited, moreover offered positions in various institutions from state security to medicine and education, where they were not allowed openly represent their pro-psychoanalytic views. Thus the offer was not accepted by everyone of them. Some former psychoanalysts tried to transfer his/her knowledge by informal channels, in private flats, in secret. Some others hid themselves in silence, there were some who went through the border risking their life, and others accepted some way of cooperation. Alice Hermann`s choice was the last. She started - or better went on - to work for preschool education.

Alice Hermann - born Czinner - was born in 1895. She came from a wealthy and educated jewish family. She studied psychology, philosphy and aestetics at Budapest University, and made her PhD at laboratory of Geza Revesz. In 1922 she married Imre Hermann, medical doctor originally.

He was a significant and interesting member of Budapest School. Hermann`s most important contribution to psychoanalitical theory was the discovery that palm (grasping) reflex and Moro - reflex of the newborn baby are the heritage of the newborn primata`s instinct to cling to their mother`s hair. /Hermann, I. 1984/ Thus, there are some innate instincts that express the newborn`s claim to "others", that means that the original psychic state of the child is not narcissistic - as it was stated by Freud - but it needs the social environment from the very beginning. In his paper, presented in 1935 Michael Balint concerned to Hermann`s clinging instinct as an evidence against Freud` theory of narcissism. However many of Hermann`s later work and his interpretation of clinging instinct seem to be obsolete now, he was the most significant returned survivors of Budapest school of psychoanalists, and he had a very important rule in keeping psychoanalysis in Hungary alive. /Vajda, 1995/

The Hermanns` marriage was happy. They had three children. Alice was a loyal, self sacrifising wife, so much, that by some gossips her cooperation with the communist regime was the price for her husband`s relative freedom. But I don`t think that to be true. From the thirties she also worked as an analyst, but - as she confesses in her autobiography, 1964 - she could never accept psychoanalytical practice. /Hermann, A, 1979/ Her feeling was that it did not fit to her - however she admired the theory. In the thirties she was involved into child-rearing and education affairs. She was an active contributor of the journal "Gyermeknevelés" (1936-39) where Alice Balint and other Budapest School members published their papers. Another author of Gyermeknevelés was Emmi Pikler, who became the "official" expert of baby care in the fifties. (Her husband was an economic expert of Communist Party). In 1933 Alice Hermann made an experimental research with a colleague called Edith Lenard: they compared a traditional and an "alternative" school. /Hermann A, - Lenárd E. 1933/

From 1945 as a delegate of the Democratic Association of Women, controlled by communists from the beginning. Alice Hermann was one of the foundators and main experts of Hungarian preschool education. In 1949 she was assigned to the Department of Preschool education of the City Council of Budapest. From 1956 - to 1962 she worked at the Preschool Department of Hungarian Ministry of Education. /Hermann, A. 1979/ She was involved in the preschool teacher training, actively took part in elaboration of preschool program that is still considered to be one of the best. In my paper I would like to present her writings on child-rearing and education in 1949 - 1952.

How much education was submitted to political issues? The problem, as Alice Hermann`s case shows us, is rather sophisticated. Her sympathy to leftist values at the beginning was undoubtedly honest. Let us quote her own words:

"When I was a child, my aunt let her shoes laced by the servant, servants used a separate staircase and WC. When I got ill, I was taken to the "professor in Wienna" and the poor children in my village frequantly died since there was no cart that would take them to the doctor, living in the neighbouring village"

One must also take into account that Alice Hermann and her family shared the fate of Hungarian Jews during the war: Soviet army`s coming was a real liberation for them. In her book, published in 1946 she wrote:

"Beyond all of that probably I must thank for the time that finally had arrived, the air in which one can live, the freedom that gives us to think"

In the volume of her collected papers, published in 1979 - 4 years after her death - there is not any of her writings from 1947 - 1956. Surely, that would be her own decision too. Tipically, Hungarian intellectuals don`t list their writings from the fifties in their bibliography.

Alice Hermann`s small articles, published in newly edited Gyermeknevelés - from 1947-1952, then the journal`s title has changed to Preschool education - are also infiltrated with glorification of Sovietunion, socialism, and the like. But seemingly she did not forget what she knew about children. We are witnesses of a supposedly rather difficult effort: to conform her experience and knowledge about children`s nature and development with the ideological pressure - or keep them up against it.

