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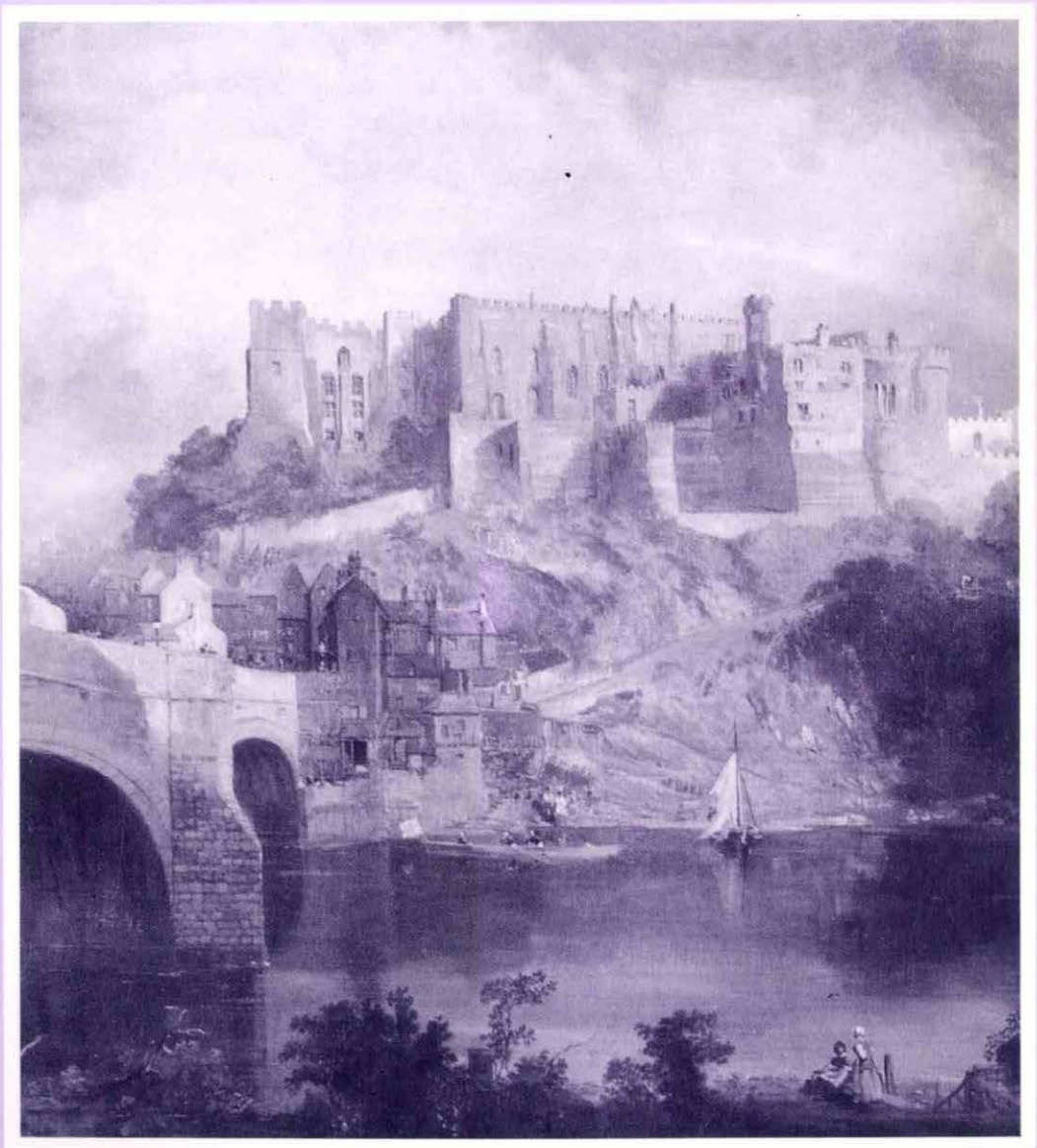
European Society for the History of the Human Sciences

XVIIth Annual Conference



Durham Castle
28th August - 1st September 1998

University of Durham
Department of Psychology

CHHS Centre for the History of the Human Sciences

L. Abma

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European Society for the History of the Human Sciences
(Formerly CHEIRON-EUROPE)

PROCEEDINGS

OF THE

XVIIth ANNUAL CONFERENCE

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Department of Psychology

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Cover Illustration: *Durham Castle, Framwellgate Bridge and the River Wear in about 1815.* From a painting by Edward Hastings and reproduced by kind permission of the Master of University College.

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Ruud Abma

Blaming the System: Family Therapy in The Netherlands

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Summary

From the fifties onwards various new approaches in psychotherapy were created. Among these 'post-psychoanalytic' therapies, 'family therapy' or 'systems therapy' stood apart because of its different practical and theoretical approach. It located the origin of mental illness within distorted communication patterns within the family, and therefore treatment consisted in observation and correction of these patterns as they presented themselves in the therapist's room. This paper adresses the history of family (systems) therapy, especially in The Netherlands, and tries to assess the various cultural and professional backgrounds of its success.

Nowadays, family therapy or 'systems therapy' is an accepted and succesful approach both inside and outside the realm of psychotherapy. Rather than focusing on the intrapsychic problems of one individual, family therapy locates the origin of mental problems in the interpersonal relations and communication patterns of the family as a whole. It thereby replaces the linear causality of mental illness or problems - traumatic events lead to mental problems - by a 'circular' causality: pathology resides within the communication patterns themselves, one family member becoming the 'scapegoat'.

Historically, family therapy is an interesting phenomenon, because it started in various locations at roughly the same time. In the early fifties, the anthropologist Gregory Bateson tried to apply 'systems theory' to schizophrenia (Coulter, 1973). His efforts coincided with the experiments of psychiatrists, like Jackson and Ackerman in the U.S.A. and Laing in the U.K., who were looking for a new way of treating schizophrenic patients (Jackson, 1969; Broderick & Schrader, 1991). Also, social workers in Child Guidance Clinics were seeking to improve on their social case work with 'problem children', by including whole families into their approach (see Hollis, 1972). In the following decade, these scattered attempts at creating a new form of psychotherapy came together, and 'family systems therapy' became recognized as a new brand of therapy.

In this paper, I will focus on the introduction and rapid expansion of family therapy in The Netherlands. First, a brief general assessment of family therapy is given amidst other 'post-psychoanalytic' forms of therapy. Second, I will describe the way the family approach was introduced in The Netherlands. Finally, I will try to explain why family therapy became such a success in The Netherlands.

Family therapy as a 'post-psychoanalytic' psychotherapy

In their historiography of the mental health system in the United States, Castel, Castel and Lovell (1979) coined the term 'post-psychoanalytic therapies' for forms of ambulant treatment that presented themselves as 'modern' alternatives to psychoanalysis, which was regarded as focusing too much on the exploration of the unconscious past of the patient, leading to a time-consuming and élitist approach. Nevertheless, 'post-psychoanalyst' therapists agreed with psychoanalysis that mental problems have psychological causes (not biological ones), that they can be solved within the context of verbal, ambulant treatment, and finally that within psychotherapy no moral standards or rules of conduct should be applied. In the words of Donzelot (1977), both psychoanalysis and post-psychoanalytic therapies are mainly concerned with 'the regulation of images'.

The abandoning of what might be considered the cornerstone of psychoanalysis - the unconscious - in favor of current causes of mental disorder or distress, was in part precipitated by the felt necessity of creating more 'efficient' (i.e. brief) forms of treatment. In this way, client-centered 'counseling' and behavioral therapy (to name a few) used psychoanalysis a stepping stone for their own invasion of the field of psychotherapy (Abma, 1994).

Dissatisfaction with the outcomes of psychoanalytic treatment also triggered the first experiments with the simultaneous treatment of family members in each others' presence. Often it was found that individuals who were considered to be 'cured' developed symptoms of mental illness after

returning to their families, or, alternatively, some other family member showed up for treatment. This 'revolving door' problem inspired some therapists to invite the whole family for therapy sessions, focusing on the distorted communication patterens within the family. Or, as Jackson (1969, 226) put it: `We are much more concerned with influence, interaction, and interrelation between people, immediately observable in the present, than with individual, internal, imaginary, and infantile matters'.

Family therapy in The Netherlands

In The Netherlands, ambulant treatment of mental illness in the fifties was still governed by the psychodynamic approach, although with a heavy phenomenological accent (Abma, 1994).
In the year 1965, family therapy was first introduced to The Netherlands by the American sociologist and family therapist Norman Bell. On leave in Amsterdam to study the Dutch approach to 'multi-problem' families, Bell seized the opportunity to give a small course in family therapy to psychiatric social workers in the protestant child guidance clinic in Amsterdam. As news of the alleged effectiveness of the new method spread, the Ministry of Cultural Affairs, Recreation and Social Welfare, the National Council of Social Work, and the Dutch Journal of Social Work joined forces to introduce the new 'family systems' approach on a larger scale (Bell, 1967). Being eager to expand their territory and to boost their status as professionals, psychiatric social workers visited centres of family therapy in the United States, invited American family therapists (such as Ackerman, Kempler, Satir and Minuchin) to come to Holland, organized introductory lectures and courses on family therapy, and persuaded Dutch publishers to translate American introductions into family therapy, such as Satir 'Conjoint family therapy', Watzlawick et al., 'Pragmatics of human communication', and Minuchin, 'Families and family therapy' (which, incidentally, was published in Dutch one year before the American edition came out). Supported financially by the Dutch government, and intellectually by university-trained psychiatrists and clinical or social psychologists, training courses in family therapy became a regular part of the education of social workers. By the mid-seventies, most mental health institutions in the Netherlands dealing with family problems and problem families could provide their clients with treatment along the lines of family systems therapy. Nowadays, family therapy, although less in fashion, is still regarded in the Netherlands as one of the major strands in psychotherapy.

Blaming the system

The rapid rise of family therapy in the Netherlands calls for an explanation. On the *professional* level, there can be no doubt that *social workers*, especially those working in the mental health field, operated as the primary 'task force' behind the introduction of family therapy in The Netherlands. First of all, they were in need of a more effective form of treatment for troubled families, the psychodynamic approach being too time-consuming and the methods of social casework being too individualistic. Second, not being trained in medicine or psychiatry, they were effectively excluded from existing training courses in psychotherapy and psychoanalysis; the only possibility for social workers to become psychotherapists was by becoming representatives of a new approach, for example the 'salesmen' of family therapy. Young academically-trained professionals, such as clinical psychologists, (social) pedagogues and even sociologists followed suit, expanding the domain of family therapy from lower class multi-problem families to middle and upper class couples with 'relational difficulties' and parents having trouble in controlling their children.

The 'aggiornamento' of social workers and social scientists towards psychotherapy was also greatly helped by the spirit of the counter-culture of the sixties, the main representative within the therapeutical field being 'anti-psychiatry', which criticised the medical establishment for being both elitist, repressive and anti-social. (The career of Ronald Laing, starting as a psychoanalyst, becoming one of the founding fathers of family therapy and very soon afterwards one of the main representatives of anti-psychiatry, is a case in point.) Although the 'social' in the 'social model' of family therapy for the most part was restricted to interpersonal relations or 'communication' within the family (see Jacoby, 1975; Kovel, 1976), it still could function as a moderately 'progressive' role model for professionals who considered antipsychiatry 'a bridge too far' and moreover not very useful within the practice of ambulant treatment.

Apart from developments on the professional and ideological level, family therapy greatly benefited from *cultural changes* since the fifties. Secularization and democratization in Dutch society at large (and Western societies in general), also had profound effects on family relations, both between parents and children and between husbands and wives. The traditional structure of command, with the husband functioning as the officially recognized head of the family had given way within two decades to a more 'democratic' structure, involving explicit motivations of desired actions between parties as well as negotiations concerning family decisions (see Lasch, 1977). Needless to say, that many families required professional help in adapting their course of action to the new cultural norms, that in turn were influenced by the 'ideology of communication' so typical of family therapy.

Finally, family therapy itself contributed to its own success by its theoretical and practical versatility. Drawing on 'systems theory', it was able to reconstrue almost any social or mental problem in terms of `faulty communication', thereby displacing attention from social cause-effect relationships towards techniques for co-operative problem solving and conflict resolution. (Cheal, 1991). (Incidentally, this avoiding of social causes provided feminists with an important argument in their critique of family therapy, see Hare-Mustin, 1978.) Like psychoanalysis in the earlier days (Donzelot, 1977), systems theory enables a wide range of specialists to share a common language, and it therefore facilitates communication among them (Cheal, 1991). To this characteristic can be added its very 'open' attitude towards various theoretical and ideological influences. Textbooks and readers of family therapy show a variety of theoretical approaches, ranging from psychoanalytically inspired traditions to 'humanistic' or even 'behavioristic' ones. More recently 'postmodern' conceptions have entered family therapy, so it is not uncommon to find family therapists announcing themselves as `constructionists' (McNamee & Gergen, 1992). Ideologically, family therapy also manifested a considerable flexibility, sympathising with humanistic or even marxist approaches in the early seventies as easily as adapting itself to the no-nonsense, more directive approaches of the eighties.

Summarizing: family therapy succeeded in entering various domains of psychosocial treatment, thereby serving the ambition of social workers and various non-psychiatrist academics to gain professional status as therapists. Compared with both psychoanalysis and 'post-psychoanalytic' approaches, family therapy had two major advantages, which helped its rapid rise in the modernizing culture of the sixties. First, it included explicitly - both in theory and practice -important aspects of the social context in the treatment itself, not just the 'family images and phantasies' of the individual patients, but the actual relations as demonstrated in the therapy room. Secondly, family therapy proved itself capable of absorbing most all varieties of theoretic or ideological preference, so that it - more than other forms of therapy - was able to help both the 'Zeitgeist' and its contemporaries along.

References

(References to Dutch sources are omitted here for reasons of space)

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

Bell, N.W. et al. (1967) *New ways in family treatment. Study-conference, June 1965.* Den Haag: Staatsuit-geverij.

Broderick C.B. & S.S. Schrader (1991) The history of professional marriage and family therapy. In: A.S. Gurman & D.P. Knispern (Eds.) *Handbook of family therapy, vol II,* New York: Brunner/Mazel, pp. 3-40.

Castel, F., R. Castel, & A. Lovell (1979) *La société psychiatrique avancée. Le modèle américain.* Paris: Grasset.

Cheal, D. (1991) *Family and the state of theory.* New York etc.: Harvester Wheatsheaf.

Coulter, J. (1973) *Approaches to insanity. A philosophical and sociological study.* London: Martin Robertson.

Donzelot, J. (1977) *La police des familles.* Paris: Minuit.

Gergen, K.J. & S. McNamee (Eds.) (1992) *Therapy as social construction.* London: Sage.

Hare-Mustin, R.T. (1987) The problem of gender in family therapy theory, *Family Process, 26,* 15-27.
Hollis, F. (1972) *Casework, a psychosocial therapy (Second Ed.).* New York: Random House.
Jacoby, R. (1975) *Social amnesia. A critique of conformist psychology from Adler to Laing.* Boston: Beacon Press.
Jackson, D.D. (Ed.) (1969) *Therapy, communication and change (Human communication, Vol. 2).* Palo Alto: Science and Behavior Books.
Kovel, J. (1976) *A complete guide to therapy. From psychoanalysis to behaviour modification.* New York: Random House.
Lasch, Chr. (1977) *Haven in a heartless world. The family besieged.* New York: Basic Books.

John Bintliff

A Discipline of Fragments - in Fragments? Archaeology within a Contemporary Interdisciplinary Context

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Summary

This paper will focus on the breakup of the discipline of Archaeology as a coherent body of theory and method, a process which began with the scientisation and statist interventionism of the 1960s, and was visibly enhanced by the impact of Postmodern approaches within the academic community of archaeologists. The present absence of a common intellectual position for the diverse communities of heritage managers, rescue 'diggers', archaeological scientists and university social theorists is a phenomenon that can be explained historically, and in ways that reflect on wider trends in 20th century Western society. But it is far from obvious that an integrative solution can be found to reunite the community intellectually. Nonetheless this paper will strive to show that, by broadening Archaeology's reading in contemporary interdisciplinary theory beyond the classics of Continental Postmodernism, to include the French Annaliste approaches within History, Chaos-Complexity theory within the General Sciences, Wittgenstein's theory of discourses from Philosophy, and the concepts of Stephen Jay Gould and Richard Dawkins from Evolutionary Science, the non-communicating sub-communities within Archaeology can be brought under a single elaborate but logically coherent intellectual umbrella - very much to their, and possibly your, surprise.

Horst-Peter Brauns, Gesine Grossmann and David Miller

On Conceptualizations of Post World War II Developments in Psychology: Colonization, Decolonization, Reception or Productive Exchange?

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Some introducing remarks about "metaphor"

The use of metaphors in scientific texts yields various functions. Besides a vivid diversity of formulation a metaphor for instance may produce something that can't be expressed in discursive language, demonstrate something figuratively or may accentuate some aspects of a thing like an optical instrument (cf. Bulhof, 1987). Although there certainly are much more metaphorical potentialities it already became obvious that whatever a metaphor may achieve, as a secondary object (SO) it bears some relation to a primary (PO) one in the general sense that it serves as a model for it. Stressing the context of introducing a metaphor we suggest that a PO instigates or calls forth some features of SO. These features give rise to a set of implications which fit to PO. Finally, SO is parallelized to the fitted PO. A mutual reference fixing has taken place (Kuhn, 1993).

One of the most intriguing features of this process is its openness. That means, the fixing of mutual reference bears some degrees of selective freedom and once carried out, the introduction of a metaphor leaves open to its recipient to a large extent what the mutual implications are. The selections of the author can be congruent with the ones of the reader or not. Metaphor plays indeed an important role in science. Its function does not have to be a transient one, as the positivistic idea of a replacement of figurative speech through "proper scientific" discursive language might imply. Abstract terms as well as metaphorical expressions can be useful instruments for what Kuhn (1993, p. 539) calls determining "the way in which language attaches to the world". The realm of science they both serve as representational instruments and their use is less a matter of ontology, but of proper fitting and complexity. Seen as a representational instrument "metaphor reminds us that another language might have located different joints, cut up the world in another way" (Kuhn, 1993, p. 537).

The use of metaphors in historiography of psychology is quite common since its newer beginnings around the turn of our century (Ebbinghaus, 1900; Stern, 1900). So, in principle there is nothing new when more recent developments of our science are metaphorically illustrated too. Moreover, it may be doubtful whether any scientific language can dispense the use of metaphors. Insofar however as theory guided research is concerned metaphor's availability for use seems to be regulated in a certain degree, their mission looks to be restricted or predetermined by the theoretical conceptual network. Introducing metaphors on their own without further theoretical semantical backing will be therefore much more at risk of being interpreted more freely even in a sense the author did not have in his mind or intend to produce.

The "colonization" metaphor in psychohistorical context

An interesting use of an atheoretical metaphor obviously happens when a metaphor is "(used) deliberately, and somewhat provocatively" (v. Strien, 1997, p. 359). It is a case of "colonization" which is introduced in order to subordinate, perhaps better to classify "the Americanization of European Psychology" after World War II (v. Strien, 1997, p. 349). Without any doubt, a political concept is transferred to a segment of the recent history of psychology. In our terminology it is used as a SO for a psychohistorical PO. We are asking therefore, what does it offer us as historians of psychology?

Preparing an answer to that question at first we will comment the author's semantical reflections added. Afterwards our focus centers on the historical evidence given, followed by a glance at the secondary object chosen and its coordination with historical facts, so that a mutual reference fixing between PO and SO can be approached in the light of the pertinent sources. Finally we will consult some new primary sources.

As already mentioned above, "colonization" is used as a more general term than "Americanization", so that the latter is conceived "as a form" of the former (v Strien, 1997, p. 349).

But that is no operation only on the conceptual level. Reference is taken to "a number of publications" (loc. cit.). That again make sense only when the studies referred to support positive evidence for the "Americanization" of "European Psychology" (loc. cit.). In the eyes of the author "colonization" indirectly gains psychohistorical reference by its connection with studies showing that "Americanization" took place.

"Colonization" itself is elucidated in a variety of ways. For instance, we are informed about "scientific colonization", namely "intellectual domination of an existing culture by a foreign, more powerful" one (loc. cit.). However, "his domination... is meant... in the more subtle sense of a voluntary submission... a relationship designated... as a <colonial pact> (loc. cit.)". As a supplement "cultural submission" is inserted, consisting of reference to the culture of the colonists, self-criticism of the colonials, their learning of the language of the colonists, and traveling to the colonist culture. Finally, "after some time... a de-colonization process sets in..." (loc. cit.). As a conceptual alternative for this characterization of European Psychology "neo-colonialism" is suggested. Additionally, in regard to European social-psychology a "reception of American ideas" must be noted (loc. cit.).