At that point we cannot avoid the problem, what was socialist education like? Does any totalitarian system has a totalitarian educational philosophy and practice?

First of all it is not so easy to decide, what we should call a totalitarian kind of education. E. g. if we mean by that that children compose an unimportant part of the society comparing to adults, we must state, that with communist regime this was not the case. This fact can be proved best on material level. One can maybe say, that nurseries, preschools and kindergartens were built mostly with purpose to make sure that children get enough socialist education, but there was an ambition to make them both materially and professionally good. In every former socialist country all kind of goods for children (from nappies to books, from clothes to furniture) were strongly supported by the state and were extremely cheap until the collapse of the regime. Physical punishment was strictly prohibited from the late forties, since it was allowed in some western countries until sixties.

A second point is that communist regime (and mostly Hungarian, with a lot of Jewish members and sympathizers) had to separate itself from nazi totalitarianism, and one of the important field for that was education. All publications of that time refuse blind discipline, education for obedience. By Alice Hermann` words, from her 1946 book, education for obedience is a danger for the whole population: children treated in this way may become a kneadable mass that follows its leader as a herd of sheeps.

According to her it was not accidental that fashism became a dominating system just in Germany where education was a "building based on drill, planned by scientific methods". (Hermann A. 1982, 49-50). I would like at least mention that Makarenko - whatever we think about him and collective education - was not a drill for obedience. It was something different.

These conditions could make the situation extremely difficult for a professional, like Alice Hermann was. There were important things, that she supposedly could agree with. She was invited for a job, she may had a hope that she can use her knowledge, help children, etc.

In her first paper, that I am going to present, she wrote about changings that had taken place in kindergartens /here children were from 3 to 6 year/. She criticises two systems of kindergarten, existed before the war: one was run by local authorities for poor children of working mothers. Here children were educated for obedience. They had to do everything together, did not get freedom even in the play, they were not allowed to talk while eating, never involved into decisions. These children - states Alice Hermann - never knew, what is happening with them and why.

The other kind of kindergartens were private ones. According to A. Hermann, here education was too much individualistic. Teachers strived to satisfy all needs of children just in the moment of its emergence; their activity was not limited. (Her reasoning reminds contemporary communist party declarations which also railed themselves off both the sectarian leftists and rightwing ideologies.) But here - in 1949 - the silent argumentation between A. Hermann and the "official" line got started, that can be followed until 1952. The question is about the necessity to change the teachers of kindergarten. "Are there real changes without changing people?" - she asks. The answer is yes. However in so difficult times one cannot expect to have funds for buying real child-furniture and other things for kindergartens - she writes - preschool teachers understood that children need an environment where they feel competent. They coped with the problem themselves: shortened the foot of tables, cut off cupboards and made them comfortable for children. On the other hand they took part in trainings and listened communist propaganda thus they can work by the new principles. This was a very important statement in the time when people were dismissed in masses, among others nuns who worked as sisters in hospitals.

She also tried to keep alive refusal of the strict discipline by communist ideology. According to her the most important changing in the kindergartens is that children are emancipated. They are well informed, involved into important events and decisions. They are no more humiliated, when punished.

Hidden arguing we can find in her paper about preschool programs. It was the time of the transfer for planning economy, plans and programs were made everywhere. Preschool teachers had to make plans, which were sent to Alice Hermann, who was in charge in city council by that time. She agreed with using a prepared program in preschools: that may mean that life is planned and children know what they can expect. But she criticises the program, which was elaborated in details for every day. How can the teacher know, what children want to do on a special day? It is better to make a week - program that can

be changed if circumstances make it necessary. When to make a real good program one must never forget about children: maybe they don`t want to walk when the wheather is not so good or prefer to design over listening a story . She emphasises that activity in the preschool must be initiated always by children.

Another point of reference for Alice Hermann was the education for the future. Future as it is wellknown was always very important issue for communist ideology (it is reflected in the words of the song "Internationale": we are nothing now, but we`ll be everything). Education for the future by Alice Hermann meant that the parent or the teacher doesn`t act by his/her momental mood, but tries to think over long-term consequences of a special kind of behaviour. In her book from 1946 she stated that we bring up people, not children. The latter mean that parents take into account the child momentary interests and selfishly only their own vanity or love for comfort - e.g. other children develop quicker thus they try to hurry up their own or they let the child stop moving or speaking when feel disturbed by him. Education for the future is an important argument against strict punishment. In a case, presented on a meeting by a preschool teacher, a child behaved naughty, and the teacher asked other children, how to punish their companion. Six year old children`s group suggested to shut the child to a dark room. Teacher felt that the punisment is too strict, and she withdrawed from it in the last moment, seeing that the "delinquent" is much frightened. This was a punishment for the moment - criticised Alice Hermann. A momental success can never prove the rightness of an educational method.

Collective education was one of the main ideals of communist education. Alice Hermann had a paper under the title "Bringing about community in the kindergarten".

What is a real community? - she asks. When children feel responsibility for their kindergarten, where they feel to be attached. Of course they have to feel too that they are the part of a big community, socialist country. But she warned that small children will not make a real community. It is worthy to mention that there were different views in those times. According to Emma Pikler, mentioned above, director of a children`s home, even 5-6 months old babies can become friends.

On the other hand by Alice Hermann teacher must not instruct children in a direct way: she have just conduct them to the joy of common play. She was also worrying that the system of "brigades" may mean that the small leader of the brigade will act arbitrary with other children.

Children have to learn to share their toys with other children - that very trivial rule was connected in the fifties with the battle against the private-property mentality. Alice Hermann `s recommendation here is also a sophisticated one. Children have to share their toys, even if they were taken by them from home - except the first days, when children need consolation. After that period it is better not to take toys from home - that was official recommendation - but it is even better to take private toys regularly to the kindergarten and share with others - she states.

She also emphasised that children must be attached to preschool teacher. She recommended that teachers guide the same group of children during 3 years - this is a principle that is working until now.

But if a change is necessary, the new teacher need to learn everything about children. She also recommends that school teachers consult with preschool teachers while preparing oldest preschoolers to school. Another recomendation is that preschool teacher eat with the children - each day with another group - and at a separated table.

In another article A. Hermann compared two ways how teachers were eveloping children`s speech. Both are correct, but different. One teacher is speaking kindly, she informs children, but she adresses the whole community, and only when there is some common affair. The other teacher adresses children individually, she is talking a little with each of them, knows about their private problems: the mother`s illness, a new flat, etc. She sits down with a child who never speaks and they watch together a book. In developing children speaking ability it is important to correct grammatical mistakes - she warns - but one must never stop a child when he/she is telling something interesting and important.

By the widespread belief communist regime wanted to monopolize education. That was partly true, but not completely. In all educational institution had to be a parent`s comity - true, it was hoped that these comities will serve also for making propaganda more effective. Alice Hermann`s recommendation is also special here: according to her preschool teachers have to have good personal relationships with parents. She mentioned a case when the preschool teacher learn excellent cake recipes from a mother

In all of her writings there was some kind of praising the new regime or Sovietunion as it was usual. Soviet scientist tought us, that children need play; in developing children`s speach abilities, we can learn from Soviet lingvists, etc. These "laps of honour" were rather ridiculous even by that time, I am afraid. But in a way her proposals and suggestions - never declarations - on the practical problems of education seem to be separate, they may not be mixed with the political thougths as it is in the case of Emma Pikler and others. This did not remain hidden for the authorities as well: in 1952 Alice Hermann was dismissed from her job. Four years she worked in a foster home where "I was helpless with the children, educated by corporal punishment" - she wrote. /1979/

What was her contribution to the education, what was her contribution to the psychology? One have to ask the question: whether her professional proposals even helped to raise children in the spirit of communist ideology. But this was not the case. She was dismissed when official educational ideology and practice returned to drill and discipline in the fifties. A slow changing started after 1956 revolution when Alice Hermann was in appointed for official charge again. She published excellent small articles on education in popular journals. Her impact on preschool education, theory and practice, was really significant. But it was only in 1979, 4 years after her death when her papers on education were published in a book, and her excellent book, first published in 1946 has only in 1982 a second edition.