Obviously, there is at least a twofold specification of "colonization", namely by "do-mination" and "submission", whereby the latter is dubbed with the wider term as "cultural". We feel some trouble with noting the tacit extension of "colonization" to culture in the scientific context given as well as the incompatibility between "reception" on the part of social-psychology and "domination" of the whole realm of psychology but we will not turn to these problems in further detail. As already indicated above, we will rather concentrate ourselves on the evidence presented by v. Strien for his more or less implicit thesis of "Americanization" proven already by "a number of publications" (loc. cit.). We suggest that the conceptual-modeling claim of "colonization" - as a SO - mainly rests upon positive evidence for "Americanization". If "Americanization" can not be supported by historiographical evidence, it does not make much sense further to qualify it as "colonization". In so far as the latter term does not gain correspondence with historical facts it hangs in the air, lacking real-historical reference.

On historical evidence for "Americanization" of German Psychology

Restricting ourselves for the present to the two works on the development of post World War II German psychology by Métraux (1985) and Graumann (1994) which are mentioned in the appendix (v. Strien, 1997, p. 360), the support for "Americanization" is rather weak. From Métraux (1985, p. 226) we hear that "Americanization of German Psychology is only a short form for a rather complex process". Its "polemical connotations" should be dismissed (loc. cit.). In order to avoid "misunderstandings" at least exact defining features should be offered as long as an acceptable definition is not available. The author concludes after the inspection of sources which comprise such relative strong items as the use of American textbooks, lectures given by American scientists and last not least the distribution of Education manuals that German psychology as a whole "underwent a quantitative and qualitative change in the sense of reception and assimilation of the American ideal" (Métraux, 1985, p. 235). Obviously, "Americanization" is specified by "reception and assimilation" which are non-political but science-descriptive terms. Precisely these accentuations are lost in v. Striens reference to Métraux's results.

In regard to social-psychology alone in German as well as in European perspective, Graumann and Eckhardt (1994, p. 795) do not claim either evidence for an "Americanization". Understanding "Americanization" as "acculturation", namely "adaptation of an existing social-psychological scientific discipline to the American way to pursue it", it can not be substantiated (loc. cit.). The main reason is the non-existence of a monodisciplinary European social psychological research program which could have been suppressed. "Insofar...the hypothesis of Americanization can not be confirmed" (loc. cit.).

Tracing the two sources adduced by v. Strien (1997) in order to back "American-ization" of German psychology after World War II we may after all suggest that one can hardly gain sufficient support for it. This means again, that in so far as the two sources referred to are concerned "Americanization" is lacking a real-historical basis. From here follows furthermore that so long as this specific evidence is missing the more general term "colonization" looses its extensional

reference to the segment of modern German history of psychology in question - and the derivative of de-colonization too.

It must be conceded however, that things may look quite different in regard to Dutch developments in the same time. Because there are no additional sources at our disposal in this respect we must leave this open and will draw upon new sources of German psychology after World War II. Before that we will shortly deal with the colonization metaphor more directly.

Colonization as metaphorical secondary object

Although colonization seemingly lost its extensional reference to "Americanization" of post World War II German psychology it still could be valid for Dutch circumstances. As indicated above, we can, however, not touch this topic here any closer. For the time being it will be supposed that the inferences drawn by v. Strien are sufficiently covered by their Dutch sources. That gives another reason to try to reconstruct the conceptualizations of colonization as given and focus them solely as parts of a metaphor for Dutch developments.

As indicated before, a metaphor as a SO serves as a model, whose features bring about a set of implications on the PO's side so that a mutual reference fixing takes place. There hardly can be any doubt about the features which are connected with "colonization" as the metaphorical SO and the set of implications which fit to post World War II psychology of the Netherlands as PO: From above it is clear that - neglecting de-colonization - domination and submission with the components *reference to the culture of the colonists, self-criticism by the colonials, learning language and travelling* belong to the main explications given in general. To them certain items of Dutch post World War II history of psychology are coordinated: To *Domination* Fulbright scholarships, Marshall Aid Plan for the Netherlands and "disseminating American ideas and values"; as well as spending a sabbatical year at Dutch universities and feeling of American superiority come through.

The colonials exhibit *submission* in form of adoration and traits of a cargo cult; *reference to the culture of the colonists* includes teaching main-stream American social-psychology; *self-criticism by the colonials* is prominent in downplaying of European pre-achievements; *learning language and travelling* is realized by younger Dutch psychologists going to the USA.

What strikes us most in this reference fixing is the lack of referring to concrete sources and the distance to psychology. The mere existence and offer of financial and other means and the mere presence of persons do not prove or even guarantee their use in the sense of colonization.

The criterion of learning language and traveling looks a little bit weak as it was certainly met without World War II and the specific historical conditions afterwards. English has had replaced German already as the dominant international scientific language and traveling to the US could serve the aim of learning it to be fit for international scientific communication.

After reconstructing in the light of the colonization metaphor a set of implications on the side of post World War II Dutch psychology some self-produced discongruence and lack of fit must be stated. As announced before, we now come to sources about German post World War II psychological developments.

Some new sources about "Americanization" of post World War II German psychology

In order to be in the position to trace possible scientific developmental processes of shorter durance it seems adequate to differentiate between sources of the first years after the end of World War II and those after 1950. A first three item sample of the first interval we could draw up to now shows titles as "New orientation of the science of the soul" by Lersch (1949), "Presence and future of psychology" by Rohracher (1948) and - "Psychology today" by Undeutsch (1948). Summarizing these articles by German authors on the actual status of German psychology between 1945 and 1950 there hardly can be registered any indication for "America-nization". This does not mean an international close-down in favour of a constricted national outlook: While Undeutsch is concerned with characteristics of the developmental phases of psychology after both World Wars, Lersch refers to factor-analysis "known from American Psychology" (loc. cit., p. 113) and Rohracher (loc. cit., p. 525) points to America and Russia who had valued but not profited from experimental cooperation between psychology on one side and zoology and biology on the other. A poor representation of social-psychology is complained by Lersch (loc. cit., p. 113). The three sources

have in common to direct their respective discourse to at least two levels of analysis: psychology as the whole of one subject and the psychological subdisciplines. As a first German source representing German psychology after 1950 we use here only v. Bracken's 1952 paper on "Recent trends in German psychology" which appears in the "Journal of general psychology" (Vol. 47, pp. 165-179). In our context we must stress the statements that

- "historically the ties between American and German psychology have been very close"
- "closer international collaboration of psychologists is greatly desired" and that
- "social psychology is a field in which Germany has very much to learn from achievements in America."

Confronting these primary sources with v. Strien's secondary ones the historiographical hypothesis of Americanization of German psychology during the time considered does not gain more positive evidence. It should be noted in addition, that the state of social psychology attracts some interest already so early after the war. We further learn the veridical differentiation between the disciplinary and subdisciplinary level and to take into consideration the status quo ante the war as an initial state of various circumstances to which the respective after war changes must be compared before new or stronger developments are marked.

Evidently, in our context American sources are of immediate relevance, too. There is, for instance, Adams' report in the year 1957 on "the status of psychology in the universities of Austria and Germany" after having visited 16 psychological institutes of these countries during the summer of 1955. Although "it was learned that American authors are utilized most in the areas of experimental, psychological testing, industrial, statistics and learning" and "very rarely or not at all in the areas of personality, characterology, graphology, expression, developmental, applied, clinical, general, depth psychology and Psychagogik" (loc. cit., p. 154) Adams is not simply willing to state Americanization. In a contrasting reponse to Welleks judgement "that through the regime of occupation, the influence of American psychology in Germany has notably grown" (loc. cit. p. 155) he admits "that research is turning increasingly in the direction taken by American psychology" (loc. cit.). But it is explicitly left open, "whether this is due to the influence of American psychology or to the natural growth of the science" (loc. cit.). In accordance with this cautious statement the inspection of the actual disciplinary interests of 21 directors of German-speaking psychological institutes reveals "only one who in part credits his present orientation to American psychology" (loc. cit., p. 150), namely H. Thomae.

After all we see in the contact between the US and German psychological communities the revival of paths of mutual reception which were interrupted by the war. Strictly speaking the sources introduced here so far support a continuation of exchange relations which will have been seen as productive from both sides. Whether and how a way for a change to a preponderance of US psychology was paved which may request other concepts as science decriptive ones may be more open than ever according to the documents included here. That even holds if more sources should indicate a set of processes mainly due to differences between scientific generations. According to that view it could be hypothesized that the generation of psychologists educated and appointed before the war would keep after the war in line with the tradition and its international exchange relations which brought them up. Simultaneously, the cohort of post-war students could be more sensitive for more recent developments, in particular in the United States. While the old stick to the old, the differential scientific reception of the younger does by no means necessarily produce a qualitative change, requiring the introduction of political concepts, neither in discursive language nor by means of metaphor.

This multi-process-view should find a complement in a multi-subject-view. The request contrasts with v. Strien's approach which was implemented here to a certain extent too. It should be criticized by not differentiating enough between psychology as a discipline and its subdiciplines. Generally speaking, besides programmatic declarations concerning the state of psychology as a science a predicate may be attributed to the discipline as a whole only to the degree of being attributeable to its parts. Therefore it should be asked whether "Americanization" not only of social psychology but also of the developmental, general, differential, clinical varieties and Ausdruckspsychologie could be demonstrated. In the case of differential psychology e.g., it must be noted that factor analysis was introduced already during World War II (Hofstätter, 1953, p. 158).

Returning to the role of metaphors in science, the statements from the beginning could be confirmed. Using metaphors does not necessarily mean to avoid a proper scientific approach. However, it depends on the choice of metaphor and its context to generate a respectable and helpful instrument that leads us to "more effective ways of dealing with some aspects of some natural phenomena" (Kuhn, 1993, p. 539).

References

Adams, J. (1957). The status of psychology in the universities of Austria and Germany: 1955-1956. The Journal of General Psychology, 56, 147-157.

Bracken, H. v. (1952). Recent Trends in German Psychology. The Journal of General Psychology, 47, 165-179.

Bulhof, I. (1987). Literarische Elemente in wissenschaftlichen Texten: Grundlegung einer hermeneutischen Ontologie. Allgemeine Zeitschrift für Philosophie, 12, 35-54.

Ebbinghaus, H. (1901). Die Psychologie jetzt und vor 100 Jahren. In, P. Janet, (Hrsg.), 4. Internationaler Kongreß für Psychologie, Paris, 1900. (S. 49-60).

Graumann, C.F. & Eckhardt, G. (1994). Die Entwicklung der europäischen Sozialpsychologie nach dem 2. Weltkrieg - ein Ost-West-Vergleich. In, K. Pawlik, Hrsg., Bericht über den 39. Kongreß der DGfP (S. 793-795). Göttingen: Hogrefe.

Hofstätter, P.R. (1953). Einführung in die quantitativen Methoden der Psychologie. München: Barth.

Kuhn, T. (1993). Metaphor in science. In A. Ortony, Ed., Metaphor and thought, (pp. 533-542). Cambridge: Cambridge University Press.

Lersch, P. (1949). Neuorientierung der Seelenkunde. Philosophisches Jahrbuch, 49, 112-114.

Métraux, A. (1985). Der Methodenstreit und die Amerikanisierung der Psychologie in der Bundesrepublik 1950-1970.

Rohracher, H. (1948). Gegenwart und Zukunft der Psychologie. Universitas, 3, 521-528.

Stern, W. (1900). Die psychologische Arbeit des neunzehnten Jahrhunderts. Zeitschrift für Pädagogische Psychologie, 2, 329-352, 413-436.

Strien, P. v. (1997). The American „colonization" of northwest European social psychology. Journal of the history of the behavioral sciences, 33, 349-363.

Undeutsch, U. (1948). Psychologie heute. Götttinger Universitätszeitung, 3, 20, 1-3.

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Philosophical Inheritance in Pierre Janet's Pathological Psychology[1]

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Summary

According to traditional French historiography, French scientific psychology was born when it differentiated itself from philosophy. This split between the two disciplines is attributed to Taine and to Ribot, who, consequently, are considered to be the "founding fathers" of French psychology.

In this paper we shall examine the case of Pierre Janet, who, at the turning point of the century was recognised world-wide as the most important French psychologist.

It is generally said that he was the follower of Ribot and of Charcot. However, he was also Paul Janet's nephew. Paul Janet was a very well-known and influential philosopher of the so-called French "spiritualistic" school, for which psychology was central to philosophy.

Following his uncle's footsteps, Pierre Janet taught philosophy for 16 years and wrote in 1896, a philosophy text-book for pupils of secondary schools.

At the same time, since 1885, he became a specialist in pathological psychology and published in 1889 his doctoral dissertation, *L'Automatisme psychologique*, which was immediately considered to be a classical book in psychology.

We will argue that this book is as much indebted to the old spiritualistic psychology, which claimed the substantial unity of the self, as to the new psychology at the time, which questioned it.

With Pierre Janet, the idea according to which French psychology split off from philosophy shall be reconsidered. It would be more accurate to speak in terms of a compromise between philosophy and the "new" physiological and pathological psychology.

It is widely accepted among French psychologists (Reuchlin, 1957 : 6-7) that the break with philosophy was a turning point in the history of French psychology. Only after the break could psychology aspire to scientific status. Taine and Ribot's manifestos of 1870 are often cited in favour of this view. Contained in the introductions to Taine's *De l'intelligence* and Ribot's *La psychologie anglaise contemporaine*, they proclaim the need for psychology to cut itself off from its roots in philosophy and rely instead on physiology and pathology.

The principal object of Ribot and Taine's attack was, in fact, French spiritualist philosophy to which we shall return. Ribot's 1873 thesis, *L'Hérédité. Étude psychologique*, was clearly intended as a broadside against the spiritualist school, and was hotly debated in the Académie des Sciences morales et politiques[2]. But Taine and Ribot had not suddenly seen the light. From their perspective, it was probably more a matter of effecting a compromise between science and philosophy. The combined medical and philosophical training favoured by French psychologists over the next 70 years testifies to the efficacy of this compromise.

Pierre Janet is generally presented as the founder of this tradition. For several decades, he enjoyed international renown as a great psychologist. *Agrégé* in philosophy, in 1889 he wrote a philosophy thesis entitled *L'Automatisme psychologique*. After qualifying as a doctor of medicine, in 1893 he presented a medical thesis entitled *Contribution à l'étude des accidents mentaux chez les hystériques*. A « student » of Ribot and Charcot, in his training he embodied an alliance sought by both Ribot, who was not a doctor of medicine, and Charcot, who was not a philosopher.

Attention has been drawn to the fact that Janet taught philosophy for 16 years (1882-1898) and published a course book of philosophy (Ellenberger, 1970 ; Prévost, 1973). But until now no-one has explored the link between his twin identities, as philosopher and teacher on the one hand, and famous psychologist on the other. This link needs to be viewed in the context of cultural history in general and Janet's intellectual biography in particular. One cannot, we feel, understand Janet's intentions in *L'Automatisme psychologique* without bearing in mind the fact that it was originally

written as a philosophy thesis. In *L'Automatisme psychologique*, Janet writes as a philosopher, and, as Ellenberger notes, his examiners congratulated him on the fact. Though later affiliated with Ribot and Charcot, Janet was, first and foremost, the nephew of Paul Janet, who was a Professor at the Sorbonne and a leading spiritualist philosopher. Paul Janet was a highly influential figure; he drew up the syllabus for the sixth-form philosophy option early in the Third Republic and was President of the Board of Examiners of the Agrégation in Philosophy (Fabiani, 1988). In 1885, he presented his nephew's first communication to the Société de Psychologie physiologique, and he was one of the examiners for Pierre Janet's thesis in 1899.

Paul Janet and French spiritualism

Before analysing Pierre Janet's relation to philosophy, we need to sketch out a context specific to France. There, under the aegis of spiritualism, psychology was considered a constitutive part of philosophy, and effectively synonymous with it. Moreover, it is important to remember that most of the protagonists of this story shared a professional identity of almost initiatory character; they were all agrégés in philosophy and former pupils of the École Normale Supérieure. Here we touch on a system of elite education specific to France. The French Revolution found the universities excessively clerical in tendency, and replaced them with the grandes écoles. When Napoléon reinstated the universities, the grandes écoles survived. Among them was the École Normale Supérieure, which trained secondary school teachers in Arts and Sciences; many of these teachers later went on to become academics. Not only Paul Janet, Hippolyte Taine, and Théodule Ribot, but Henri Bergson, Emile Durkheim and, of course, Pierre Janet were products of the École Normale Supérieure. And all but Taine were also agrégés in philosophy. The agrégation is not a university diploma, but a nation-wide competitive exam of extreme difficulty. Agrégés automatically acquire the status of secondary teacher, and with it a much-coveted title that, even today, commands great prestige.

Beginning in the early 19th century, an official school of French spiritualist philosophy had grown up. The French term is not synonymous with its English form. It generally refers to the school of psycho-philosophy founded by Victor Cousin. Cousin had named it *eclecticism*, claiming to preserve only what was best in any given philosophical doctrine. His followers, Théodore Jouffroy and Adolphe Garnier, oriented Cousin's system toward psychology. Their principal influences in this were Maine de Biran and the Scottish school with its « faculties of the soul ». For these « psychologists », psychology was a specific area of knowledge, attained by introspective observation, and distinct from both physiology and ontology. Thanks to the mind's immediate intuition of itself, it was possible to experience the unity of self under different states of consciousness. A psychology of faculties was developed using this method, and scientific claims made for it. Finally, it allowed an ontology to be devised on a model attributed to Descartes. This ontology was based on the distinction between the spiritual and the material, or, in the 19th century terms, between the moral and the physical. Psychology thus became the central component of spiritualist philosophy. The fact is attested by a school manual published in 1890, which warns philosophy students against

> a confusion of which they are often guilty: psychology is not philosophy, and it is a grave error to use either word indiscriminately in place of the other. (Hannequin, 1870 : 3)

Taine and Ribot, in their polemic, asserted that there was no contact between spiritualist psychology and mental medicine. They were wrong; Garnier was an active member of the Société Médico-Psychologique, whose membership included a certain number of philosophers and a majority of alienists. Behind the theory of automatism borrowed by the alienist Jules Baillarger from Jouffroy lies spiritualist psychology, and behind Jouffroy lies Maine de Biran. Baillarger distinguished two states of mental activity. The first of these allows us freely to direct our ideas, and is called « voluntary mental exercise ». The second is « automatism of the intelligence » and is characterised by « involuntary exercise of the memory and imagination » (Baillarger, 1890 : 496 passim). In this state, the faculties operate spontaneously, and present a succession of ideas and images that we cannot control; this explains both normal phenomena such as reverie and dream, and pathological phenomena such as somnambulism, hallucination and delirium.

In England, at around the same time, Laycock and Carpenter were working in the field of cerebral physiology, and their notion of unconscious cerebration was modeled on the functioning of the

spinal cord (Gauchet, 1992). By contrast, French spiritualist psychology concentrated on modes of obscure perception in which the sense of self is lost, and contrasted them with modes of clear consciousness. Consequently, most French « psychologists », from Maine de Biran on, were interested in sleep, dreams and somnambulism.

Paradoxical as it may seem, the psychology that Taine and Ribot attacked had prepared the ground for them. For the spiritualist school, introspection was the distinctive methodology of psychology. Auguste Comte denied introspection any claim to scientific status, and by so doing disqualified psychology itself. Faced with such radical claims, Taine and Ribot were forced to defend the notion of psychology's autonomy as a science. They did so by appealing against the severity of Comte's sentence. Thus Taine, though critical of Jouffroy, praised him for having presented psychology as a « particular science » (Taine, 1857: 231). Like the spiritualists, Taine and Ribot were interested in the various states in which direct intuition of self is lost. But they turned these states against spiritualism, making them the spearhead of their attack on the central spiritualist doctrine, that of the unity of self. In 1876, the celebrated case of Félida was written up in the *Comptes rendus* (Acts) of the very spiritualist Académie des sciences morales et politiques, and in an article published by Azam in the Revue Scientifique under the title « Periodic Amnesia or Duplication of Life ». Taine and Ribot perceived in this case a refutation of the unity of self. They felt that it offered experimental proof of the empirical critique of personal identity advanced by Hume.

Certain spiritualist thinkers sought to take up the challenge constituted by the rise of positivism in philosophical circles. Paul Janet was one such; he attempted to integrate the discoveries of the new science into psychology, and thus into philosophy. Spiritualist psychology could not, he felt, ignore the advances of mental and neurological medicine. He accepted them wholeheartedly; just as Adolphe Garnier had been a member of the Société Médico-psychologique, Paul Janet was Vice-President of the Société de psychologie physiologique, which had been founded in 1885 ; its President was Charcot. But his insistence on an explanation compatible with spiritualism was an effort to counter those who sought to found a new psychology. He instantly reacted to Azam's account of Félida by interpreting it as « an extension of dream and somnambulism » affecting only the « external self », not the internal and « fundamental » self. Janet considered the « fundamental self » as a kind of transcendental self, self-identical despite the variations of the outer, empirical self. This form of argument is repeated in very similar terms in 1880 in his classic *Traité élementaire de philosophie* (Paul Janet, 1880 : 111-112), and in an analysis of his nephew Pierre Janet's thesis (*L'Automatisme psychologique*), in his *Principes de métaphysique et de psychologie* (Paul Janet, 1897). In the *Traité élementaire de philosophie*, double consciousness and double personality are presented as a problem; the solution he proposes was adopted as the « official » spiritualist account, and consisted in making these « facts » compatible with spiritualism and thinkable for teachers of philosophy.