Notes

Harmat, P. : Freud, Ferenczi és a magyarországi pszichoanalízis, Bern, 1986

Hermann, A: Az új rendszerű tanítás eredményei / with Lénárt Edit/
In: A jövő útjain, 1933, p.106.
Az óvodai közösség kialakítása
In: Gyermeknevelés, 1949/6,
Változások az óvodában
In: Köznevelés, 1949/V/6
Büntessen-e a közösség?
In: Gyermeknevelés, 1950/1
Iskolai előkészítés az óvodában
In: Gyermeknevelés, 1950/10-11
Középső csoportos lettem
In: Gyermeknevelés, 1951/9
A beszédkészség fejlesztése néhány fővárosi óvoda nagycsoportjá- ban In: Gyermeknevelés, 1951/VI
A gyermekben érlelődik a jövő /selected writings/
Budapest, 1979
Emberré nevelés
/second edition/ Budapest, 1982

Hermann, I: Az ember ősi ösztönei
Magvető, 1984

Vajda Zs: A pszichoanalízis budapesti iskolája és a nevelés
Budapest, 1995

RESCUING PSYCHOANALYSIS FROM ITS CRITICS

William R. Woodward, Department of Psychology, University of New Hampshire, Durham, New Hampshire 03824, USA (woodward@hopper.unh.edu)

Psychoanalysis has come under attack on multiple fronts: its therapeutic failures (Sulloway, 1991), its abandonment of the seduction theory (Masson, 1984), the lack of refutability of its theory of repression (Grünbaum, 1984, Woodward, 1992). Now even North American practitioners such as Leo Rangell, Merton Gill, Roy Schafer, and Alan Stone have come to believe that psychoanalysis belongs to the arts and humanities, but not to science (Stone, 1997). I want to place psychoanalysis into the larger arena of psychotherapy practiced in different cultures over the past century. I will do so by highlighting how sexuality, drives, and transference have been socially constructed.

THREE CIRCLES IN THE ENVIRONMENT OF EARLY PSYCHOANALYSIS

Consider as a way of approaching the practical history of psychoanalysis three concentric circles (Danziger, 1990).

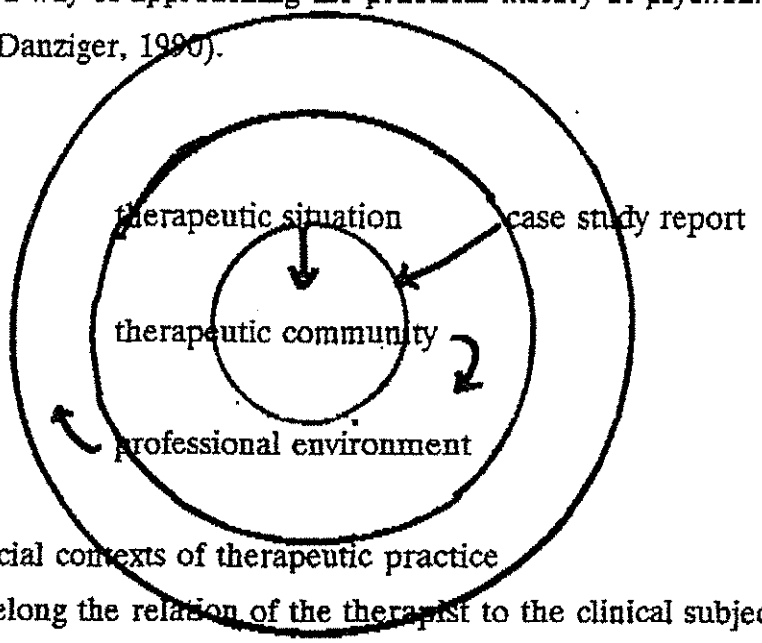

Figure 1. Social contexts of therapeutic practice

In the first circle belong the relation of the therapist to the clinical subject: the individual treated in a private practice setting, a group of subjects in a hospital or group home setting, or various combinations of individual and group in private and hospital practice.

Dyadic and interpersonal techniques belong in this circle, e.g. hypnosis, free association, interview, play, talking cure, strategic family therapy, or some combination of these. My larger point, as the argument unfolds below, will be that psychoanalysis can be "saved" (or its practices better understood) by recognizing its dissemination, dilution, and absorption in a host of modern psychotherapies.

Between the first and second circles Danziger places the research report and I place the case study as presented in articles or books. This case study must conform to acceptable standards of professional interaction in psychotherapy, the inner circle, while also informing the therapeutic community about its knowledge claims in the second circle.