Paul Janet's enlightened spiritualism cleared the way for Bergson, who belongs in the same tradition. When, in 1889, Pierre Janet proposed a pathological psychology free of philosophical polemics, he was, arguably, responding to the wishes of his uncle Paul. In Paul Janet's view, medicine had monopolised the subject of the mental faculties of the mad for far too long. Even in their morbid state, these faculties were facts of consciousness. He praised Pierre Janet's thesis in the following terms:

> It was therefore justifiable to reclaim and reconquer this area for psychology, and thus for philosophy. This is what Pierre Janet has done. (Paul Janet, 1897 : 556)

Psychopathology and philosophy in the work of Pierre Janet

There was some truth in Paul Janet's praise, as we shall see. By making the new pathological psychology compatible with the earlier philosophy, Pierre Janet had indeed recovered territory seemingly lost to spiritualism.

Janet's thesis was well received not only by doctors of medicine and 'positivist' psychologists, but by philosophers too. (The major exception was Alfred Binet, who, in 1890, published a fairly severe critique in the Revue philosophique.) Why was the reception so favourable? Ribot's thesis had not received the same plaudits. For the medics, Janet's thesis wore the aspect of experimental research on double and multiple personalities; it could be read as belonging to the evolutionist

approach of Spencer and Jackson, and thus as following in the footsteps of Ribot. But the remarkable fact is that Janet does not refer to Spencer and Jackson in his thesis. For the philosophers, Janet's presentation of the celebrated notion of subconscious psychological activity kept alive the possibility of a synthesis between the old and the new psychology. Janet's subconscious psychological activity has since been unjustifiably assimilated to a notion of the unconscious prefiguring Freud's. Terminology is of the essence here. Janet explicitly sought to create a psychology of degrees of consciousness, and from the outset stated his belief that consciousness of however rudimentary and obscure a kind was a concomitant of all actions, however automatic they might seem. And here his main influence is Maine de Biran. As a result, Janet sedulously avoids reference to double personalities; instead he uses expressions such as « disaggregation of the personality » and successive or simultaneous « psychological existences ». He was praised for this by his uncle:

> It would be misleading, we feel, to define as « double personality » the empirical double existence that we have just described (Paul Janet, 1897 : 567).

Pierre Janet concludes his chapter on simultaneous psychological existences in strikingly ambiguous fashion:

> We shall, it seems, have to move the boundaries of the metaphysical person still further back, and consider the very idea of personal unity as an appearance that can undergo modification. Philosophical systems will undoubtedly contrive to accommodate these new facts, since they seek to express the reality of things, and one expression of truth cannot be at variance with another. (Pierre Janet, 1889: 323).

He thus issues a twofold plea: for a metaphysics reformed in the light of his experiments, and for a compromise between the old psychology and the new facts.

The ambiguity of the thesis gave way to a more eclectic formulation in Pierre Janet's 1896 Manuel du *Baccalauréat*[3], which went through numerous editions[4]. In 1902, two themes involving pathological psychology, psychological heredity and psychological automatism were introduced in the *baccalauréat,* and the Manuel was reorganised to fit the new syllabus. Pierre's course book followed in the footsteps of Paul's, and might even be described as plagiarising it. On the subject of « psychology' » Pierre restates, almost in the same words, many of the arguments of the *Traité élémentaire de philosophie.* It gives pride of place to the same authorities: Bossuet, Maine de Biran – described as « one of the most interesting and ingenious of French psychologists » (Pierre Janet, 1904 : 441) – Jouffroy, Garnier and the Scottish philosophers. Pierre Janet explicates his own pathological psychology, and emphasises the work of Tarde in his chapter on sympathy and imitation. But he fails to cite the most famous philosopher of his time, Henri Bergson, even when speaking of the duration of states of consciousness and their incommensurability (ibid : 10). By contrast, in the almost contemporary survey conducted by Alfred Binet among one third of French philosophy teachers, certain teachers had cited Bergson as a major influence. On this point, then, Pierre Janet had not updated his Manuel, and gave the impression that spiritualism had not progressed beyond Paul Janet[5].

From the outset, psychology is defined as « the science of the facts of consciousness and of their laws »(ibid : 8). Psychological phenomena differ by their nature from physiological phenomena and give rise to a reflexive knowledge described as « certain » and « infallible », which forms the basis of « the moral sciences »(ibid : 14). This is the starting point of Pierre Janet's account of psychology, and it falls squarely within the spiritualist tradition. However, subjective observation must be supplemented by objective observation and experiment, in particular by the natural experiments afforded by « illnesses of the mind »(ibid : 17). In this way the new pathological psychology becomes compatible with traditional introspective observation.

Such eclecticism is a characteristic of the *Manuel.* Perhaps the most striking example is the chapter on *La personnalité et l'idée du moi.* Pierre Janet states that « multiple personalities » do indeed exist, and classes them as pathological phenomena(ibid : 179). Accounting for them did not require one either to adopt the sensualist and organicist critique advanced by Ribot or to assume that direct intuition of the unity of the self is possible. Pierre Janet puts forward a « constructive theorie »':

> The unity and identity of the personality, far from being granted from the first moment of life as intuitions, far from being the mechanical result of sensation itself, must be gradually

acquired and constructed. The unity of the personality is the ideal and endpoint of our efforts. (ibid : 181)

Thus, for Pierre Janet, the sensualists are right about the inferior and primitive states and the spiritualists about superior and evolved ones. As in *L'Automatisme psychologique*, the evolutionism underlying this argument remains implicit. Pierre Janet abandons the immediate intuition of self so dear to Paul Janet, but contrives to save the unity of self under the heading of « mental synthesis »; he thus makes the fact of personality the *terminus ad quem* of a process of evolution identified as progress toward an ideal.

Evidently, a course book is not the place to set out one's own notions, and Pierre Janet scrupulously confines himself to the subjects specified by the syllabus of the *baccalauréat*. A similar kind of conciliatory eclecticism characterises other course books of the period, such as that of Elie Rabier. Bergson's classes were similar. What is significant for our purposes is that, after his two theses had conferred on him the status of a rising star in French scientific psychology, Pierre Janet still felt the need to follow in his uncle's footsteps and publicly assume the identity of a philosophy teacher. Probably he wanted to make some money from a much-used and much-revised course of philosophy. But comparison with Ribot makes it clear that Janet found no contradiction between the status of philosophy teacher and that of scientific psychologist. Whereas Bergson refused to publish his lessons, Janet made no distinction between teaching and writing. Indeed, publication of his courses accounts for the majority of his works.

In 1925, he entirely reworked his long-serving *Manuel* to adapt it to the new syllabus of 1923. On the subject of aesthetics, he enlisted the collaboration of Charles Lalo. For experimental psychology, more significantly, he chose Henri Piéron, Professeur of the College de France and Director of the Laboratoire de Psychologie of the Sorbonne. For more than 50 years Piéron had masterminded the institutionalisation of experimental and applied psychology in France[6]. In this manual, the general psychology sections were eliminated. The new course book comprises two juxtaposed parts, which could be sold as separate fascicules: *Elements de psychologie expérimentale* by Henri Piéron (pp. 1-108) and *Elements de psychologie pathologique* by Pierre Janet (pp. 109-160). Janet thus gave physical form to a divide between two orientations in French psychology. The divide has lasted till the present day.

It is clear from Janet's contribution that he had renounced the spiritualism of his debuts, replacing it with his own system. This is an evolutionist psychology of behaviour involving a hierarchical ordering of tendencies. Yet it remains, in essence, a development of the psychology that he had initiated in 1889. Chapter IV, which deals with « illnesses of the personality » summarises and explicates most of the themes of *L'Automatisme psychologique*. Pierre Janet thus integrated his psychology into his philosophical curriculum, where it maintained the tradition of spiritualist psychology[7].

Conclusion

The traditional historical account of the « discovery of the unconscious »' perceives the work of the young Pierre Janet as highly innovative. This impression has been reinforced by the revival in the United States of the theme of dissociative problems. But to perceive Janet in this way is to dissociate him from his own time and map present-day concerns onto him. In this study, we have attempted to restore the context that ensured the success of a young philosophy teacher, Pierre Janet, and almost immediately bestowed classic status on his writings. In our view, Pierre Janet attempted to construe psychology in a way compatible with what Paul Janet called the « very solid doctrine of the unity of consciousness, without which everything disintegrates into universal illusion » (Paul Janet, 1897 : 570).

Our point of view is parallel to that expressed in a *boutade* by the psychiatrist Eugène Minkowski. In 1939, Minkowski noted that Pierre Janet had defined himself as an « uneasy spiritualist ». Janet himself gave some credibility to this point of view in 1942, in his preface to Jean Delay's *Les Dissolutions de la mémoire*. Delay's book was a neo-Jacksonian treatise that drew on the evolutionism set out by Janet in his courses at the College de France. In this preface, Pierre Janet notes nostalgically (and perhaps a little mischievously) that he accepted the interpretations of his own work by the Catholic philosopher Jean Paulus (1941). And Paulus had

sought the derivation of Janet's youthful notions, over and beyond Spencer and Jackson, in the old French psychology of Jouffroy and Baillarger.

Our starting point was a French orthodoxy that sees in the break with philosophy the *sine qua non* of the foundation of French scientific psychology. After our examination of the exemplary case of Pierre Janet, this is no longer tenable. It is, perhaps, a specifically French foundation myth, intended to hide the ambivalent relations between psychology and its philosophical roots.

References

Azam, E. (1876) « Amnésie périodique ou doublement de la vie », *La Revue Scientifique* X : 481-489.

Baillarger, J. (1890 [1845]) "Théorie de l'automatisme", in *Recherches sur les maladies mentales*, Tome 1. Paris : Masson, pp. 494-500.

Bergson, H. (1990). *Cours*, Tomes I &II. Paris : Presses Universitaires de France.

Binet, A. (1890) "Pierre Janet. L'Automatisme psychologique", *Revue philosophique* XXIX : 186-200.

Binet, A. (1908) « Une enquête sur l'enseignement de la philosophie », *L'Année psychologique* XIV : 152-231.

Ellenberger, H. (1974 [1970]) *A la découverte de l'inconscient. Histoire de la psychiatrie dynamique* [The Discovery of the Unconscious. The History and Evolution of Dynamic Psychiatry]. Villeurbanne: Simep.

Fabiani, J.L. (1988) *Les philosophes de la République*. Paris : Editions de Minuit.

Gauchet,M. (1992) *L'inconscient cérébral*. Paris : Seuil.

Hannequin, A (1890) *Cours de Philosophie. Introduction à l' étude de la psychologie*. Paris : Masson.

Janet, Paul (1876) « La notion de la personnalité », *La Revue Scientifique* X : 574-575.

Janet, Paul (1880) *Traité élémentaire de philosophie à l'usage des classes*. Paris : Delagrave.

Janet, Paul (1897) *Principes de métaphysique et de psychologie. Leçons professées à la Faculté des Lettres de Paris, 1888-1894,* Tome2. Paris : Delagrave.

Janet, Pierre (1910 [1889]) *l'Automatisme psychologique. Essai de psychologie expérimentale sur les formes inférieures de l'activité humaine*. Paris : Alcan.

Janet, Pierre (1904 [1896]) *Manuel du Baccalauréat de l'Enseignement secondaire classique*. Paris : Nony.

Janet, Pierre (1925) *Manuel du Baccalauréat. Seconde partie. Philosophie*. Paris :Vuibert.

Janet, Pierre (1942) "Préface" in Delay J, *Les dissolutions de la mémoire*. Paris : Presses Universitaires de France, pp. XI-XX.

Minkowski, E. (1939) « Pierre Janet. Essai sur l'homme et l'oeuvre » in *Centenaire de Théodule Ribot. Jubilé de la Psychologie scientifique française*. Agen : Imprimerie Moderne, pp. 199-230.

Paulus, J. (1941) *Le problème de l'hallucination et l'évolution de la psychologie d'Esquirol à Pierre Janet*. Paris : Droz.

Prévost, C. M. (1973) *La psycho-philosophie de Pierre Janet. Economie mentale et progrès humain*. Paris : Payot.

Rabier, E. (1884) *Leçons de philosophie I. Psychologie*. Paris : Hachette.

Reuchlin, M. (1980 [1957]) *Histoire de la psychologie*. Paris, Presses Universitaires de France (Que sais-je ? N° 732).

Ribot, Th. (1870). *La psychologie anglaise contemporaine*. Paris: Alcan.

Ribot, Th.(1873) *L'hérédité. Etude psychologique*. Paris : De Lagrange.

Taine, H. (1905[1857]). *Les philosophes classiques du XIXe siècle en France*. Paris : Hachette.

Taine, H. (1870). *De l'intelligence*. Paris: Hachette

Notes

1 Translated from French by Chris Miller.

2 These debates were recorded in the *Journal Officiel de la République Française* (3 December, 1873 : 7418-7419). They were sufficiently newsworthy for the newspaper *Le Temps*, the ancester of today's *Le Monde*, to report on them on 8 December.

3 The French *baccalauréat* is the exam that concludes secondary eduction. Those who pass it gain the right to unversity education.

4 An earlier and less complete edition for the syllabus of the maths option was published in 1894. The 1904 edition, which we have used, includes all the 1896 additions, but reorganises them to fit the new syllabus.

5 Bergson made his first appearance in the reworked 1925 edition of the Manuel, of which more later.

6 In the biographical index of the 1925 manual, the references to Jouffroy, Garnier, and Paul Janet are unchanged, but there are additional entries under Bergson, Freud, Pierre Janet, Henri Piéron and Jean Piaget.

7 Thereafter, all psychological course books included the progress made by the psychology of the period. Until recently, psychoanalysis had replaced psychology in course books of philosophy. It remains to be seen whether the congnitive sciences are now taking over this role.

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Descartes, Spinoza and Psychotherapy

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Summary

The aim of this paper is to revisit one contrasting thought on the notion of cause and effect between Descartes and Spinoza. This debate will be re-examined in the light of clients' knowledge of the cause and effect of their psychological distress, prior to seeking psychotherapy and in the process of receiving treatments. The treatment approach chosen for this paper is psychodynamic. This paper also aims to demonstrate how psychotherapeutic ideas can contribute to philosophical debates.

While the influence of philosophical thoughts and debates upon psychology has been well recognized (MacDonald & MacDonald, 1995; O'Donohue & Kitchener, 1996; Valentine, 1992; Robinson, 1995), such influence is also evident in psychotherapy, e.g. psychoanalysis and existential psychotherapy (Cavell, 1993; Deurzen-Smith, 1988; Erwin, 1996; Farrell, 1994, Frankl, 1967; May, 1983; Yalom, 1980). However, one must not overlook the contribution that psychotherapeutic ideas can make to some longstanding philosophical debates.

The debate that has been chosen for the present paper is shared between Rene Descartes (1596-1650/1978) and Benedict (Baruch) de Spinoza (1632-1677/1993). The former was born at La Haye, France in 1596 and the latter was born in Amsterdam, Holland in 1632. Although neither described themselves as "rationalists", they were commonly regarded to be the dominant thinkers who led the movement of rationalism in the 17th century. Despite the fact that their debate has been chosen for the present paper, this does not mean that these two great thinkers had actually met and discussed philosophical issues. In fact, when Descartes died, Spinoza was only 17 years old and had never read any of Descartes's works (Cottingham, 1988). However, as Spinoza developed his philosophy, it became evident that his works were significantly influenced by that of Descartes. It also became evident that during the development of his philosophy, Spinoza refuted many aspects of Descartes's philosophy. The word "debate" indeed reflects their contrasting views.

It is beyond the scope of this paper to point out all of their contrasting views. Deleuze (1990) has recently summarized some of their debates, one of which has been chosen to be the basis of our present discussion. It focuses on the notion of cause and effect. Simplistically put, Descartes believed that we have a clear and distinct knowledge of an effect before we have a clear and distinct knowledge of its cause. He demonstrated this by referring to his famous proposition, "I think, therefore I am". That is, I know that I exist as a thinking being (the effect), before I know the cause of my existence. In other words, a clear and distinct knowledge of an effect presupposes the knowledge of its cause. Descartes then went on to say that the knowledge of the cause is a confusing one. Thus, a clear and distinct knowledge of an effect presupposes a confusing knowledge of its cause and never depends on a perfect knowledge of the cause.

Spinoza on the other hand, viewed this notion of cause and effect quite the opposite from Descartes. He said that although a clear and distinct knowledge of an effect presupposes the knowledge of its cause, we will not know anything about the cause beyond what we consider in the effect. Thus, he suggested that we need to obtain what he called "adequate knowledge" of universal causes. To describe in detail his theory on adequate knowledge is beyond the scope of this paper, but it can be briefly summarized as follows.

Adequate knowledge refers to the knowledge of the determined causal laws in Nature or Substance or God (these three terms mean more or less the same thing, i.e. the word God does not mean that which we know in Christianity). In other words, this knowledge conveys the necessary truth which necessarily and logically constitutes Nature. It is a universally true knowledge by which we discriminate genuine knowledge from confused and uncertain judgement. It is worth noting that according to Spinoza, acquisition of this adequate knowledge does not mean a complete knowledge or understanding of all possible universal laws.

Such adequate knowledge proceeds from universal causal laws to the effect. The idea is that an effect cannot be known except when its universally determined causes are already and better known. In other words, Spinoza believed that it is not enough to show how the effect depends on the cause (Descartes); rather we need to show how the knowledge of an effect depends on the universally agreed knowledge of its cause.

Descartes: Prior to Psychotherapy

While the above debate has been discussed by professional philosophers, many of whom take an analytical approach (e.g. Bennett, 1984; Curley, 1994; Hampshire, 1988; Parkinson, 1993), I wish to examine it from the following two perspectives, that is, focusing on clients' knowledge of the cause and effect of their psychological distress prior to seeking psychotherapy, and whilst in the process of receiving treatments, respectively.

Although it is difficult to generalize, it is usually the case, with some exceptions, that people seek psychotherapy because they are experiencing psychological distress. In other words, prior to receiving psychotherapy, some clients (type one) are aware of the fact that they are suffering from psychological pain, i.e. the effect, though they might not be aware of the exact symptoms which constitute this distress or of its cause.

As an example, a lady who attended therapy for about one year said, in a preliminary meeting (before the first therapy session), that she did not know why she had come but she felt that she needed to come. She had been feeling "distressed" and "fed up". However, the intensity of these feelings varied and was not consistent enough to be identified as "clinical symptoms". That is, she did not know what constituted her distress. Neither did she know what caused it. At this stage, seemingly, Descartes's claim is partially right in that this client has a clear and distinct knowledge that she is experiencing psychological distress, i.e. the effect; however, she finds the essence of it confusing. Likewise, as Descartes rightly pointed out, she also finds the cause of her distress confusing.

However, a closer look at the above type one clients' experiences of psychological distress actually reveals a weakness in Descartes's claim. It is noteworthy that when these clients claim that they have a clear and distinct knowledge that they are experiencing psychological distress, they are reporting an "empirical" knowledge. That is, this knowledge is based on their immediate sensory experiences rather than intellectual and logical reasoning. In other words, these clients have a clear and distinct "empirical knowledge" of their distress (effect) which is, however, not the kind of knowledge to which Descartes would refer as clear and distinct. This is due to the fact that he was essentially a rationalist who had based much of his thinking on logical reasoning, as opposed to empirical knowledge. That is, for clients to say that they have a clear and distinct knowledge that they are suffering from psychological distress, they have to have gone through a stage of intellectual or logical inquiry. However, type one clients have not done so. In that sense, one can argue that they still have not obtained the Cartesian sense of clear and distinct knowledge of the effect. Hence, the effect is still confusing to them in the same way as the cause.

To expand the argument further, prior to therapy, some clients have an "empirical knowledge" of their distress, like the type one clients, and also have "some ideas" about the symptoms of the distress (the effect). Let's call them the type two clients. They now wish to understand the "exact symptoms" which constitute it, i.e. understand the exact nature of the effect. For instance, another lady, prior to therapy, said that she knew that she was suffering from psychological distress. She also knew that in her distress, she regularly experienced tearfulness, as well as anger and rage. However, in addition to these symptoms, she did not know if there were others accompanying them, and whether or not these other symptoms would make her commit suicide or harm others. Neither did she know if they would eventually drive her to "madness". Seemingly, this lady, a type two client, now wants to pursue the Cartesian sense of clear and distinct knowledge of her distress by investigating the essence of it, implying an intellectual or logical inquiry rather than simply relying on her empirical knowledge of the distress. Again, at this stage, she did not know what caused her distress.

There are some other clients (type three) who believe that they have the Cartesian sense of clear and distinct knowledge of their distress (the effect), and now want to seek psychotherapy mainly in order to find out the causes and treatment of it. They have a clear and distinct knowledge that their

distress is composed of, for example, depression, panic attack and severe anxiety. They also know that they have been experiencing these for a certain period of time and know what they could make them do. In fact, they are so familiar with these symptoms that they can articulate them to their therapists at great length. To a large extent, these clients' experiences echo Descartes's claim more fully, in that they might have gone through a kind of intellectual inquiry and subsequently obtained a clear and distinct knowledge of their distress. However, the cause remains confusing to them.

There is a further type of client (type four) which comprises those who have the Cartesian sense of clear and distinct knowledge of their distress, like the type three clients. However, contrary to Descartes's claim, they also have a clear and distinct knowledge that the symptoms of their distress are due to certain causes. That is, they have a clear and distinct knowledge of both the effect and the cause. They seek psychotherapy mainly to find out how to reduce the intensity of their distress. For instance, a man, in a preliminary meeting prior to therapy, said that he suffered from panic disorder and could manifest all the typical symptoms, whenever he found himself in crowds. He also knew that this was due to the fact that he had been involved in a fire in a very crowded underground station some months before. It is possible that this man has gone through an intellectual inquiry of his psychological and physical reactions to different crowded situations, which consequently led him to obtain this clear and distinct knowledge of both the cause and effect of his psychological distress. Seemingly, this case casts doubt on Descartes's claim.

Descartes in the Light of One Principle of Psychodynamic Psychotherapy

Let us now examine Descartes's claim from the second perspective which focuses on clients' knowledge of the cause and effect of their distress, whilst they are in the process of receiving psychotherapy. For the sake of discussion, psychodynamic psychotherapy has been chosen. Many principles of psychodynamic psychotherapy have been described and explained in detail (Malan, 1993; Brown & Pedder, 1991; Patton & Meara, 1992) and are too numerous to be included in the present discussion. However, there is one principle which is of interest to this discussion. In psychodynamic psychotherapy, there is an intimate or interdependent relationship between clients talking about their distress, exploring its causes and receiving treatments. That is, when clients describe their distress with the therapists, they are already in the process of exploring the causes, which in turn means that they are already in the process of receiving treatment. All three processes happen together, affect each other simultaneously, and do not exist independently of each other. In other words, they are essentially different expressions of the same thing, in that the more clients have talked about their distress, the more they would have explored the causes, and in turn, the more they would have received treatment.

In the light of the above principle, psychodynamic psychotherapists might ask how it is possible that clients, according to Descartes, can have a clear and distinct knowledge of their psychological distress (effect) and yet not know the cause during treatments. They argue that during treatments, if clients truly claim that they have a clear and distinct knowledge of their distress, they would have adequately described their distressing symptoms, and adequately explored the causes. In other words, there is no way that during treatments, clients can truly claim to have a clear and distinct knowledge of their distress without having had a clear and distinct knowledge of its cause. In that sense, Descartes's argument seems to be problematic. Of course, it is not uncommon that clients "delude" themselves by claiming that they have a clear and distinct knowledge of their distress so that they can terminate the painful exploration of causes, hence, treatments.

Spinoza: Prior to Psychotherapy

Turning to the examination of Spinoza's argument from the perspective of clients' knowledge of the cause and effect of their psychological distress prior to seeking psychotherapy, it seems that Spinoza is mistaken in thinking that the adequate knowledge of universal causal laws is what people, in this case, clients, should have. The types of clients mentioned previously have clearly demonstrated their confusion in finding clear and distinct causes, with the exception of the last type of clients (type four).

However, to be fair with Spinoza, in his philosophy, he does not deny that people can be confused about things. In fact quite the opposite, he is too aware of the confusion of cause and effect that people often experience. His explanation for that rests upon the fact that people pursue

"sensory knowledge" which is on the one hand a knowledge of common sense, and on the other hand the lowest form of knowledge that people can obtain. According to Spinoza, this kind of knowledge is derived from one's imagination, ignorance, testimony, memory, habits or from the previous ideas pressing upon oneself rather than any systematic and logical investigations. That is, this is a vague form of knowledge implying a lack of order of logical necessity. It does not represent the true causal laws of Nature but represents our common-sense knowledge or sense perception which is passive, i.e. not genuine, subjective and uncertain. Many of us, including the types of clients mentioned above, are operating with such knowledge. Thus, it is not surprising for Spinoza to know that these clients do not have a clear and distinct knowledge of the cause and effect of their distress.

Due to his awareness of the above confusion in humans, Spinoza's aim is then to help people to avoid this confusion by focusing on finding out the adequate knowledge of universal causal laws. That is why he thinks that clients should obtain the adequate knowledge in the first place. The successful acquisition of this knowledge helps people to improve or emend their intellect so that they can think "clearly" and "distinctly" about the causes of their own actions. Thus, it might appear that Spinoza's claim is mistaken but in fact what he is proposing is a form of "therapy" (Bennett, 1984; Neu, 1977) in which, if people can obtain the adequate knowledge by knowing the true causal laws of Nature, they can then know their own nature (i.e. mind and behaviour) within these universal laws of Nature. This means that they can begin to live according to these causal laws of Nature, not to their own passive emotions or sensory knowledge. Hence, they can then reduce the likelihood of falling into confusion and error. In other words, they should try to achieve an intellectual understanding where their thoughts, energy and activities are directed to act according to the logical laws and causes of Nature.

Spinoza in the Light of One Principle of Psychodynamic Psychotherapy

Keeping this Spinozistic therapy in mind, it is then more appropriate to examine Spinoza's argument by turning to the second perspective of clients' knowledge of the cause and effect of their distress, whilst receiving treatment. To an extent, there is a similarity between the Spinozistic therapy and the principle of psychodynamic psychotherapy. Spinoza echoes the latter by saying that the knowledge of clients' distress (effect) and of the cause should be intimately interrelated in treatment. That is, people should obtain adequate knowledge of the universal causal laws, as a form of Spinozistic treatment of one's intellect, which then leads to an adequate understanding of the effect. Indeed, the notions of cause, effect and treatment in Spinozistic therapy affect each other in the same way as that of psychodynamic psychotherapy. Clients' failure to explore the universal causes of Nature meant the failure in understanding the effect. This in turn meant a failure in treatment or the emendation of their intellect.

However, obviously, Spinoza's therapy is a lot broader than that of psychodynamic psychotherapists. While the latter are interested in working out the causes of clients' neurosis within their dynamic experiences (e.g. with significant others within families) or with significant experiences (e.g. sexual abuse), the former wants to search for the adequate knowledge, a clear and distinct knowledge of the determined causal laws of Nature, i.e. the laws of Nature to which our minds and behaviours are subject. In other words, as Hampshire (1972) pointed out, Spinoza is not looking for specific causes within one's specific dynamic experiences. To do so in fact leads one to fall into error. We often isolate one cause and think of it as something which makes the difference. This is indeed a failure to realize the infinite complexity of the connections between things in the temporal order. The error is that we often love or hate the particular thing which we isolate in our minds from the infinitely complex network in the common order of nature. Indeed, we should not detach ourselves or be sceptical about the infinite complexity of causes.

Final Thoughts

In closing this paper, I wish to make three final remarks. First, Spinozistic psychotherapy might appear to be too abstract for many of our contemporary psychologists and psychotherapists. The idea that one can use a "therapeutic tool", which is based on searching for something universal and on working out where one is in relation to Nature, is probably beyond the thoughts of many contemporary psychotherapists. It is therefore easy for us to be sceptical of the fact that Spinoza, being an intellectual philosopher in the 17th century, can offer some useful insights to the field of

contemporary psychotherapy. However, to see if Spinozistic psychotherapy is a realistic or an effective therapeutic tool, one needs to carry out empirical testings. Whether or not therapists and clients can ever obtain this adequate knowledge remains to be seen. That is, we should not allow our own personal bias to dismiss an opportunity in which a new therapeutic approach might come into being.

Secondly, prior to these testings, one needs to have a good understanding of Spinozistic therapy. What has been described in terms of Spinoza's claim by no means represents the entirety of his philosophy. It only provides an opening description of where Spinoza stands in relation to other thinkers. More explorations and clarifications of his philosophical thinking are needed. In particular, one needs to point out that Spinozistic therapy is not a technique but a set of principles which can possibly be incorporated into other forms of contemporary psychotherapy.

Finally, while philosophical ideas can be applied to understand and explain the theory or practice of psychotherapy and psychology (Erwin, 1996; Griffiths, 1994; Spitzer & Maher, 1990), this paper has demonstrated an attempt to understand or examine philosophical ideas through psychological or psychotherapeutic ideas. Philosophy and psychotherapy should no longer stand by themselves. Perhaps, one of the main aims of contemporary psychotherapy research is to continue to show how philosophical ideas can be applied to psychotherapy and how psychotherapeutic ideas can contribute to philosophical debates.

References

Bennett, J. (1984). *A Study of Spinoza's Ethics*. New York: Hackett Publishing Company.

Brown, D., & Pedder, J. (1991). *Introduction to Psychotherapy*. London: Routledge.

Cavell, M. (1993). *The Psychanalytic Mind: From Freud to Philosophy*. Massachusetts: Havard University Press.

Cottingham, J. (1988). *TheRrationalists*. Oxford: Oxford University Press.

Curley, E. (Eds.) (1994). *A Spinoza Reader*. Princeton: Princeton University Press.

Deleuze, G. (1990). *Expressionism in Philosophy: Spinoza*. New York: Zone Books.

Descartes, R. (1978). *A Discourse on Method*. Tran. by J.Veitch. New York: Everyman's Library.

Deurzen-Smith, E.V. (1988). *Existential Counselling in Practice*. London: SAGE.

Erwin, E. (1996). *Philosophy and Psychotherapy*. London: SAGE.

Farrell, B.A. (Eds.) (1994). *Philosophy and Psychoanalysis*. New York: Macmillan College Publishing Company.

Frankl, V.E. (1967). *Psychotherapy and Existentialism*. New York: Washington Square Press.

Griffiths, A.P. (Eds.) (1994). *Philosophy, Psychology andPpsychiatry*. Cambridge: Cambridge University Press.

Hampshire, S. (1972). *Freedom of Mind*. Oxford: Clarendon Press.

Hampshire, S. (1988) *Spinoza*. New York: Penguin Books

MacDonald, C., & MacDonald, G. (1995). *Philosophy of psychology*. Oxford: Blackwell.

Malan, D.H. (1993). *Individual Psychotherapy and the Science of Psychodynamics*. Oxford: Butterworth Heinemann.

May, R. (1983). *The Discovery of Being*. New York: W.W.Norton & Company.

Neu, J. (1977). *Emotion, Thought & Therapy*. London: Routledge & Kegan Paul.

O'Donohue, W., & Kitchener, R.F. (Eds.) (1996). *The Philosophy of Psychology*. London: SAGE.

Parkinson, G.H.R. (1993). *Spinoza's Theory of Knowledge*. Hampshire: Gregg Revivals

Patton, M.J., & Meara, N.M. (1992). *Psychoanalytic Counselling*. New York: John Wiley & Sons.

Robinson, D.N. (1995). *An Intellectual History of Psychology*. London: Arnold.

Spinoza, B. (1993). *Ethics*. Trans. by A.Boyle. London: Everyman.

Spitzer, M., & Maher, B.A. (Eds.) (1990) *Philosophy and Psychopathology*. New York: Springer-Verlag.

Valentine, E.R. (1992). *Conceptual Issues in Psychology*. London: Routledge.

Yalom, I.D. (1980). *Existential Psychotherapy*. New York: Basic Books.

Jacques Dane

A Visual History of Psychological Testing

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Summary

The Archives of Dutch psychology made a slide series on the history of psychological testing. The series shows a lively image of the development of one of the central fields of psychological practice. Although psychological testing is a twentieth century phenomenon, attempts to assess the particular psychological propensities and capacities of individuals have a much longer history. Various techniques like physiognomy, phrenology, and the more recent projective and intelligence tests are shown on the 32 slides and give an impresssion of the background of psychological testing and the context in which the test were used.

Trudy Dehue

Establishing the Experimenting Society: Scientific Experimentation as a Social Theory + Social policy

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Summary

In the spring of 1997 the Dutch minister of Health, Welfare and Sports advanced a remarkable proposal in the war against drug addiction. A scientific experiment should establish whether the condition of severe heroin addicts improves if they get their drugs for free. Furthermore, the experiment should determine whether free heroin reduces the social trouble caused by chronic addicts. The present paper discusses some historical aspects of the random group design's use for experimental policy evaluation in Western welfare societies. An examination of the period in which this idea was still contested serves as entrée into the analysis of present-day experimental policy evaluation.

Introduction: an experiment with heroin

In the spring of 1997 the Dutch minister of Health, Welfare and Sports advanced a remarkable proposal in the war against drug addiction. Her department wanted a definite answer to the question whether the problems of heroin addicts, some 24,000 in the Netherlands, are caused mainly by the use or rather the high costs of the drug.[1] A scientific experiment should establish whether the condition of severe heroin addicts improves if they get their drugs for free. Furthermore, the experiment should determine whether free heroin reduces the social trouble caused by chronic addicts.

Experiments with free heroin had been conducted in Europe before, but this would be the first based on the scientific ideal of the randomized controlled design. Experimental and control groups would be compared that were composed at the basis of chance. In total, about 750 experimental subjects would be needed. The experiment would be conducted in two phases of half a year each. In the first half year the experimental group would be given heroin three times a day, whereas the control group would receive only methadon. Every two months the somatic and social condition of the groups would be systematically recorded. In order to ensure control group cooperation, members of this group would be rewarded with a 50% chance of being added to the experimental group for the next half year. As there would still be a randomly composed control group in the second half year, the experiment could be continued. The last control group would be motivated by the prospect of free heroin after a full year of cooperative participation.[2]

In 1986 the prominent British sociologist Martin Bulmer discussed the testing of governmental policies and spoke of random group experiments as "the apogee of methodological rigour" and "the Rolls Royces or Cadillacs of evaluative research design."[3] Whereas it was a novelty in the 1990s to employ the random group design for investigating the effects of free heroin, it was not a novelty to test governmental policies in this way. My paper discusses some historical aspects of the design's use for experimental policy evaluation in Western welfare societies. An examination of the period in which this idea was still contested serves as entrée into the analysis of present-day experimental policy evaluation.

Reforms as experiments

The first faltering employment of random groups for testing administrative measures dates back to the early 1920s when educational policies were tested by assigning schoolchildren to experimental and control groups on the basis of chance. In the decades thereafter use of the design for administrative purposes gradually increased, primarily in the context of education.[4]

The methodological and statistical aspects of experimental evalution became increasingly complex. Often several experimental groups receiving different treatments were involved. Statistical analysis of variance was used for drawing conclusions from the results.

In 1969 the psychologist and methodologist Donald T. Campbell published an astute article on the methodological aspects of social experimentation which would become a classic paper in social

science methodology. The paper's title, *Reforms as Experiments*, adequately summarizes the basic idea that administrative enactments should be seen as scientific experiments.[5] In *Reforms as Experiments* Campbell discussed the importance of the random group design using the example of a crackdown on traffic speeding by the state of Connecticut in 1955. One year after the imposition of the speed limit substantially fewer traffic deaths had taken place. The state governor consequently praised the success of the enforcement. Campbell, however, pointed out that the governor had been too quick to credit himself with the decrease in casualties. The simple procedure of counting the motor vehicle death toll before and after the speed reduction failed to assert whether the reduction was due to the new regulation or to one of many other possible factors. Perhaps the weather was better in 1956 than in 1955, or the difference in traffic victims had been just a chance fluctuation.

Campbell emphasized that "our needs and our hopes for a better society" demand a proper experimental approach. He specifically recommanded the random group design, indicated by him as the "true" experiment. Clear conclusions can be drawn only when treated and untreated groups are compared which are equal in all respects but the treatment. The best way to equalize the groups was to assign people on the basis of chance to either the experimental group or the control group. For cases in which no random groups could be composed, Campbell developed a range of second-best designs which he termed "quasi experiments."

The tools-to-theory heuristic

Campbell's argumentation for (as he expressed it) "extending the logic of the laboratory into the field" helps to uncover the telling aspects of a currently evident practice. His phrasings clearly display the so called *tools-to-theories* heuristic about which I will now insert a brief intermezzo.

The expression of the "tools-to-theories heuristic" was introduced into science studies by the historian Gerd Gigerenzer.[6] Gigerenzer used it in pointing at the remarkable aspect of phenomenon that theories and models in the sciences may be inspired by a discipline's *tools of research*. His main example was a theoretical change in cognitive psychology. By the 1960s, he argued, statistical inference had become such an obvious instrument in psychological research that its rules were elevated to a general model of human thinking. Psychologists began to imagine human cognition as statistical inference. In this way a supposedly neutral research tool was turned into a representation of reality. Moreover, the theory of the intuitive statistician was not a purely descriptive one. It, so to say, was only descriptive in an anticipating sense: People were supposed to intuitively *try* to apply the rules of inferential statistics, but they clearly did not succeed in doing so properly. The human mind was described as a spontaneous *though failing* statistician.

The conception of social reforms as scientific experiments provides an analogous example. The introduction of random group experimentation as a model of social reform implied that an established methodological tool inspired a particular image of social government. According to this view, social policy was a matter of experimentally testing ameliorative attempts. Moreover, just like statistical inference as a model of human cognition, the random group design both was a descriptive and a normative model. As is exemplified by Campbell's story on the Connecticut speed limit, the gist of the idea was that although social administrators tried to experiment they had no sense of how to do it properly. Whereas cognitive psychologists saw the human mind as a lay statistician, the advocates of social experiments looked upon administrators as unprofessional experimentators and society as a disorderly lab.

Making policy while testing policy

In the 1970s and 1980s social scientists increasingly offered their methodological expertise to guide "the experimenting society." The United States in particular initiated large-scale and complicated experimental tests in which innumerable experimental and control groups were compared. Many thousands of participants were enrolled in projects which often lasted several years. The effects were tested of educational interventions ranging from the toddler's television program Sesame Street to campaigns on safe sex, and of social policies ranging from welfare allowances to voting campaigns, electricity pricings, and felon rehabilitation projects.

The social scientists involved adopted a self-image of extreme neutrality. They were sheer facilitators to the aim of efficient social administration. The core of their professional identity was their methodological expertise in testing other people's claims. In this respect too, Campbell's

example of the Connecticut crackdown on speeding is a fine illustration. Campbell offered neither factual knowledge nor hypotheses from his own field. His contribution was purely methodological. Here a psychologist helped transform a politician's view into a scientific hypothesis.

Campbell summarized the image of complete disinterestedness in the title of a 1973 article "The social scientist as methodological servant of the experimenting society.[7] According to this image, social scientists simply, not to say humbly, provide the means to the experimenting society's needs. Yet, as the expression of the tools-to-theories heuristic indicates, methodology cannot be as impartial as some may think. Social science methodology may constitute models -even *normative* models- of proper reasoning and decision making.

In the case of policy testing, the tools-to-theory heuristic had many more social ramifications. However, before discussing them I would like to emphasize that I am not criticizing these developments. For a balanced assessment much more historical, sociological and political analysis would be needed. Such an assessment would have to take into account that the view of society as an experimenting society is the outcome of long historical processes in which Western democracies increasingly delegated decisionmaking from persons to procedures.[8]

My aim is to discuss the image of sheer neutrality. I argue that the relation of policy testing and policy making is much more complicated than is usually assumed. In various senses, policy testing is policy making. To begin, once methodologists had successfully advanced their interpretation of governmental problems as largely a matter of inferior experimental design, of course the special expertise of social science methodology became vital to society. From the 1970s, Western governments established special departments for evaluation research, a practice shortly imitated by the bureaucracies of other complex institutions such as hospitals, universities, and businesses. Moreover, small and large companies specializing in social program evaluation mushroomed in the private sector. Nowadays, social science methodologists form an important part of the so called "fifth branch" of science advisors to all kinds of governmental boards.[9]

The growth of scientific policy evaluation not only brought an increase in offices, departments, and civil servants, but also increasing standardization of individual behavior. In many social science disciplines, scientific objectivity has become synonymous with following the rules of experimental research. As a consequence these rules define most of the work done in the social sciences. Administrators also are expected to adhere to the mores of experimental research, as are the people voluntarily or involuntarily participating in social experiments. Experimental subjects watch prescribed television programs, undergo particular educational training, are put on diets, live in experimental houses, and take prescribed heroin or methadon. To establish the effects of such interventions subjects are observed or interrogated in standardized ways. If they can give their own opinions, these are recorded according to prespecified schemes. The inventories and classifications needed for testing the results of social policies affect their thoughts and self-images.

In conducting their experiments researchers of administrative programs rebuild society after their ideal of a well-organized lab. Rather than being neutral, the research tools transform the investigated phenomena in their image.