In the second circle, the therapeutic community involved social interactions of practitioner and client in face-to-face encounters as well as diagnostic tests, laboratory materials, and hardware and software. The roles of practitioner and clinical subject extend from indigenous healing relationships to individual psychotherapies, group treatments, and community mental health practices. In the French hypnosis tradition, for example, Jean Charcot exemplified the dangers of the clinical experiment when his paternalistic attitude and arrogance led his hospital staff to show him what he wanted among the hospital patients from the lower classes (Danziger, 1990). In Vienna and Berlin, the first generation practitioners of the psychoanalytic case study treated fee-for-service patients of the upper classes. As in Paris, however, mutual suggestion surely operated in the social psychology of these cases. Moreover, both were working with hysteria, a particular kind of neurosis particularly vulnerable to social influence.

By contrast, what Danziger called the Galtonian model came to prevail in the United States. Mental testing and home visits stretched the definition of the clinical case into a social work "case," where teams of psychiatrist, clinical psychologist, and social worker assessed foster children and their families on a continuum of "normality" -- witness the Chicago's Juvenile Psychopathic Institute and the Boston Psychopathic Institute (Healy, 1919, Bronner et al, 1927). In the second circle, then, we see three three therapeutic communities somewhat parallel to Danziger's three social structures of experiment: Leipzig's generalized mind, Paris's case study, and London's normal curve.

A third circle in the history of psychoanalysis would comprise the professional environment. How was mental healing understood and practiced, not only in Europe but in colonial and postcolonial cultures? What were the attitudes toward the discovery of trauma and sexual etiologies? Here we must draw upon social histories of mental healing in other cultures, including for example urban psychotherapy in Ghana (Mullings, 1984) and urban psychiatry and social work practice in the United States (Lunbeck, 1991). Certainly sensitivity to family violence has increased, but by the same token, a plethora of therapeutic options and theoretical frameworks have swept psychoanalytic theory and led to a more contextual understanding of sexual etiologies, trauma, and child abuse. Family therapies, sex therapies, couples therapy, behavior modification, and especially cognitive therapies have supplanted pure psychoanalytic therapy--to the point where individual psychoanalytic treatments have become a rarer option in mental health.

THE PROFESSIONAL ENVIRONMENT: SEDUCTION DEBATES IN FREUD'S TIME DO NOT UNDERMINE PSYCHOANALYTIC EFFECTIVENESS

The professional environment belongs to the third circle. Freud's acceptance and then rejection of the seduction theory belongs in this circle of the professional environment. Jeffrey Masson has criticized Freud for dropping the seduction theory in 1896. He argues (1985) that several sources of knowledge about sexuality provided early analysts with a sensitivity to the etiology of sex in patients' lives. This debate draws on some of the following features of the professional environment.

Charcot had pointed to the sexual etiology of hysteria in his lectures, which Freud heard in 1885-86 and debate raged on both sides. Some professors of forensic medicine in Paris claimed that childrens' accusations of adult men for child molestation were false (Masson (1992 [1984]). These sources drew connections between hysteria, abuse, and lying. Yet Ambroise Tardieu in his book Les attentats aux moeurs in 1883 reported that over half of his 616 cases were due to rape of girls under age eleven. Tardieu's successor Paul Brouardel was well known to Freud in 1885 (Masson 1985, p. 51). Brouardel often did autopsies on abused children for students (Delcasse, book on cruelty to children, 1885, cited in Masson, p. 51). Articles brought to light by Masson include Etienne

Bourdin's "children who lie," August Motet's "false testimony of children before the courts," and Paul Garnier's "women who accuse." Freud was well exposed to a literature suspicious of seduction reports. On the other hand, Richard Freiherr von Krafft-Ebing (1840-1902) practiced pathology at the Psychiatric Clinic of the Vienna Hospital (Gay, 1988, pp. 136-140) and became a promoter of Freud. The author of Psychopathia sexualis, he reported many sexual pathologies of children who had suffered at the hands of adults.

Thus, therapists increasingly faced and disagreed about the sexual etiology of neurosis. To Josef Breuer's chagrin, Freud stressed the sexual etiology, in particular Anna O's attraction to Breuer as a transference of her love for her father (Breuer & Freud, 1895, in Gay, 1989). Pierre Janet in Paris was also on the trail of sexual etiologies of neurosis (Macmillan, 1990; Ellenburger, 1970). Were they on the trail of a statistically-important phenomenon?