Facts based on decisions

Moreover, in the design of every single experiment various decisions must be made. Testing the ameliorative effects of free heroin for severe addicts, for instance, demands an inevitably arbitrary definition of "severe" addiction. How is it to be determined that someone's addiction is irreversible? Definitions are also needed for diagnosing the condition and the behavior of the participants. Have subjects significantly improved when their bodily weight is back to normal? If so, how normal is normal enough? Can it be justly claimed that distribution of free heroin diminishes social malfunctioning if addicts no longer mug fellow citizens? Or can we only speak of significant social improvement when they have functioned in a regular job for a particular period of time? If so, how does one define a regular job as well as a long enough period of time?

Policy experiments usually are conducted to decide politically hot issues on the basis of facts. However, inevitably the facts themselves are based on various arbitrary decisions. This explains why even random group experiments in fact rarely settle controversies. As history has amply demonstrated, in cases of serious disagreement every single element of an experiment may add yet another issue to the original disputes. The chair of the Dutch heroin research group was remarkably

naive when he self-assuredly presented the group's research script envisioning "a clear-cut answer" in the free heroin issue.[10]

Epilogue: political interference in experimental designs

As a matter of fact, controversies on the Dutch heroin experiment have already blown up before the project got started. The minister did not succeed in getting the approval of Parliament. Members of parliament protested against the social implications of the plan. Christian parties opposed it for its implication of incurability of severe addicts. The most remarkable opposition, however, came from the conservative party. This party welcomed the idea of an experiment with free heroin, but not according to the proposed random group design. The faction rejected the idea of "state heroin" for the control group, even if that impeded the enrollment of a control group. In addition, the conservatives objected to the large number of 750 participants which it feared would disturb public order.

Thus the methodology of scientific research became a subject of debate in Dutch parliament. The minister, herself a full professor of the methodology of treatment assessment before she entered politics, did her very best to explain the need of a guaranteed control group. She also elucidated the statistical reasons for at least 750 participants. However, her combined expertise as researcher and politician did not suffice. For the time being, she received support only for a three month project with 50 addicts investigating the effects of free heroin on public order.

This parliamentary veto on a research design caused understandable worry and indignation in the Dutch scientific community. However, I would argue that in light of the intertwined nature of methodology and politics, the methodology of politically delicate experiments cannot be a matter of "methodological servants" alone. Democratic societies should be alert to servants silenty taking over the household.[11]

Notes

1 The number of 24,000 is given in Driesen FMHM, Methadonverstrekking in Nederland [Provision of Methadon in the Netherlands]. Utrecht: Bureau Driesen, 1990.

2 This and the following information on the Dutch heroin experiment is borrowed from: Centrale Commissie Behandeling Heroïneverslaafden [Central Committee Treatment Heroin addicts], *Onderzoek naar heroïne op medisch voorschrift* [Research of heroin on medical prescription]. Utrecht: unpublished report to the Ministry of VWS, (nr 24077-54), 1997. Kerncommissie Ethiek Medisch Onderzoek [Core Committee for Ethics of Medical Research], *Heroïne op medisch voorschrift. Beoordeling van een onderzoeksvoorstel* [Heroin on medical prescription. Assessment of a research proposal]. Rijswijk (reportnumber 1996/K97/01), 1997. Tweede Kamer der Staten Generaal [House of Parliament] '24077 Drugbeleid' [24077 Drug Policy] nr. 44-60. 's-Gravenhage: Sdu Uitgevers, 1996-1998. For providing this material I am indebted to Gijs Meijer, who is currently preparing a masters thesis on the heroin experiment at the program of Theory and History of Psychology at Groningen University.

3 M. Bulmer, ed., *Social Science and Social Policy*. Londen: Allen en Unwin, 1986, on p. 159 en p. 169.

4 T. Dehue, Deception, efficiency, and random groups. Psychology and the historical origination of the random group design. *Isis*, 88, 4, 653-673.

5 D.T. Campbell, Reforms as Experiments, *American Psychologist, 24*, 1969, 409-429. For a discussion of the article as a classic paper, see P.H. Rossi and H.E. Freeman, *Evaluation; A Systematic Approach* (London: Sage Publications, 1985), and M. Bulmer, *Social Science and Social Policy* (London: Allen & Unwin, 1986).

6 G. Gigerenzer, "From tools to theories: A heuristic of discovery in cognitive psychology." *Psychological Review, 98*, 1991, pp. 254-267; G. Gigerenzer, "Discovery in cognitive psychology: new tools inspire new theories." *Science in Context, 5,* 1992, pp. 329-350.

7 D.T. Campbell, "The Social Scientist as Methodological Servant of the Experimenting Society," *Policy Studies Journal, 2,* 1973, pp. 72-75.

8 See T. M. Porter, *Trust in numbers. The pursuit of objectivity in science and public life*. Princeton: Princeton University Press, 1995.

9 On natural scientists in the fifth branch see Jasanoff, S. 1990. *The fifth branch: Science advisers as policymakers*. Cambridge: Harvard University Press.

10 Ree, J.M. van, Voorwoord [Foreword], in Centrale Commissie Behandeling Heroïneverslaafden [Central Committee Treatment of Heroine addicts], *Onderzoek naar heroïne op medisch voorschrift* [Research of heroine on medical prescription]. Utrecht: unpublished report to the Ministry of VWS (nr 24077-54), 1997.

11 For a similar conclusion with regards to the influence American AIDS activists had on the design of clinical experiments see H. M. Marks, *The progress of experiment. Science and therapeutic reform in the United States, 1900-1990*. New York: Cambridge University Press, 1997.

Douwe Draaisma and Sarah de Rijcke

The Graphic Strategy: Illustrations of Experimental Apparatus in Wundt's *Grundzüge*

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Summary

Wilhelm Wundt included an impressive amount of illustrations in the six editions of his *Grundzüge der Physiologischen Psychologie* (published between 1873 and 1911). These illustrations were carried out as woodcuts, the most advanced technique available, and appeared with extensive explanations in the text. The last edition featured 399 figures. Clearly, illustrations served an important role in the articulation of Wundt's experimental program. Focussing on woodcuts of apparatus and experimental set-ups, we nvestigate the uses and functions of illustrations in the experimental culture of the physiological and psychological sciences. We will use Shapin and Schaffer's notion of 'virtual witnessing' and Latour's concept of 'immutable mobiles' as analytical tools to explore the strategical aspects of Wundt's illustrations.

In 1873 and 1874 the Leipzig *Verlag* Wilhelm Engelmann published the first edition of Wundt's two volume *Grundzüge der physiologischen Psychologie*. The adjective 'physiologischen' referred both to the origin and the methodological orientation of the new science. The techniques of experimental physiology were to supply the instruments for the investigation of psychological subjects. After his arrival at Leipzig, in 1875, Wundt initiated a series of research programs, mainly on reaction times and psychophysics. In 1880 the growing corpus of new findings from Leipzig and elsewhere found its way to an expanded edition of the *Grundzüge*. In the decades to follow Wundt prepared four more editions of his textbook. The sixth and last edition appeared between 1908 and 1911. By then the *Grundzüge* had nearly tripled in size.

In the preface of the first edition Wundt indicated that he wished to present an overview ('Über-blick') of psychology. The idea of an overview might tempt one to compare the *Grundzüge* to one of those magnificent nineteenth century painted panoramas, as if Wundt had found himself a high viewpoint in the landscape of psychology, made a slow turn around his axis, and produced a painted copy of what he saw. The metaphor is seductive - but not because of its suggestion of verisimilitude. Rather the opposite: Wundt's overview of psychology was the result of radical choice and selection. Like a true landscapist he left out much of what he saw. Some of the parts he did include seem to occupy a disproportionate amount of space. The *Grundzüge* was a panorama indeed: it reflected both the perspective and appreciations of its creator. This is why the subsequent editions of the *Grundzüge* present such a wonderful source of information for historians of psychology. They offer glimpses of the development in Wundt's preferences, his plans and programs, his blind spots and aversions.

We propose to discuss a prominent - if neglected - feature of the *Grundzüge*: its illustrations. In the first edition, Wundt included no less than 155 illustrations. This number grew with each new edition, up to 399 in the last edition. These illustrations were carried out in the most advanced and minute technique available, to wit woodcuts. They appeared with extensive explanations in the text. Clearly, illustrations had an important function in Wundt's literary technology. We feel that they are worthy of historial investigation. What did they depict? What did Wundt hope to gain by including them? How did they fit in his program for psychology? We shall focus our discussion on illustrations of apparatus and experimental set-ups. But first we will present some statistics on the number of illustrations in the six editions of the *Grundzüge*.

W. Wundt, *Grundzüge der physiologischen Psychologie*, Leipzig: Engelmann.

Edition		Pages	Illustrations	Apparatus	
I	(1873/74)	863	155	6	(4%)
II	(1880)	963	180	8	(5%)
III	(1887)	1098	210	17	(8%)
IV	(1893)	1248	237	35	(15%)
V	(1902/03)	2035	384	64	(17%)
VI	(1908/11)	2317	399	68	(17%)

As the table shows, the number of illustrations increases proportionally with the number of pages; in all six editions there is roughly one illustration to every five pages. Most of these illustrations are anatomical drawings, but there are also diagrams, curves and visual illusions. Wundt kept his illustrations carefully up to date; the fourth edition of 1893, for instance, contains a discussion of the Müller-Lyer illusion, published in 1889, with eight new figures. The relative sizes of these first four categories of illustrations remain roughly constant in subsequent editions. There is a significant shift, though, in *what* the illustrations show: in the first edition the curves and diagrams depict predominantly *physiological* processes, whereas in later editions representations of *psychological* processes - like the relation between time and forgetting or changes in concentration - gain prominence. Even if psychology had its origin in physiology, the new science had to build up an autonomous collection of methods, theories and findings. The gradual, but accelerating shift from physiological to psychological representations was at once the expression of this program and a forceful support for it.

A fifth category of illustrations showed experimental apparatus. The first edition has woodcuts of stereoscopes, experimental set-ups for reaction time measurement and Wundt's famous *Pendelapparat* for complication trials, all in all six illustrations. In the editions to follow this number expanded quickly, at times even exponentially, up to 68 in the last edition. Why so many? And to what purpose?

First of all it should be noted that the inclusion of illustrations in a handbook of 'physiological psychology' was quite unusual. If one inspects the monographs and handbooks on the 'new psychology' published between 1872 and 1893, one finds that both the *number* and the *diversity* of Wundt's illustrations are exceptional. The author who resembled Wundt the most was, surprisingly, William James, who included 94 illustrations in his *Principles of psychology*. That was in 1890. By then, Wundt was preparing an edition of the *Grundzüge* which was to contain 237 illustrations. Even more remarkable is that practically none of Wundt's colleagues in psychology included illustrations of experimental apparatus. Sergi (1888) has six drawings of instruments, James (1890) has two and that is all. In this respect the first edition of the *Grundzüge* was a glaring exception. And so was the second, and the third and the fourth. In the 'new psychology', one is forced to conclude, Wundt had no conventions to guide or inspire him.

How can we get to grips with all these somewhat anomalous woodcuts? One way to answer this question is to find Wundt a background in which he does *not* appear as an exception. This background is to be found in the science he was educated in and that shaped his early professional career: physiology. Physiological textbooks from the period routinely contained a sizable number of illustrations, invariably woodcuts, often showing apparatus and experimental set-ups. And for good reason: the development of a new experimental procedure or a new piece of apparatus was part and parcel of the profession *itself*. All physiologists of fame had new techniques and instruments to their credit. These innovations were described in articles as important discoveries and named after their author: Brücke's dissection glasses, Helmholtz's ophtalmoscope. Contributions to the technical conditions of research added significantly to a physiologist's professional prestige.

This is the very convention, we argue, that Wundt was following in the *Grundzüge*. The descriptions and depictions of experimental machinery were the expression of a style of professionalization which was directed at the new domain of psychology, but originated from

physiology (Draaisma & De Rijcke, 1998). The paradox to emerge from this is that Wundt, writing his thick tomes on the new psychology, resembled his *former* colleagues more than his present peers.

Eyewitnesses and allies

If this is where the convention comes from, what were the functions of these illustrations? What were their effects and consequences, intended or unintended? Before we shift our attention to these more strategic aspects, we should briefly discuss two notions which may help us analyse Wundt's treatment of illustrations: the concepts of 'virtual witnessing' and 'immutable mobiles'.

In their classic study of the Hobbes-Boyle controversy on the nature of the void, Shapin and Schaffer (1985) have described how Robert Boyle went about securing a collection of 'matters of fact'. The production of these facts, they point out, demanded the use of several 'technologies'. The *material* technology referred to the Pneumatick Engine, the air-pump with which Boyle gathered his facts. The details of this machine and its operation need not detain us here; suffice it to say that it was an extremely elaborate instrument, demanding endless preparations and adjustments. It was also very costly to build, which may explain why there were so few air-pumps available. This created both a methodological and a social problem. According to Boyle, each member of the scientific community should have the opportunity to witness the experiments with his own eyes. In practice, however, experiments were performed for audiences of limited size. Those who were granted access to the laboratory were assigned the role of eye witnesses and the credibility of their testimony depended - as in court - upon its multiplicity and agreement. For this reason Boyle requested his guests to sign a register as 'Witnesses of all the said Proceedings, who, by Sub-scribing their Names, will prove undoubted Testimony.'

Another way to increase the number of witnesses was the mobilization of what Shapin and Schaffer have called 'virtual witnesses'. This mobilization was to be achieved by employing a *literary* technology. Boyle presented the narrative of the experiments as graphically and circumstantially as possible. He also offered visual assistance in the form of engravings in a naturalistic style, to give the reader the distinct impression that he witnessed the experiments himself. One of the engravings shows a dead mouse in the receiver of the air-pump. These realistic representations, Shapin and Schaffer argue, 'served to announce, as it were, that "this was really done" and that "it was done in the way stipulated"; they allayed distrust and facilitated virtual witnessing.' (p. 62) By including illustrations, Boyle secured that the actual witnesses were joined by virtual witnesses - a company increasing in size each time a reader took up the written report.

The notion of witnessing is equally essential in Latour's analysis of visual material in the dealings of scientists. Latour (1990) has introduced the notion of 'immutable mobiles': representations, usually on paper, which may be transported, copied and distributed, while at the same time guaranteeing a certain stability and constancy. An explorer who draws a profile of some foreign shore to take back home is creating an immutable mobile. Photography is a forceful manufacturer of immutable mobiles: all copies are identical, while the great number allows for circulation. Immutable mobiles serve to find allies. If it is inconvenient or impossible to show the thing itself - an eclipse of the sun, an island, an instrument -immutable mobiles may represent it. Latour singled out technical drawings as a species of immutable mobiles with a significant rethorical force, especially when they are associated with precision and objectivity. For this reason, mobilization processes in science have become intimately connected with metrology, a branch of science dealing with the design of instruments and methods for measurement and the development of standards. All attempts at measurement and standardization *ipso facto* support the production and distribution of immutable mobiles.

The export of brass, mahogany and psychology

Returning to the scientific culture which shaped Wundt -mid-nineteenth century experimental physiology - we may recognize several elements from the analyses of Shapin and Schaffer and Latour, although in a different configuration. The public nature of science, initiated by Boyle, has become a selfevident value. The way this openness is given shape in a literary technology, however, has changed dramatically. The authority of experiments no longer depends on something as personal as the testimony of trusted witnesses, much less on the convictions of those who, by reading the report, got the *impression* they had witnessed the experiments. The validation of facts is now being

organized by publishing the relevant experimental details in one of the many *Annalen* and *Archive* available to physiologists. The 'virtual witnesses' in *this* culture are fellow researchers who may use the specifications in the report to repeat the experiments. In the literary technology to serve this new type of 'virtual witnesses' illustrations are still essential. They show experimenters how to conduct the trials and handle the instruments. Illustrations have become visual instructions in a text designed to guarantee the replicability of the experiment. This change of function called for a change of style. Illustrations now had to be 'realistic' in a different sense: they should depict the instrument as exact as possible, in a schematic and naked way. No dead mice, just the hardware, in suitable scales and proportions. In this new technique for the 'multiplication of the witnessing experience' immutable mobiles were indispensable. They had to indicate to researchers in other laboratories how to create a satisfactory resemblance between their own experiments and the original ones. Illustrations had become means to re-create experiments.

Such were the material and literary technologies of physiology - and Wundt managed to introduce and distribute these in psychology. In Latourian parlance, Wundt succeeded in mobilizing allies in the scientific community, psychologists who were prepared to subscribe to his observations, discoveries, inventions, theories and methods. As a token of assent they placed their signatures - not, as in the days of Boyle, in a register after the trials, but as authors of dissertations, articles and books in which the results of experimental research Wundtian style were established as facts.

Once in full swing, this process of distribution could be seen to have a wonderful selfperpetuating quality. But how was one to get it *started*? Heading an official psychological laboratory, Wundt was in the position to supervise graduate students and assign research projects. In this way at least two generations of psychologists received a thorough instruction in Wundtian psychology. They learned how to arrange the instruments, how to subject the results to statistical analysis, how to report the findings. This immersion in Wundtian methods and manners contributed much to the distribution of experimental psychology à la Wundt. The Leipzig laboratory attracted students from all over the world, many of whom became founders of laboratories themselves. In 1900 twelve of the 43 new laboratories in America had been founded by pupils of Wundt (Garvey, 1929). Many of these laboratories were virtual replicas of the Leipzig lab.

A second way to get the process of distribution started was connected to Wundt's passion for the technology of measurement. One of the pivotal ideals in his experimental program was the refinement of the standards for precision measurement. This ideal led Wundt to a relentless pursuit of the smallest fraction of a second to be measured and registered reliably. Psychological metrology demanded considerable efforts, of the most diverse nature: calibrating and checking instruments, standardizing experimental procedures, developing conventions for notation, disciplining experimenters and subjects (Benschop and Draaisma, 1998). One of the ways to reduce a potential source of error - human intervention - was the rigorous 'electrification' of the experimental set-up. Typically, even a 'simple' reaction time experiment involved at least four instruments, all electrically operated. The production of the stimulus and the registration of the reaction were both fully automated electro-mechanical processes. The result was a form of experimentation which Daston and Galison (1992) have called 'mechanical objectivity'. The instruments designed by Wundt and his collaborators were links in a metrological chain demanding only a minimum of human intervention.

The development of this material technology can be followed closely in Wundt's literary technology. Each new instrument was explained, shown and patented, first in articles, often in the *Philosophische Studien*. Then the next edition of the *Grundzüge* would include a full description of the new invention, securing its place in the standard equipment of a psychological laboratory. The illustrations that went with these descriptions acted as immutable mobiles, facilitating distribution. Hence, they had to be exact, clear and minute. All suggestion of artisticity was shunned; Wundt's woodcuts were, if one may use so paradoxical an expression, emblems of functionality. They resembled technical drawings. Combined with the lengthy explanations, they instructed the reader how to handle the instrument or how to build a replica.

That the illustrations in the *Grundzüge* were actually intended to facilitate replication is confirmed by Wundt's preface to the fourth edition (1893). There he states that psychology has developed its own, independent methods and techniques, and that these are illustrated in 'carefully drawn woodcuts'. He says he hopes that 'this expansion will be appreciated by those readers who are

conducting psychological research themselves.' (Wundt, 1893a, p. VIII) The detailed instructions, both verbal and pictorial, articulated the codes and conventions Wundt wished to be heeded.

E. Zimmermann, Präcisionsmechaniker

The fact that the distribution of Leipzig experimental culture was closely connected with instruments, implied a crucial role for a group of professionals often neglected by historians of science: technicians and instrumentmakers (Shapin, 1989). In 1887 the instrumentmaker Ernst Zimmermann set up his workshop in Leipzig (Gundlach, 1986). He offered his services both to the physiological and the psychological laboratory, but over the years experimental psychology became his main market. Wundt and Zimmermann worked closely together, to the benefit of both. Often Zimmermann would construct instruments after a design by Wundt; Wundt, in his turn, agreed to test Zimmermann's prototypes in his laboratory. Once the new instrument worked to his satisfaction he included its description in the *Grundzüge*, inviting the scientific community to replicate the experiments with the instruments supplied by Zimmermann. Within a decade, Zimmermann had taken the majority of Wundt's instruments into serial production. They were shown and described in the famous *Preisliste* or catalogs. Page after page exhibits apparatus 'nach Wundt', with code-like references to the relevant literature. The very first instrument in the 1903 catalog, for instance, is a 'Demonstrations-Ophtalmotrop nach Wundt (Wdt. II. 534)', meaning that information on this instrument was to be found in volume two of the *Grundzüge*. In fact, references like these were so numerous that there wasn't much point in consulting the catalog without having a recent edition of the *Grundzüge* at hand.

Around the turn of the century, the distribution of Leipzig material technology had assumed the characteristics of commercial export. Laboratories all over the world ordered their instruments from Zimmermann or asked local instrumentmakers to copy them from the woodcuts in the catalog or the *Grundzüge*. From the fifth edition on, the mutuallity in the relationship between Wundt and Zimmermann received its final (and pictorial) expression when Wundt included illustrations supplied by Zimmermann, with the firm's name cut out sharply in the base of the instrument. Thus immutable mobiles were at once condition and consequence. Condition: the design, construction and supply of instruments facilitated the distribution of methods of measurement; consequence: once gaining momentum, distribution created an increase in demand. In his autobiography *Erlebtes und Erkanntes* (1920), Wundt remembered with gratitude the 'great merits' of Zimmermann in supplying 'numerous foreign institutes of experimental psychology' with instruments. He had every reason to be grateful: with every apparatus prepared for export in the workshop of Zimmermann a part of the Leipzig experimental culture was ready to depart for foreign countries as well.

Proof race

Following Latour's somewhat militant mobilization metaphor, one might say that Wundt manoeuvred very skillfully in his campaign for a Leipzig style of psychology. Upon returning to their home countries, several of his foreign students became true partisans for the Wundtian cause. Scripture, for instance, summoned his colleagues to observe the Leipzig standards of accuracy (Scripture, 1893). Scripture also copied Wundt's treatment of illustrations: his manual for experimental psychology contained 129 illustrations, 49 of them illustrations of apparatus (Scripture, 1897). Another of Wundt's students, Titchener, presented, to all accounts, a perfect imitation of Wundt at Cornell. His four volume *Experimental psychology* (1901-1905) featured no less than 251 illustrations, including 168 illustrations of instruments. Titchener even replicated Wundt's connection to Zimmermann, casting the Chicago instrumentmaker Stoelting in the role of Zimmermann. Closer at home, Wundt's handling of illustrations began to be followed as well. The textbooks of Höfler (1897) and Ebbinghaus (1902) each contained numerous illustrations. Even the *Zeitschrift für die Psychologie und Physiologie der Sinnesorgane*, founded in 1890 by Ebbinghaus with the veiled intention to counterbalance Leipzig psychology, adopted the same policy for illustrations as the *Philosophische Studien*.

All of this attests to Wundt's successes in what Latour (1990, p. 35) has called a 'proof race'. A proof race shares its dynamics with an arms race: the party which succeeds in supporting its position with convincing evidence, compels the other party to exceed these efforts. Thus the costs of

a rival theory or technique may make it unprofitable to compete. If a psychologist should want to develop an alternative to -say - Wundt's apparatus for checking chronoscopes, he was faced with a controlhammer which already had a secure position in experimental practice, effectively protected by a network of references in catalogs, textbooks and laboratory manuals. In many cases the prospects for producing a rival technology must have looked grim enough to prevent the project altogether. Part of Wundt's success in producing and distributing a material technology seems to have been due to the advantage of a 'first strike'. At an early stage, Wundt had created favourable conditions for the development of experimental machinery, their design, use and marketing. He hired skillful technicians, supported metrological research and finally teamed up with an instrumentmaker who proved to have a keen eye for commercial opportunities.

Wundt's literary technology was equally effective. The first edition of the *Grundzüge* appeared when there was hardly such a thing as experimental psychology. New editions appeared in regular intervals and both text and illustrations were carefully kept up to date. Even if we refrain from speculating on the intentions behind Wundt's dealings, there is no doubt about their consequences - and these were most discouraging for potential competitors. In the full thirty-eight years between its first and last edition, the *Grundzüge* remained without serious rivals. It is a telling detail that the instruments in the Zimmermann catalogs *not* 'nach Wundt', are mostly in psychological domains which were not very much 'nach Wundt' either. Memory research is a case in point: the score of 'mnemometers' featured names like Ranschburg, Hempel, Wirth, Ach, Müller, Pilzecker.

No hegemony, however, is absolute - or lasting. After the turn of the century Wundt's grand experimental programs began to lose momentum. There were reasons and causes for this, of course, but these would involve us in a new story. It would be a story of decline, though, rather than a sudden fall: there were no specific battles lost or decisive defeats. The closing decade of classic Leipzig psychology is best described as the waning of an era. For one thing, even Wundt's own interests had shifted to non-experimental areas of psychology; he had little personal involvement in the preparation of the sixth edition of the *Grundzüge*. Its heavy emphasis on sensory psychology and mental chronometry was clearly out of balance with contemporary psychology. The programs which had served for decades as paragons of precision and cutting edge science were now seen as out of touch with modern developments. The *Grundzüge* had become a panorama of a landscape which no longer existed. Engelmann *Verlag* never published a seventh edition. Even the illustrations looked oldfashioned. Woodcuts, once an advanced technique, gave the book a distinctly nineteenth century look. The newest textbooks featured photographs. By the time the last volume of the last edition of the illustrious *Grundzüge der physiologischen Psychologie* appeared, in 1911, the tide for the 'new psychology' had run out. Its 399 woodcuts presented a graphic monument for a bygone era in experimental psychology.

References

Benschop, R.J. & Draaisma, D. (1998) In pursuit of precision. The calibration of minds and machines in late nineteenth century psychology. Submitted.

Daston, L. & Galison, P. (1992) The image of objectivity. *Representations*, *40*, 81-128.

Draaisma, D. & Rijcke, S. de (1998) De grafische strategie. De illustraties in de *Grundzüge* van Wundt. *Psychologie en Maatschappij*, *82*, 52-71.

Ebbinghaus, H. (1902) *Grundzüge der Psychologie*. Bd. 1. Leipzig: Von Veit.

Garvey, C.R. (1929) List of American psychological laboratories. *Psychological Bulletin*, *26*, 652-660.

Gundlach, H. (1986) Inventarium der älteren Experimentalapparate im Psychologischen Institut Heidelberg sowie einige historische Bemerkungen. *Bericht aus dem Archiv für Geschichte der Psychologie*, Historische Reihe 9.

Höfler, A. (1897) *Psychologie*. Wenen/Praag: Tempsky.

James, W. (1890) *Principles of psychology*. New York: Henry Holt.

Latour, B. (1990) Drawing things together. In: M. Lynch & S. Woolgar (Eds.) *Representation in scientific practice*. Cambridge (Mass.): MIT-Press, 19-68.

Scripture, E.W. (1893) Accurate work in psychology. *American journal of psychology*, *6*, 427-430.

Scripture, E.W. (1897) *The new psychology*. Londen: Walter Scott.

Sergi, G. (1888) *La psychologie physiologique*. Parijs: Félix Alcan.
Shapin, S. (1989) The invisible technician. *American scientist*, 77, 554-563.
Shapin, S. & Schaffer, S. (1985) *Leviathan and the air-pump. Hobbes, Boyle, and the experimental life*. New Jersey: Princeton University Press.
Titchener, E.B. (1901-1905) *Experimental psychology. A manual of laboratory practice*. New York: Macmillan.
Wundt, W. (1873-1874) *Grundzüge der physiologischen Psychologie*. Leipzig: Engelmann. (2nd ed. 1880; 3rd ed. 1887; 4th ed. 1893; 5th ed. 1902-1903; 6th ed. 1908-1911)
Zimmermann, E. (1903) *Preisliste über psychologische und physiologische Apparate*. Leipzig: s.n.

Diana Faber

1800 - 1806

The Wild Boy of Aveyron: Social and Philosophical Considerations

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Summary

The arrival in Paris in 1800 of the "Wild boy of Aveyron" aroused excitement and speculation. J. Itard, a doctor in the Institution for Deaf-Mutes undertook the child's training over the next few years and submitted two reports (1801 nd 1806). The importance of this event is explained in terms of the opportunity offered to Itard to test Condillac's theory of the origin of understanding and knowledge. Contemporary popular philosophical questions included speculation as to the nature of "man"; his place in the universe; the origins of civilisation and of civilised persons. These philosophical considerations owed much of their currency to the works of Locke, Rousseau and to Condillac in particular. It is argued that philosophical themes and contemporary social mores determined the expectations which were aroused by the challenge to socialise and educate the "wild boy".

The story or case study of the wild boy of Aveyron, known as Victor has become familiar through various studies and accounts. Some historical studies have focussed on the pedagogical training given by the doctor Itard over the period 1801-1806; others have pinpointed this case as one of the first studies of pedo-psychiatry in which Itard attempted the habilitation of a boy who had lived in isolation from human contact in the wilds for some years. It is not the intention here to review the accounts and the judgements made on Itard's success or failure with reference to his reports, but to describe the context in both philosophical and social terms. These terms or aspects, of course, can only be understood with reference to both the time and the place in which the events of Victor's training was undertaken. This understanding explains why the case of the "wild boy" became a talking point in France and the rest of Europe in the early 1800's.

The place was the National Institution for Deaf-Mutes, Paris, near to the Jardins du Luxembourg, which had formerly been Abbé de l'Epée's school for the deaf. Because of its high reputation and this teacher's success, it was established as a National Institute in 1790 when Sicard was appointed as its new director. The Institution became famous for its care and training of the deaf. In France there had been a growing interest in the blind and the deaf since Diderot's "Lettre sur les Aveugles" in 1749. The question posed - which was also relevant to the deaf - was about the relation of sensory experience to the perceptions, ideas and hence to the educability of sensory deprived individuals. In the wake of empirical theories of knowledge proposed by Locke (1632-1704) and endorsed by Condillac (1715-1780) in France, emphasis was put on the value of observations. In December 1799, a Society for the Observers of Man was set up, of which Sicard was one of its founding members. It was noted by the Society that while systematic observations were made in the natural sciences, this method had not been extended to the study of human development. The members proposed a prize for the best account of human development. It was in this context that the doctor Jean Itard (1774-1838), was appointed to the Institute in 1800. Sicard, on the command of Lucien Napoleon, the Minister of the Interior, summoned the recently found "wild boy of Aveyron" to be brought to Paris. It was soon discovered that Victor was not deaf, but was without language. His arrival aroused considerable excitement and curiosity.

Philosophical Questions

One reason for the excitement was related to the philosophical question of the nature of "man", his place in the universe and relation to other living species, a question that was dominant in debates during the Enlightenment period, in France and England particularly. Buffon (1708-1788) had proposed that "the natural history of animals encompassed the natural history of man" (1749). Linnaeus (1707-1778) had classed "feral man" as a subspecies of homo, and since these eighteenth century proposals for the determination and position of the animal/human boundary became a

question of debate. In fact, Bonnaterre who cared for the child a few months before he was brought to Paris, applied this epithet of "feral" to the boy. A widely held belief was that the possession of language differentiated humans from animals. As the wild boy was held to be human - just - it was hoped that he could acquire language, provided that he were not classified as an "idiot".

Another popular philosophical concern was that of hypothesising and searching for origins. Of particular interest was the origin of a "civilized" man, ie. someone probably like an educated Parisian. How did such a person evolve? Since Rouseau's "Sur l'Origine de l'Inégalité parmi les Hommes" in 1752, the notion of the "bon sauvage" or noble savage had gained considerable acceptance and appeal. The addition of socialisation and education for this benign creature would then complete the process to produce a civilised human being. A demonstration of such a process could be made if only a "tabula rasa" state could be found in the first place. This idea was taken up in fiction: for example, the writer, Pierre Marivaux (1700-1763), an habitué of three famous salons in Paris became familiar with such debates and philosophical concerns. In "La Dispute" (1744), he created a fictitious tabula rasa to show the origins of jealousy and love. The arrival in Paris of the newly found child who, on some evidence, had spent most of his childhood fending for himself in the wild was thought to provide this state of nature on to which could be grafted social habits and mental achievements, notably of language.

At a more formal level the French philosophical tradition stemming from La Mettrie (1709-1751) and Condillac (1715-1780) provided the impetus for the "natural experiment" on the wild boy. In particular Condillac through his "Essai sur les Origines des Connaissances Humaines" (1746) and his "Traité des Sensations" (1754) saw himself as a follower of Locke. He claimed to be extending Locke's empirical theory of knowledge by proposing, in some detail, how each of the senses, through attention and memory led to intellectual capacities, understanding and knowledge - a Sensualist account of the origins of understanding. His metaphor of the marble statue activated by the contact of the senses with external stimuli and experience provided Itard with the theoretical base which he attempted to test empirically in his training of Victor. The condition of the boy as Bonnaterre found him was also couched in Sensualist terms, à la Condillac, thus: "If he has sensations, they give birth to no idea. He cannot even compare them with one another. As a result he has no discernment, no real mind, no memory" (Bonnaterre, 1800).

Condillac also theorised on the origins and acquisition of language via what he termed a "language of action". In addition, his pedagogical experience as tutor to the son of the duke of Parma led to some general methodological principles which Epée, Sicard and Itard adopted in turn. In 1775 Condillac recognised Epée's debt to himself, and praised his work in training deaf-mutes in sign language. This philosophical background and its themes explain some of the assumptions and expectations which informed Itard's decision to train the wild boy, and influenced certain reactions and criticisms of his experiment.

Diagnoses and Expectations

Throughout the eighteenth century, the treatment of children and adults diagnosed, formally and informally as idiots was simply to relegate them to the category of incurable and neglect them. Philippe Pinel (1745-1826), the director of the hospital of Bicêtre diagnosed Victor as an incurable idiot whose parents had probably abandoned him at a very young age; this was a custom, particularly in the country and where "enfants trouvés" were less likely to be picked up and sheltered. Pinel's view with regard to the future of Victor was therefore pessimistic. On the other hand, Itard, armed with the philosophical tools of Condillac, was hopeful of a more favourable outcome, and no doubt with some belief in the importance in offering opportunities to effect the betterment of society - an aim implicit in some post revolutionary thinking. As well as being led by his conviction that the wild boy's condition could be ameliorated, Itard's efforts were probably led by some personal ambition, as a newly appointed doctor to the Institution.

If we consider the aims which Itard set out for the education of Victor we have some idea of his expectations. He wished to attach him to social life, to afford him pleasures and variety of experience. He wished to extend his ideas and needs by appealing to, and refining his senses. In practice, this meant keeping Victor more enclosed indoors than he had been. In specific terms, it meant adding meat to his preference for a diet of vegetables, and a taste for wine to his love of drinking water. A more general expectation was that when Victor had learned to speak, he would b

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able to relate his past life. It was expected that this learning would take place within months, and that Victor would come to appreciate for example, the beauties of Paris, varieties of food and drink, and music. Gineste and Postel (1980) have commented on the heavy pedagogical programme that Itard imposed on Victor. They see this as a continuance of the tutor's role in Rousseau's "Emile" (1762). Did Itard assume too, that the tutor's role entailed constant surveillance and teaching?

Concerning Victor's psycho-sexual development, the tutor awaited with some anticipation the advent of puberty and the evidence of his attraction to, and love for women. Victor's behaviour showed that he experienced sexual arousal, but this did not manifest itself in the contemporary "civilised" manner of behaving with members of the other sex. It was at this point that Itard gave up his formal training of Victor, who was then entrusted to the sole care of Madame Guérin.

There have been various criticisms of Itard's method of training Victor. What I have found to be of interest is not so much particular pedagogical strategies which were to some extent unsuccessful, but the assumptions and expectations that initiated Itard's experiment. By the time that Itard brought it to its close in 1806, public interest in this phenomenon of the wild boy had waned. It was left to Itard's pupil, Edouard Séguin to modify Itard's methods with great success. It is generally claimed that Itard's experiment marks the foundation of a medical pedo-psychiatry. The general remark that new turning-points are embedded in social, institutional and philosophical positions is a truism, and I have attempted to show, in the case of the "wild boy of Aveyron" how specific phenomena contribute to the initiation of turning points and the direction of their course.

References

Bonnaterre (1800), *Notice historique sur le sauvage d'Aveyron et sur quelques autres individus qu'on a trouvés dans les forets à différentes époques*. Paris: Panoucke.

Buffon, comte de, (1749) *Histoire Naturelle*. Paris.

Condillac, E B (1746), *L'Essai sur l'Origine des Connaissances Humaines*. Paris

Condillac, E B (1754) *Traité des Sensations*. Paris: Debure.

Diderot, D (1749) *Lettre sur les Aveugles*. Paris.

Gineste, Th. et Postel, J (1980) J M Itard et l'enfant connu sous le nom de "Sauvage de l'Aveyron", *Psychiatrie de l'enfant*, XXIII,251-307.

Marivaux, P (1774), *La Dispute*. Paris

Rousseau, J-J (1752) *Sur l'Origine de l'Inégalité parmi les Hommes*.

Rousseau J-J (1754) *Emile, ou de l'Education*.

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Wolfgang Köhler and National Socialism

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Summary

The change at the German universities coming along with nazi government was responded by different patterns of "coordination" ("Gleichschaltung"). The Gestalt psychologist Wolfgang Köhler has been one of the outstanding persons resisting the temptations of concentration and unification. My paper gives a survey on the studies refering to the multiple dimensions of the dispute between Köhler and the nazi regime. A synopsis of the proceedings at Berlin university from 1933-1935 will be continued by a more general analysis of scientific structures under an authoritarian regime. This finally leads to a psychological point of view: can we estimate Köhler's behavior as a particular way of coping with the provocation of utterly inverted values?

The historians of psychology consider the "Berlin School of Gestalt Theory" one of the basic concepts of 20th century psychology. Going back to the second decade of our century Gestalt psychology was then a new and fascinating psychological concept derived from the mother land of modern psychology (Fitzek & Salber 1996). The reputation of the Berlin institute headed by Wolfgang Köhler was quite international. On different occasions Köhler had been invited to the second increasing centre of modern psychology: the United States. When William McDougall was to resign at Harvard Köhler naturally was discussed as his successor.

The international reputation of Gestalt psychology corresponded to the cosmopolitan style of the Berlin institute. It was not only one of the most flourishing centers of Germany psychology, having its residency in the former palace of Prussian kings and German "Kaisers". Its members derived from different social and political traditions building an "all-star cast of characters" (Henle 1978, 939). Their students came from all over the world - although psychology must be remebered then as a discipline without clear professional perspective and therefore as a rather small one.

For the Berlin institute the nazis' "coming into power" ("Machtergreifung") on January, 30th, 1933 must have looked like an incident of emergency. Its institutional variety was as much threatened as the social and political multiplicity of its members. Would a participation in the international discussion of psychological topics still be possible under a German-centered regime? And even: Could a wide-spread mixture of people - e.g. the Jewish professors Kurt Lewin and Erich von Hornbostel, the antifaschist aristocrate Otto von Lauenstein, the son of the famous communist Hermann Duncker - be maintained under the pressure of uniformism and racist policy. We now know this had been impossible.

Different ways of watching the fate and fortune of the Berlin institute can be imagined. I will focus my attention here on "Wolfgang Köhler and the National Socialism" as announced in the headline of my contribution. Though this might be the most recognized and frequently applied one, it is not the only possible focus on Gestalt Psychology under the Hitler regime. By choosing this focus I can refer to a multiple research beginning with the psychologist Mary Henle (1978) and the historian Mitchell G. Ash (1979; 1985; 1995) and followed by works of a rising interest in German history of psychology (e.g. Geuter 1984; Jaeger 1992; Sprung 1987).

But why picking up an already worked out topic? Hadn't we better leave this topic and turn to less carefully regarded aspects? I think there are at least two reasons for continuing the Köhler case: 1.) There are plenty of studies, but still no summarizing report. 2.) The study of history is not completed with the report of facts and dates drawn from archive material. On the contrary: the compilation of the historical events can be estimated as a basis for historical analysis. When facts are collected and put together the work on analysis and discussion may start.

By saying so I refer to the programmatic view on doing historical research in psychology by Wehner (1991), who calls the collection of historical data a first step of historical investigation followed by a discussion of their historical background and - as a third step applying sociological

and psychological knowledge on the data collected and commented on. In the following presentation I am only able to take a first step: collect and summarize the work of my colleagues which seems to have reached an untimely end somewhere in the 1980's.

When we consider the change coming along with the nazi government and the consequences of this change for university life we can find different phases of unifying and coordinative activities marking the unequal "dialogue" between Köhler and the representatives of nazi administration; namely the university chancellor (Eugen Fischer) and his deputy (Ludwig Bieberbach) as well as the representatives of government, the Prussian minister for science, art and public education Achelis and his state secretary (the executive director Theodor Vahlen). It will be the task of this paper to outline these phases in a chronological order.

February - March 1933: We cannot state any manifest consequences for research and teaching in the days of actual political change. Some colleagues who had been inclined to an authoritarian regime before began to be more prominent. The group of nazi students, coming up since about two years, could not be ignored any longer. Prominent members of the professorship like Max Planck, Eduard Spranger and Wolfgang Köhler consulted each other in order to estimate the irritating state of affairs. From their self-confident or even elitarian - "mandarin" (Ringer 1969) - point of view they discussed what may be justified about the nazi matter. Indeed they expected some social and cultural change as unavoidable or even as being useful. But they were disgusted at the violent and undifferentiated nazi policy. Nevertheless they arrived at different positions: Max Planck recommended Köhler to hold on his professorship whereas Spranger retired. Two months after Hitler's coming into power Köhler wrote to his friend Ralph B. Perry at Harvard: "Nobody in Germany with any decency in his bones ... knows very much about his near future... However, there will still be some fight during the next weeks. Dont't judge the Germans before it is over" (Henle 1978, 940).