Epidemiological studies of the numbers of reported cases of sexual abuse represent the third circle, the professional environment. Freud actually had some access to statistics on the prevalence of seduction in his day (Macmillan, 1990, p. 568). The numbers came from one hundred case studies by Felix Gattel, a student of Freud who also worked in Krafft-Ebing's Psychiatric Clinic. Freud even wrote in July 7, 1897, that "Gattel is becoming attached to me and my theories." During a shared Italy vacation in September 1897, Gattel had the opportunity to explain to Freud why seduction was unlikely in his patients. The ratio of hysterics to neurasthenics would be 17 to 30, over 50 percent. This large statistic may have disinclined him to push the seduction theory further (Sulloway, 1979, p. 515). In any case, he gave it up (letter to Wilhelm Fliess, September 21, 1897). In our day, Finkelhor et al (1989) reported that sexual abuse, defined as sexual contact with a person at least five years older, yielded a rate of 20-30% in women and 10-15% in men. Methodological criticism might suggest a lower percentage (Okami, 1990). In other words, it was not only fear of disapproval but uncertainty about reports that led Freud to back away from the seduction theory. More broadly, let me quote an excellent rebuttal of Jeffrey Masson by two Hamburg psychologists: "Given that psychoanalysis has had to defend itself for more than a century

without its spiritual founder, it seems a striking simplication to explain the deficits of its practical success by the role of seduction theory alone" (1993, p. 242).

THERAPEUTIC SITUATION AND THERAPEUTIC COMMUNITY: TRANSFERENCE, COUNTERTRANSFERENCE, AND FAMILY RELATIONS

Let us turn from childhood sexuality and child abuse to the nature of the psychotherapeutic relationship. Here we return to the first circle, the relation of subject to therapist. In a review of anti-Freudian books, Reuben Fine calls attention to the fact that "transference is by definition an interpersonal phenomenon" (1985, p. 398). Transference became the core of some approaches to object relations and interpersonal schools of psychotherapy would claim that they emancipated their field from Freud's individual psychotherapy based on "drives."

In the second circle the roles of practitioner and patient depended on assumptions about theoretical identities. Instincts and drives belonged to the arsenal of evolutionary psychologists and formed a building block for association theories of mental activity. Freud's unpublished "Project for a Scientific Psychology" drew upon drive theory, and his subsequent writings contained an underpinning of id and ego "drives," "life and death instincts," and biological impulses of sex and aggression. Frank Sulloway (1979) situated Freud's drive theories in the evolutionary context of Darwinian psychobiology in the nineteenth century, even calling Freud a "crypto-biologist."

In fact, Freud, Janet, and others in the Boston School of Psychotherapy, concerned themselves from the outset with the difficulties of the relationship between therapist and subject. Transference became Freud's term for the projection of feelings by the patient onto the therapist. Countertransference referred to the opposite, the feelings of the therapist toward the client. Controversy continues over just what Freud's patients remembered, and what was constructed from their early past upon the basis of circumstantial evidence (Shimek, 1989). But the deeper significance of the therapeutic relationship as a source of feelings in both directions, transference and counter-transference, remains a contested issue (e.g., Slipp, 1977).

To give an historical example, Henri Ellenburger (1970) found that the underlying

psychotherapists seeking reimbursement follow the DSM-IV in diagnosis and treatment of individuals. This classification system thus imposes standards of normality and abnormality reminiscent of the Galtonian model. Presumably psychotherapeutic treatment has evolved tremendously beyond the Paris, Vienna, and Berlin models. As just one widely-used method in Italy and the United States, strategic family therapy employs a shock to the family system to instigate a realignment of the relational dynamics. Triangular tensions between any three members grow out of one person talking about any other two persons. "Detriangulation" serves to establish direct communication of feelings and realign a family. The unconscious and sexual etiologies become subsumed under this easier-to-use patterning of triangular relationships (Lerner, 1990). Unfortunately, the outcome research using group statistics to justify such models is still wonting.

CONCLUSION

Psychoanalytic therapeutic situations and therapeutic communities have undergone constant cultural change. Have those who would surrender the scientific aspirations of psychoanalysis gone too far? They ask "why has psychoanalysis not become a cumulative discipline?" (Stone, 1990, p. 36). The history presented here reveals psychoanalysis as one element of psychotherapy among many others. Its knowledge claims arose in the context of three levels of practice: the situation, the community, and the profession. Behavioral and cognitive-behavioral techniques have displaced much of psychoanalytic or interpersonal therapy today. Still, the deficiencies of its founders should not yet lead us to discard the paradigm. One hope for the future would be much more outcome research on psychotherapy; one metaanalysis revealed the ineffectiveness of clinical treatments for depression as compared with placebo (Farreras, 1997). Cognitive, behavioral, and interpersonal therapies fared equally poorly in terms of measurable outcomes.