April - May 1933: When the so called "Law of the Reestablishment of the Professional Civil Service" ("Gesetz zur Wiederherstellung des Berufsbeamtentums") was passed on April, 7th, 1933 it turned out that university life and order could not be maintained any longer. By this law the Nazis announced that a great part of Jewish university teachers would be dismissed as "not-aryan" or politically "unreliable". Although there seemed to be several exceptions - e.g. for honoured front soldiers in World War I - many of the leading figures of German psychology and most of the institute members were threatened by this law. Köhler's reaction was strong, but differentiated. Cautiously he began to argue in support of his fellows (April, 21th, in the case of Kurt Lewin). Ultimately he opposed the expelling of Jewish colleagues form the academic psychologists' organization (May 8). His critical, but still poised contribution to a wide-spread German magazine ("Gespräche in Deutschland" on April, 28th), was an evident criticism of the general dismissal of Jewish university teachers in Germany. Although he opened his journal article with a polite address to the new rulers, he found plain words opposing the nazi treatment of honoured and outstanding representatives in public life like the Jewish physician James Franck (Köhler 1933). Köhler was well aware of his risky endeavour. In the following night we find him in the midst of his assistants expecting an arrest troop of the Gestapo. (Henle 1978; Jeager 1992; Sprung 1987)

May - November 1933: For a short period nothing happened. Köhler's outstanding protest had offended a nazi government which was still collecting its forces and watching its impact on public life. Köhler on his part watched the changes going on in a concerned tension. Apart from concrete actions there was an expanding atmosphere of suspicion and denunciation. Some nazi students whose head (Hans Preuß) studied psychology at more adapted, but less accepted departments of the psychological institute (e.g. the activities of the nazi dozent Johann Baptist Rieffert; Geuter 1984) informed the university administration, the ministry and even the Gestapo that the Gestalt branch of the Berlin institute was dominated by communist, antifaschist and jewish forces. From now on Köhler's behavior was followed by the suspicious eyes of nazi collaborators among the students and non-scientific workers of the university. With the beginning of the fall semester they distrustfully watched Köhler giving and commenting his version of the nazi salute which had become a standard for all teaching sessions. Not satisfied with his behavior and that of the other institute members they published a pointed note of the student leader called "Has the Psychological Institute coordinated itself?" ("Hat sich das Psychologische Institut gleichgeschaltet?" on November, 15th) in which they

condemned the missing adjustment to the Nazi system. Köhler opposed to these dubious practices with clear and strong reactions. In letters to the ministry he complained of the restrictions in doing his job. In remarks to the students he announced that he did respect the nazi laws in so far as they were legal, but not their spirit. With critical remarks on the nazi salute he fulfilled his duties, but was not willing to share the world view to which they corresponded. It was against Köhler's principles to cooperate with the nazi regime, for he persisted on the autonomy of the university as an institution of science and hence got into distance. (Ash 1995; Geuter 1984; Henle 1978; Sprung 1987)

December 1933 - May 1934: In Winter 1933/34 Köhler's institute was struck by a new provocation. On account to the students' denunciations the nazi administration made several inspections of the institute colloquium. They sent uniformed troops controlling every attendant in order to find suspicious persons. The first of these actions took place in December taking all members of the institute including the institute director by surprise. Afterwards Köhler phoned the rector and received the assurance that this event would remain unique. But in the end of February 1934 a further inspection should be realized. This time the rector was absent and his deputy disregarded the agreement. With reference to the rector's assurance Köhler refused the inspection. So far the exceptional position of the institute was maintained and Köhler wrote to Perry of his successful policy. "I am trying to build up a special position for myself in which I might stay with honour. As yet it seems to work, but the end may come [any] day" (Henle 1978, 941). Soon, in April 1934, there was a new attempt, and this time it could not be prevented. Although neither suspect nor anything suspicious could be found - except for some journals from abroad and cigarette smoke in an unused room the leader of the inspection, the law student Hennig, submitted a spiteful report culminating in the proposal that Köhler's assistants Karl Duncker and Otto von Lauenstein and three further persons should be dismissed. On the following day Köhler protested in the sharpest terms and announced his resignation as the institute's director. This was not yet the end of Köhler's struggle, but the situation arrived at a crisis. When the rector denied his former promise Köhler did not only protest, but accused the rector of disloyalty and dishonesty (April 20th) which was a unique offence against the university customs. The conflict came to a climax with a further letter (May 8th) in which Köhler delivered an ultimatum for the rector's remembrance and regret. When, instead of an excuse, his most favoured disciple, Otto von Lauenstein, lost his assistency, Köhler found himself at a loss. He requested for retirement and announced Perry that his resignation would be final and the German psychology abolished for many years by this (May 21st). (Geuter 1984; Henle 1978; Sprung 1987)

May 1934 - September 1934: But the position of a leading scientist in Nazi Germany was not yet as weak as to be shakened by suspicion and disloyalty. There were strong influences in favor of Köhler and his institute. This support came from different sources. First there were those 1. international colleagues like the Danish psychologist, Edgar Rubin, who questioned the German secretary about rumours of a forthcoming resignation (May 1934). Then there were the students 2. who made efforts in order to strengthen Köhler's position. They planned a torchlight procession in favour of Köhler - which in the end was forbidden - and by his support managed to have talks with the ministry and its executive director Theodor Vahlen. Even in the Nazi administration there were 3. voices to be heard who warned of a provocation and prefered more careful practices. After he had received a further letter from Vahlen Köhler mentioned conditions under which he would be willing to withdraw his request: Otto von Lauenstein had to be reinstated, the institute employee who had denounced him dismissed, the head of the nazi students removed. Finally the ministry should 4. officially declare its trust in the institute's leadership (Ash 1995, 336). When Köhler set off for taking on a guest professorship at Harvard in September 1934, he could be sure that the ministry was going to handle the whole case in a conciliating manner. Indeed Lauenstein was reinstalled on January, 1st, 1935, the students' organization was publically criticized and the university was informed by a letter that Köhler had the confidence of the ministry. Yet in the same letter Köhler's offense against the university authorities was diasapproved. With Köhler in America and the nazi collaborators put in their place the institute seemed to be calm for the moment. (Ash 1985; 1995; Geuter 1984; Henle 1978)

October 1934 - February 1935: But the whole affair had developed in an irreversible way. In America Köhler received a request for signing a loyalty oath to Adolf Hitler giving him the opportunity to point to his still valid request for retirement. After the sudden appointment of the non-gestaltist psychologist Hans Keller to the vacant Lewin assistancy Köhler renewed his proposal. Once more he opposed to the ministry's practice of violating his autonomy as the institute's director. But even now Köhler hoped for an arrangement with the new authorities. This hope was finally disappointed in February 1935 when being acquainted with new dismissals of both of his assistants, Karl Duncker and Otto von Lauenstein, with effect of April 1935. On February, 28th, 1935 Köhler for the first time accepted a deal with the nazis. He accepted Keller and asked for the reinstatement of Duncker and Lauenstein, who should gain 2-year-appointments like his first assistant Hedwig von Restorff. But there was no further agreement, and Köhler once more had to reduce his claims. When the sommer term began before the case was decided according to Köhler's wishes, he pleaded for a continued employment of his assistants at least up to the end of the semester. Vahlen submitted to Köhler's request, although from the secretary's point of view the dismissal was already valid. But the nazi front became active once more and forced Vahlen to an immediate release. Vahlen on his part insisted on a payment until september. In May 1935 we find Köhler still postponing a final decision for a professorship at Swarthmore, but after the Nazis had banned Duncker and Lauenstein from the institute, Köhler could not do anything in favour of his assistant any longer. In July he emigrated to America, and only returned to Germany in order to care for his people. (Ash 1979; 1995; Henle 1978; Sprung 1987)

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So far the facts delivered by Henle, Ash and later studies on "Wolfgang Köhler and the National Socialism". The survey of the different contributions concerning this topic shows it to be rather well developed, compared with other chapters of the history of psychology in National Socialism. There is rich archives material almost completely presented in different papers. The synopsis of the material given here reveals that Köhler's struggle with the new dictatorial order had been a process of rather long duration. It shows that his resignation was the consequence of a complex structure of interpersonal and institutional relations. As has been stressed above, historical analysis does not end with an overlook of the material, but starts with it. Like Wehner points out, a good preparation of the historical material is not the end, but supplies the foundation for historical survey (Wehner 1991). We have to find the conditions which provided the ground for the events sketched in this short report. From this point of view it is not sufficient to reconstruct history as a compilation of events. But we have to learn about the historical framework as a whole.

It often has been set forth that the history of psychology is determined by a one-sided stress on the individual point of view. The history of psychology is traditionally concerned with great acts of great man (Lück 1991). I once pointed to the curious fact that psychologists behaving as historical amateurs adopt the naive psychology of historians instead of making use of their skills (Fitzek 1995). So a psychological point of view hardly can be found in the history of psychology. Nevertheless there is some pleading for a psychology of social interaction in history. There have been efforts recently to stress the social and institutional aspects of the history of science (Lück et al. 1987). When we succeed in finding out the framework of the historical events we will be less susceptible to attribute the whole process to different individuals and more prepared to understand history as a drama of social interaction with the different persons as protagonists of historical patterns.

As this paper is concerned with "Köhler and the National Socialism" the historical work should reconstruct the social system of university under nazi pressure. Therefore we have to imagine the long tradition of autonomy of science and teaching at German universities and the sudden change in university life under the nazi government. We have to regard this as a general fact and its effects on Berlin university in particular. We have to face the position of the psychological institute within the university as a whole and the position of its director in the faculty and the administration. And we have to trace the biographies of the different persons involved in the discussion concerning

coordination versus offense. So at last we have to learn about Köhler's personality, his experience and behavior, about his disciples and their opponents.

To find out the historical framework of "Köhler and the national Socialism" was a task of a history workshop carried through at Cologne university with a group of students (Fitzek 1998). From the diverse topics of this university workshop we can go on further in the direction Wehner has scheduled as a third possible phase of historical investigation (Wehner 1991). By this information we can develop a psychological perspective on the case of Köhler. The compilation of the historical conditions may lead to the question what psychological reasons shaped the process in which Köhler refered to the nazi provocation. Putting a question like this may show the behavioral patterns of Köhler's social detachment . It may reveal the reasons for Köhler's struggle - whether they derived from a tactical, moral or autonomous point of view. It may show how the different characters of university life could be absorbed by the uniform pattern of national socialism and what caused Köhler's special kind of opposition, making him such an outstanding, but rather lonesome figure in public life.

References

Ash, M.G. (1979): The Struggle Against the Nazis. *American Psychologist* 34, 363-365.

Ash, M.G. (1985): Ein Institut und eine Zeitschrift. Zur Geschichte des Berliner Psychologischen Instituts und der Zeitschrift "Psychologische Forschung" vor und nach 1933. In Graumann, C.F. (Hg.): *Psychologie im Nationalsozialismus* (113-137). Berlin: Springer.

Ash, M.G. (1995*): Gestalt Psychology in German Culture 1890-1967: Holism and the Quest for Objectivity*. Cambridge/Mass.: University Press.

Fitzek, H. (1995): Psychologie und Wissenschaftsgeschichte. In: Jäger, S., Staeuble, I., Sprung, L, Brauns, H.-P. (Hg.): *Psychologie im soziokulturellen Wandel - Kontinuitäten und Diskontinuitäten* (115 -124). Frankfurt: Peter Lang.

Fitzek, H. (1998): Geschichtswerkstatt. Bericht über ein psychologisches Praktikum "Wolfgang Köhler und der Nationalsozialismus". In: Jahnke, J., Fahrenberg, J., Stegie, R. & Bauer, E. (Hg.): *Geschichte der Psychologie -Beziehungen zu Philosophie und Grenzgebieten*. München: Profil.

Fitzek, H. & Salber,W. (1996): *Gestaltpsychologie. Geschichte und Praxis*. Darmstadt: Wissenschaftliche Buchgesellschaft.

Geuter, U. (1984): "Gleichschaltung" von oben? Universitätspolitische Strategien und Verhaltensweisen in der Psychologie während des Nationalsozialismus. *Psychologische Rundschau* 35, 198-213.

Henle, M. (1978): One Man Against the Nazis - Wolfgang Köhler. *American Psychologist* 33, 939-944.

Jaeger, S. (1992): Wolfgang Köhler in Berlin. In Sprung, L. & Schönpflug, W. (Hg.*): Zur Geschichte der Psychologie in Berlin* (161-183). Frankfurt: Lang.

Köhler, W. (1933): Gespräche in Deutschland. Deutsche Allgemeine Zeitung, 28.4.1933, 2. Wiederabdruck in: Graumann, C.F. (Hg.)(1985): *Psychologie im Nationalsozialismus*. Berlin: Springer.

Lück, H.E. (1991): *Geschichte der Psychologie*. Stuttgart: Kohlhammer.

Lück, H.E., Grünwald, H., Geuter, U., Miller, R. & Rechtien, W. (1987*): Sozialgeschichte der Psychologie*. Opladen: Leske und Budrich.

Ringer, F. (1969*): Die Gelehrten. (Orig.: The decline of the German mandarins 1890-1933.)* Stuttgart 1983.

Sprung, L. (1987): Wolfgang Köhler in den USA - Marginalien zu einem zweiten Leben. *Gestalt Theory* 9, 299-305.

Wehner, E.G. (1971): Ordnungsaspekte der Psychologie-Geschichtsschreibung. In Reinert, G. (Hg.): Bericht über den 27. *Kongreß der Deutschen Gesellschaft für Psychologie in Kiel 1970* (206-218). Göttingen: Hogrefe.

Josephine Fueser

Between Two Wars: Progressive Baltimore and the Culture of the New Psychology

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The first part of my title is shared with that of James Mark Baldwin's aptly titled Autobiography. The two wars in question are the AmericanCivil War and the first World War. These two struggles bracket, amongother things, the advent and waning of the Progressive era, and within it,the flourishing and institutionalization of American academic psychology. During this period, what constituted 'psychology', and indeed, what was 'American' was a matter of active debate.
For Baldwin, who published his autobiography in Paris in 1921 after a decade of exile, the idea of psychology promulgated by his successor, John Broadus Watson, was remote from the future he had envisioned for the discipline in numerous books, lectures and seminars.

Although their notions of the proper subject matter for psychology diverged considerably, Watson and Baldwin shared a common fate, both having been expelled from the Johns Hopkins psychology department for transgressing the bounds of propriety in a too-visible manner. Baldwin was forced to resign from his position as chair when journalists threatened to expose his brazen step across the colour line into a black brothel in 1908. In the winter of 1920, accounts of Watson's extramarital romance with his graduate assistant, Rosalie Rayner, were plastered over newspapers and tabloids during a highly publicized divorce trial. Watson, at the height of his influence, did not fathom the degree to which his 'private' life was jeopardizing his career, and was stunned when Goodnow apprised him of his impending dismissal.

As sensational and personal as they are, the experiences of James Mark Baldwin and John Watson also reveal something of a larger plot - that of the growth of a social science within the social and political reality of progressive America. Their stories reflect changes happening in several American psychology departments - in the demographic composition of the faculty, in the understanding of what psychology was and what it was to be used for. I'd like to re-tell a portion of Baldwin's and Watson's stories in a way that provides a context for the seemingly abrupt change in the way they viewed psychology. Some of the striking differences in Baldwin's and Watson's psychologies, I believe, become more intelligible - and more interesting - when one takes into account the vantage points they occupied.

By accident of birth, both Watson and Baldwin were born white and South Carolinian. The fact that they were born twelve years and about one hundred miles apart had, in this particular time and place, significant consequences. Baldwin was born during the Civil War in Columbia, which had been a thriving capital city when his Connecticut parents migrated there ayear before his birth. His father was a merchant and a statesman. In a community that paid close attention to breeding, the fact that the Baldwin family could supply a pedigree that dated back to the Fifth Earl of Flanders was attended to and recorded. A South Carolinian encomium written for Baldwin's seventieth birthday in 1931 recites this genealogy and draws our attention to the fact that Baldwin's father bought large numbers of slaves during the decade he lived in South Carolina before the outbreak of war. In his autobiography, Baldwin assures us he did this with the intent of buying their freedom. Whether because of their abolitionist sentiments, their Northern roots or simply their proximity to the fighting, the family relocated to Connecticut shortly after Baldwin's birth. They returned to Columbia as soon as they were able.

Although the Civil War had ended, the climate into which Baldwin's family returned was explosive. The governing of the rebelling states was temporarily taken over by Northern bureaucrats, who were reviled as opportunists by many displaced white southerners. Although Baldwin's father was part of the reconstruction administration, the family made a smooth transition back into its adoptive city, seemingly due to their 'sympathy' with the dispossessed planters of Columbia. The young Baldwin seems to have likewise taken the lost cause to heart. In his autobiography, he writes, "All this social agitation [the social and economic support provided by Freedman's Bureau; the establishment of schools for black Southerners] was as misplaced as the political measure of black suffrage was mistaken, and the unfortunate south suffered the political

infamy, economic paralysis and social hell which were its lot up to 1880." At Princeton years later he chose as the subject of his first oration "The Failure of the Fourteenth Amendment."

Thus Baldwin shared the view of many of his compatriots that the attempt to induce large-scale social change in the Southern states was not only undemocratic but in some sense, unnatural. Indeed, South Carolinians expressed their resistance to the emancipation of black southerners with particular hostility. Protracted military occupation was necessary to back up their newly-declared basic rights - to vote, to hold office, to own property, to earn wages, to enter into voluntary contracts, to bear arms and to get married. Whites who saw these measures as illegitimate uses of state power organized vigilante groups such as the KKK, which sought to unite the interests of white southerners across class and ethnic lines against their common northern and black enemies. By the 1870s, federally-backed resistance to the steady tide of terrorism and lynching began to crumble, and white economic and political supremacy was effectively reinstated throughout most regions of the south.

By the time John Watson was born in 1878 what Baldwin called the 'social hell' of reconstruction was already fading: the last federal troops had withdrawn the year before. But even before the outbreak of the civil war, the backwoods Piedmont region of South Carolina, where Watson lived until he was twenty-one, had constituted a different world from wealthy urban Columbia. It was inhabited largely by poorer independent farmers, who bred a strain of white supremacy distinct from the slaveholding plantation culture in tidewater regions. Watson's family had been landholders in this region for several generations, but like many others across the country participated in the drift from rural communities to the city in the 1890s. Greenville provided a source of inexpensive labour for burgeoning cotton mills; the neighbouring farms, like those across the country, were struggling with depression. The local economic reality was at odds with the prevailing racial mythology in Watson's community. For the farmers and laborers, who would share an economic niche with the freedmen, the equality of black southerners was real and imaginable in a way that it was not for planters 100 miles away. This proximity made the patrolling of social boundaries all the more urgent. Watson in adolescence partook of these activities with seeming enthusiasm.
In his otherwise laconic and impersonal autobiography, one of the few anecdotes he recounts from his youth is that he pursued one of his favourite activities, which he called "'nigger' fighting," with such vigour that he was arrested for it. As his biographer points out, this was no mean feat in the era of the Ku Klux Klan.

Both Baldwin and Watson attended private high schools; public education across the south would remain scanty for some time. Baldwin furnishes us with a telling statement describing his early training, which was administered by a Confederate colonel. Columbia, he writes, "was well provided with private schools, conducted by members of the most cultivated families - reduced materially by the war but living still in the spirit of the old regime". Again, Watson encountered different circumstances a decade later in Greenville. He enrolled at age 16 in the 'fitting school' of Furman University, an institution that had to cater to the many needs of the community. It defined itself as a "manual labour, Classical and English school" that also provided theological instruction. However, by the mid-90s, a spirit of educational reform and the impetus towards efficiency had penetrated the small southern Baptist college. A Chemistry lab had recently been established, where Watson worked to pay his tuition; and George Moore, the philosophy instructor, began to teach his students psychology. Among the texts Moore taught was Baldwin's recently-published Handbook of Psychology, which was swimming with terms such as 'feeling' and 'will' that Watson would later try to eradicate from the discipline's vocabulary. The education Baldwin had experienced aimed to transmit the values of a culture and to create a well-rounded gentlemen. Furman, on the other hand, was concerned with practical training and envisioned its pupils as specialized professionals.

These qualities fit very well with the mandate of the University of Chicago, where Watson attended graduate school. After a disappointing year as teacher in a one-room schoolhouse, Watson planned an escape. Not knowing precisely his direction, he was convinced his route was through higher education. In a letter to the President of the University of Chicago, William Rainey Harper, he characterized himself as an 'earnest student', and told the president that it was his desire to "do advanced work in a real university". The image of the poor, hardworking, honest man, one that convinced Harper to accept Watson with a tuition waiver, would later appeal to James Mark Baldwin as he built the psychology program of Johns Hopkins university.

Watson's route through academia was a laborious one. He worked at a frenetic pace, holding down several paying jobs and spending his remaining time at the laboratory; the year before he obtained his coveted degree, he found himself suddenly in the condition of the 19th century intellectual - a neurasthenic one. Suffering angst and a nervous collapse, he suffered a protracted bed-rest before finishing his program and taking a job as an instructor at Chicago.