REFERENCES

Bronner, Augusta F.; Healy, William; Lowe, Gladys M.; Shimberg, Myra E. (1927). A manual of individual mental tests and testing. Boston: Little, Brown, and Company.

Danziger, Kurt (1990). Constructing the subject. Historical origins of the psychological experiment. N.Y.: Cambridge.

Ellenberger, Henri (1970). The discovery of the unconscious. N.Y.: Basic Books.

Ellenburger, Henri (1972). The story of "Anna O." A critical review with new data. Journal of the History of the Behavioral Sciences, 8, 267-276.

Farreras, Ingrid G. (1997). Psychotherapy versus placebo for depression: A metaanalysis, 1973-1997. Unpublished master's thesis, University of New Hampshire, Durham.

Fine, Reuben (1985). The anti-Freudian crusade continues. Journal of Psychohistory, 12, 395-400.

Finkelhor, David; Hotaling, Gary; Lewis, I.; Smith, C. (1989). Sexual abuse and its relationship to later sexual satisfaction, marital status, religion, and attitudes. Journal of Interpersonal Violence, 4, 279-399.

Gay, Peter (1988). Freud. A life for our time. N.Y.: Norton.

Gay, Peter (1989). A Freud reader. N.Y.: Norton.

Gerisch, Benigna; Köhler, Thomas (1993). Freuds Aufgabe der "Verführungstheorie": Eine quellenkritische Sichtung zweier Rezeptionsversuche. Psychologie und Geschichte, 4 (1993), 229-246.

Grünbaum, Adolph (1984). The foundations of psychoanalysis. A philosophical critique. Berkeley, CA: University of California Press.

Healy, William (1919). Mental conflicts and misconduct. Boston: Little, Brown, and Company, 1919.

Knorr-Cetina, Karin (1992). The couch, the cathedral, and the laboratory: On the relationship between experiment and laboratory in science. In Andrew Pickering (Ed.), Science, practice, and culture (Chicago: The University of Chicago Press), pp. 113-138.

Lerner, Helen Goldhor (1985). The dance of anger. A woman's guide to

changing the pattern of intimate relationships. N.Y.: Harper & Row.

Lunbeck, Elizabeth (1995). The psychiatric persuasion: Knowlege, gender, and power in modern America. Princeton: Princeton University Press.

Macmillan, Malcolm (1990). New answers to old questions: What the complete Freud-Fliess correspondence tells us. Psychoanalytic Review, 77, 554-571.

Masson, Jeffrey M. (1992 [1984]). The assault on method. N.Y.: Norton.

Masson, Jeffrey M. (1990). Final analysis: The making and unmaking of a psychoanalyst. Reading, MA: Addison-Wesley Publishing Co.

Mullings, Leith (1984). Therapy, ideology, and social change. Mental healing in urban Ghana. Berkeley, CA: University of California Press.

Okami, P (1990). Sociopolitical biases in the contemporary scientific literature on adult human sexual behavior with children and adolescents. In Feierman, J. (ed.), Pedophilia: Biosocial dimensions. N.Y.: Springer.

Scharnberg, Max (1993). The non-authentic nature of Freud's observations. Vol. 1. The seduction theory. Vol. 2. Felix Gattel's early Freudian cases. Uppsala: Acta Universitatis Upsaliensis.

Shimek, Jean G. (1989). Fact and fantasy in the seduction theory: A historical review. Journal of the American Psychoanalytic Association, 35 (1987), 937-965.

Slipp, Samuel (1977). Interpersonal factors in hysteria: Freud's seduction theory and the case of Dora. Journal of the American Academy of Psychoanalysis, 5, 359-376.

Stone, Alan A. (1997). Where will psychoanalysis survive? Harvard Magazine, 1997, 35-39.

Sulloway, Frank (1979). Freud: Biologist of the mind. N. Y.: Basic Books.

Sulloway, Frank (1991). Reassessing Freud's cases: The social construction of psychoanalysis. Isis, 82, 245-275.

Woodward, William (1992). Review of Adolph Grünbaum, Grundlagen der Psychoanalyse. Isis.