These difficulties aside, Watson writes that he felt immediately at home in the university. The faculty was unparalleled - he was mentored by John Dewey, James Rowland Angell, Donaldson and Jacques Loeb. He, and perhaps he alone, managed the collosal feat of ignoring John Dewey. Angell he revered; Loeb and Donaldson he emulated. The functionalist philosophies at Chicago opened a space for Watson's work in comparative psychology; at any department at which psychology was tied to the introspective study of faculties he would have probably become a zoologist and, as Watson quipped in 1913, we would in two hundred years still be debating whether auditory sensations have the quality of extension.

By the time Watson sought a more permanent position as an instructor of Psychology, however, his reputation as a labourer outweighed any trepidation regarding the fact that most of his work was with animals. Angell writes in a letter to Baldwin, "That he has largely gone into animal experimentation is essentially accidental. He is interested in a broad reach of subjects and quite competent in all the more basal aspects of psychology." Watson is portrayed in these letters, above all, as a resource: "He is indefatigable in work and resourcefulness... I shall never let him go, if I can help it, but I am too fond of him to let my selfish interests outweigh any opportunity to get him proper advancement." Issues of class are articulated with some frankness: "I think he would regard seriously any offer which paid him a living wage with the recognition of assured academic position. Hard as is the financial struggle for all our younger men here ... and it is very severe ... I think he at least feels quite as strongly about the lack of academic recognition in rank." On the top of a sheet containing Baldwin's notes on Watson's scientific merit is a single sentence: "He is still well under forty years of age and a full-blooded Southerner."

The trustees of Johns Hopkins did not set out to hire a radical behaviourist; indeed, Watson writes that he did not begin to formulate the ideology articulated in 'Psychology as the Behaviorist Views It' until 1912, two years after the leadership of the department suddenly fell in his lap. The correspondence between Watson, President suggest they simply hired an assistant for Baldwin; yet Watson's presence itself an indication that the social order to which Baldwin belonged and out of which he wrote was disappearing from view within as well as outside of academia.

In what way is this change reflected in their thought? Baldwin's writing comes out of a sense of confidence about what the world was. The path taken by civilization through history was one that he felt he understood deeply; it was a theme he repeatedly revisited. In a two-volume history of psychology he traces the progressive understanding of mind from Ancient Greece to its future in 20th-century America. His crowning meditation on orderly change, Genetic Theory of Reality, is his first attempt at sewing non-European cultures into a narrative of cultural evolution. In his writings on individual development, Baldwin attempted to demonstrate the way in which the history of the species was mirrored in the growth of the child. Individual growth was always already in harmony with the goals of social change. Individual volition, by the same token, did not swing free of community: "In all his learning by his agency, he learns above all the great lesson essential to the development of his thought of self: that there is a something always present, an atmosphere, a circle of common interests, a family propriety, a mass of accepted tradition". An assurance of order is also present in mode in which his psychology was written. That is to say, the task he set for himself, the question his psychology was meant to satisfy, was not so much the discovery of unknown laws of human behaviour, nor their manipulation towards a given end, but simply to explain or lay out a process that was already well-known in outline. The sense that the state of the future could be discerned in the motion of the present is evident in all his pre- World War I writings.

Although Baldwin's understanding of development was more inclusive and compassionate than that of many of his contemporaries, he shares with them an oversight of optimism, which fails to recognize that his vision of social progress is essentially a product of perspective. Two years before the founding of the American Psychological Association, Mississippi enacted the first of the oppressive Jim Crow laws. Farmers formed an alliance in the same year to combat widespread poverty. Outside the walls of the Johns Hopkins University, a predominantly working-class

community fought poor working conditions and social services. Baldwin's psychology told the story of an elite; it produced a social theory concerned with the production of a new elite. Many critics have remarked upon the fact that Baldwin develops no theory of development within a family context; this is in part because figure of nurse wanders through the pages of his developmental trilogy.

The social world that Baldwin portrayed so attentively, its elaborate hierarchies, its nuanced conventions, and subtleties of convention, is strikingly absent in Watson's psychology. From outset, Watson portrayed himself as a pragmatic, realistic man. One of the many ironies surrounding his life is that both his environment and the subjects within it become unrecognizably abstract. Baldwin's psychology, though partial, paid scrupulous attention to his own lived experience, documenting at once the formation of habits, desires, sensations, emotions, knowledge, relationships.

In Watson's move to abandon introspective methods, to render individual differences moot, and cultural differences inconsequential, it is hard to ignore an appeal to democratic sentiments. In 1913 he complained, "Psychology, as it is generally thought of, has something esoteric in its methods. If you fail to reproduce my findings, it is not due to some fault in your apparatus or in the control of your stimulus, but is due to the fact that your introspection is untrained." Watson's psychology was everyone's psychology in so far as it was the psychology of anyone. Its overt message was that no differences really mattered, and that anything was possible. This message reached its apotheosis in an oft-quoted passage in Behaviourism:

> "Give me a dozen healthy infants, well formed, and my own specified world to bring them up in, and I'll guarantee to take any one at random and train him to become any type of specialist I might select - doctor, lawyer, artist, merchant-chief and yes, even beggar-man thief, regardless of his talents, penchants, tendencies, abilities, vocations, and race of his ancestors".

This statement seems surprising from someone arrested for violence against African Americans in Greensville, NC. Yet its ethos of social control, while new to academic psychology, had a precedent in the campaign waged by white Southerners during reconstruction. In the case of the new psychology, too, the disappearance of (a race-class) order also calls for an increase in vigilance; he who controlled the environment controlled the emerging social order to an unprecedented degree. A look at his autobiography written a decade later suggests that his developmental wager had more the quality of a fantasy, a conjecture about racial difference whose truth was beguilingly uncertain. He wrote,"I sometimes think I regret that I could not have a group of infant farms where I could have brought up thirty pure-blooded Negroes on one, thirty "pure" – blooded Anglo-Saxons on another, and thirty Chinese on a third - all under similar conditions. Some day it will be done, but by a younger man". What this "study" would do, in effect, was repeat what Lincoln had called the 'experiment' of reconstruction, but under more controlled circumstances, and with more conditions.

Watson attracted the attention of first Angell and Baldwin, then an academic readership, as a plain-speaking practical man. Not only did he profess an accessible psychology; he also cast a wider net of influence in his career as a psychologist and later as an ad man. Early in his career at Johns Hopkins, he wrote for popular audiences as a supplement to his income; his interest in the application of behaviourist principles soon produced his popular manual for infant care. The larger story of psychology's path to what Haskell called 'cultural dominance' requires us to extend our timeline into 1920s. At the turn of the century the new psychology, for all its potential, still fought for social and scientific reputability. Following Baldwin's removal from Johns Hopkins, he effectively slipped from the discipline's collective memory until Piaget reintroduced him at mid-century. By the time Watson's personal life collided with his academic career, his psychology had assumed a soundness of its own.

References

James Mark Baldwin Collection. Boxes I-II. Rare Books and Special Collections Department, Princeton University Library, Princeton, NJ.

Baldwin, J.M. (1895). *Mental Development in the Child and in the Race: Methods and Processes*. New York: Augustus M. Kelly.

Baldwin, J.M. (1897). *Social and Ethical Interpretations in Mental Development: A Study in Social Psychology*. New York: Macmillian, 2nd ed.

Baldwin, J.M. (1904). The history of psychology. International Congress of Arts and Sciences, Department XV - Psychology, pp. 605-622.

Baldwin, J.M. (1915). *Genetic Theory of Reality, being the Outcome of Genetic Logic as Issuing in the Aesthetic Theory of Reality called Pancalism*. New York and London : G.P. Putnam's Sons

Baldwin, J.M. (1926). *Between Two Wars (1861-1921), being Memories, Opinions and Letters Received*. (Vols. I-II). Boston, MA: The Stratford Company.

Buckley, Kerry W. (1989). *Mechanical Man: John Broadus Watson and the Beginnings of Behaviorism*. New York: Guilford Press.

Office of the President, Record 02, Box 86 (1904-1910). Archives of Johns Hopkins University Library, Baltimore, MD.

Pauly, Philip J. (1979). "Psychology at Hopkins: Its Rise and Fall and Rise and Fall and ..., " *Johns Hopkins Magazine* XXX: 36.

Watson, John Broadus. (1914). *Behavior. An Introduction to Comparative Psychology*. New York: Henry Holt.

Watson, John Broadus. (1936). "John Broadus Watson," in C. Murchison, (Ed.). *A History of Psychology in Autobiography*, Volume III. New York: Russell and Russell.

Robert Mearns Yerkes Papers. John Broadus Watson Correspondence, Series I,
Box Manuscripts and Archives, Yale University Library, New Haven, CT.

generation x class culture

Steve Fuller

The Future is Never Definitively Fixed Except When it Has No Past: Making the Most of the Gulbenkian Report - *Open the Social Sciences*

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Summary

Raymond Aron famously said, 'The past is never definitively fixed except when it has no future'. As my title suggests, I want to turn Aron's saying on its head and use it as a basis for considering the crucial role that historians of the human sciences can play in fulfilling the ambitions of the 1996 Gulbenkian Commission report, *Open the Social Sciences*. The report itself is mainly a well-informed and sophisticated account of how many of the social sciences' current incapacities are traceable to the continuing influence of the founding conditions of these fields in 18th and 19th century Europe. Yet, this general feature of the report has gone largely neglected in its reception. Instead, commentators have focussed on the need for the social sciences to incorporate such 'postmodern' sciences as chaos and complexity theory, as if the only function that history serves in *Open the Social Sciences* is one of clearing the way of rubbish (much as John Locke described his own critical philosophical work as 'underlabouring' vis-a-vis Newtonian science). I believe that not only does this misunderstand the spirit of the report, but more importantly does an injustice to the institutionally important role that historians have played -- and continue to play -- in keeping alive alternative futures for the human sciences. Thomas Kuhn was indeed correct to observe that this idea of a living past is one of the most fundamental differences between the human and natural sciences. Unfortunately, Kuhn held this to be to the detriment of the human sciences, since it prevented them from ever making a clean break from their past. I shall argue that, on the contrary, this is a strength, and that many of the today's cultural crises in the natural sciences are symptomatic of their own historical amnesia. However, historians of the human sciences have nothing to be smug about. The challenge here is to keep the teaching of the history of the human sciences firmly within the human sciences, often in the face of colleagues who mistake historical amnesia for intellectual focus.

James Good* and Dave Rhodes**

A Pictorial History of the Life and Work of William Stephenson: Founder of Q-Methodology and Student of Human Subjectivity

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Summary

William Stephenson (1902-1989 is most widely known as the founder of Q-methodology. For most of his professional life, however, he was engaged in a quest to develop a science of subjectivity. From his famous (1935) letter to *Nature* about factor analysis to his death in 1989, his work was frequently a reaction against then current ideas in psychology. Born in 1902 in Chopwell, County Durham, England, Stephenson initially trained in physics (PhD Physics, Durham, 1927). Stephenson left Durham for University College, London where he received his PhD in Psychology in 1929. He was Charles Spearman's assistant prior to the latter's retirement. In the 1930's Stephenson was a central figure in debates about factor analysis with Charles Spearman and Cyril Burt. War service interrupted his career which was resumed at the University of Oxford where he was Reader in Experimental Psychology and Director of the Institute of Experimental Psychology. Stephenson's interests were broad and he underwent psychoanalysis with Melanie Klein. In 1948 he emigrated to the United States moving first to a Professorship at the Department of Psychology, University of Chicago. In 1958 he took up a position at the School of Journalism, University of Missouri-Columbia. Q-methodology was the fruit of his attempt to lay the foundations for a science of subjectivity. Despite numerous institutional developments promoting the interests of Q-methodology - a journal (*Operant Subjectivity*), an international society (*International Society for the Scientific Study of Subjectivity*), an annual conference, a Web Page, and a e-mail list, Stephenson remains a marginal figure in the history of psychology in both the UK and the USA. This poster illustrates the major formative influences in his life and the principal settings for his work. It also attempts to identify the significance of his quest to develop a science of subjectivity.

Jean-Marie Vianney Higiro* and William R. Woodward**

The Commodification of Genocide in Rwanda Since 1994

Western New England College* and University of New Hampshire**

Beginning on October 1, 1990 the Rwandan Patriotic Front (called RPF) launched a war against Rwanda from its base in Uganda. An agreement was signed in Arusha, Tanzania in August 1993 to bring an end to the war. On April 6, 1994, the plane carrying the president of Rwanda and his Burundi counterpart was shot down as it was about to land near Kigali International Airport. The war which had stopped following the cease fire resumed. The United Nations dispatched an investigator who concluded that it was a genocide. The United Nations then put in place a tribunal to try the perpetrators of the genocide and crime against humanity. As defined by the Geneva conventions:

> Genocide is one of the following acts committed with the intention to destroy in whole or in part a national group, a national ethnic, racial, or religious group as such: the murder of the members of the group, serious attacks on the physical or mental integrity of the members of the group, intentional submission of the group to conditions that might lead to the total or partial physical destruction, measures leading to preventing births within the group, the forced transfer of children of thegroup to another group (Dupaquier et al, 1996).

The international tribunal has under custody twenty three suspects who are being tried. In Rwanda the government has arrested more than 130,000 suspects. Last April it carried out public executions of thirty individuals convicted of genocide (Anon, CNN, April 22, 1998; Dufka, Reuters, April 24, 1998).

The current regime in Rwanda comprises Tutsi refugees who were living outside the country until 1994. They are trying to impose their ideology, as any regime would. We learn from Antonio Gramsci that one way of understanding power relations in society is to look at hegemonic relations and how they are articulated. Gramsci says that the dominant social group or class seeks consent from the subordinate class in order to rule. When the dominated group or social class resists the domination of the dominant groups, they try to negotiate. They only resort to using force when the dominated groups or classes threaten the existing social order. One of the sites where the dominant ideology is articulated is the media (Gramsci, 1971). In the case of Rwanda, a group of Rwandan refugees came from outside and won a civil war. They are trying to impose their ideology on the population as would any regimme (Karemano, 1996). We thin that we need to find out how this ideology is articulated (Abrahamsen, 1997).

According to Foucault, ideology is going to be articulated in adiscourse of statements that exclude or prevent articulating other pointsof view. So for example, if we use the term "terrorist" or "genocider," we exclude certain questions. We assume you know he or she is a criminal. We are not going to question the political and social context of events.

It seems that the current government is using genocide to legitimize its grip on power. Genocide has been commodified. By commodification we mean the use of genocide as a crime to justify or legitimate behaviors. These behaviors can be violations of human rights or they can be intended to oppress individuals or groups of people. This paper will focus on how the commodification of genocide is articulated in official discourse. We shall address the consequences of the commodification of genocide in relation to the forging of a Rwandan cultural identity and peace in Rwanda.

Methods

We used a qualitative content analysis of some news reports about events occurring in Rwanda and the great Lakes in general (Uganda, Burundi, Rwanda, Tanzania, Congo, and Kenya). International media have to rely on news reports disseminated by international new sources. Very often those

international media quote official sources and also those oficial sources stage events in order to rally national and international opinion around their world view (Myers, Klack, & Kohl, 1996). Thus we look primarily at the wire services -- Agence France Press, Associated Press, CNN, Reuters, and the Voice of America. We identified the single major theme or issue from each of over a hundred stories. Over the period November 1997 through May 1998 ten themes emerged, of which seven are briefly covered here: ethnicity, the reconstruction of Rwandan history, human rights, statistics, democracy, the memorializing of the victims, and peace. In each case, we compared information provided by the stories and information not provided by the stories but which we know from other sources.

Genocide and ethnic identity

Most news reports coming out of Rwanda emphasize the old rivalries between Hutus and Tutsi (Worsnip, Reuters, Dec. 7, 1997). The binary analysis underlying such reports failed to mention the opposite categories -- which would be Tutsi moderates, Tutsi extremes, Tutsi militants, Tutsi armed militants. At the same time, we do not know in which category individuals who are neither moderate nor extremist are; for example, the quiet peasants, Tutsi, Hutu, and Twa, women and children, and elderly fleeing for their lives.

The group of Tutsi in power today is English-speaking and from Uganda (Prunier, 1995; Rosenblum, 1998). They have control over the army and the government. Somehow Hutu who were the target of the previous regime or whose relatives are not considered genocide survivors. Yet many Hutus had relatives killed because they belonged to political parties opposed to the Habyarimana regime. Or because they were tall and looked like Tutsis according to the sterotype developed along racial lines. The senior author himself is a genocide survivor, but because he is not a Tutsi, he would not qualify for this label.

Genocide and the reconstruction of Rwandan history

In general, the social science literature subscribes to the view of the party in power in ascribing the problems of Rwanda to "Hutu genocide" (Braeckman, 1994; Chr tien, 1995). A new Rwandan journal in the U.S. also takes an apologist view for the RPF regime (Ntaganzwa-Rugamba, 1996; Kimenyi, 1997). This bias even exists in African Rights (1995), a collection of eye-witness interviews edited by a Somali woman. In fact, we think that a corrective to this bias would entail understanding the origins of the ethnic constructions used in the current genocide discourse.

Before colonialism, Hutu and Tutsi were ethnic but also socioeconomic categories relevant to central Rwanda (Prunier, 1995) and Burundi (Laely, 1997). In terms of ethnicity Tutsi and Hutu designate two groups of people. Tutsi were the group where Rwandan rulers came from, the aristocracy. The Tutsi were cattle-raisers. The Hutu were agriculturalists. However, when a Tutsi became poor, he became a Hutu. When a Hutu became rich, he became a Tutsi. Wealth meant having cattle. There were intermarriages and some children were fathered by Tutsi chiefs to Hutu women. With colonization, the social mobility which existed at that time was no longer possible. The Germans and then Belgians first created schools for the sons of chiefs so that they could be chiefs and replace their fathers. Second, they introduced ethnic identification Ids, and as a result socio-ethnic categories became fixed. After three decades of supporting the Tutsi monarchy, Belgium switched sides from Tutsi to Hutu around 1959 and continued to support the Hutu regimes. Rwanda's present government blames France and Belgium for supporting the Hutu genocide in 1994 and, operating from eastern Zaire, continuing attacks on Rwanda during 1994 to 1996 (Anon, Reuters, Dec. 15, 1997). Let us look at the actual human rights situation.

Genocide and human rights

The RPF launched its attack on October 1, 1990. Its killings of peasants before it took over in July 1994, and after then, has never been seriously documented. The British journalist Nick Gordon of Sunday Express (April 21, 1996) and the French journalist Steven Smith of Liberation (Anon, IFEX, Dec. 2, 1997) have written long articles documenting the massacres committed by the RPF between April 6, 1994 and July 1994. Smith is a persona non grata in Rwanda today. These are the only reporters to have documented the killings of the RPF during its military conquest of power. In April 1994, the RPF murdered people selectively in Amahoro stadium. It went from house to house in the

section of the capital, Kigali, called Remera 2 and 3. Individuals who had fled to the Amahoro stadium were subsequently evacuated to Byumba; some of the individuals evacuated to Byumba were summarily executed.

Genocide and statistics

Most news reports state that during the genocide there were between 500,000 and 800,000 people who died. Sometimes the figures one million is used. They died in the hands of Interahamwe and government forces. The 500,000 was an estimate of the CIA. Before April 1994, the CIA said if the war is resumed, at least half a million people will die. No one knows how those estimates, which are repeated in the media, were arrived at. What is sure is that those estimates do not include the killings done by the RPF because the RPF is supposed to be a victim of the genocide. It is an innocent. In other words, we do not know the numbers of people who died in the territory controlled by the RPF. And we do not know the number they killed after they took power. Making a census to find out exactly the current population in order to compare with the prewar population is not a priority with the current government. However, when there is an anniversary of the genocide, statistics are used and one is told that in this area there were so many Tutsis and there are so many survivors. The statistics are reinforcing the positive image of the RPF and they call for empathy. They are exaggerated.

Genocide and democracy

When the Rwandan Patriotic Front attacked Rwanda, one of its grievances was returning home and the other was democracy. In the period 1991 to April 1994 there were political parties which were recognized and allowed to operate. Because of the genocide, political parties were suspended.

Political parties could not hold political rallies or political conventions. Only the RPF could. Even opposition parties which supported the RPF were not allowed to hold political activities. The Arusha Peace Agreement allowed powersharing among political parties and opposition parties and Habyarimana's party (Clapham, 1998). After the "genocide," Habyarimana's party MRND had disqualified itself because of the actions. The RPF took the lion's share of seats at the Transitional National Assembly and the cabinet positions. There were no elections. The Arusha Peace Agreement had a provision about the integration of the army. Because of the "genocide," there was no integration due to the fact that former government forces had "disqualified themselves" by taking part in the "genocide." The RPF had coopted some officers and some soldiers, but the army remains dominated by Tutsi.

Genocide and the memorializing of the victims

Because the RPF "stopped the genocide" or "ended the genocide", there are no sites to indicate where the RPF massacred Hutus (Gordon, April 21, 1996; Smith, Liberation, date?). The victims of the genocide are Tutsi. Hutus who were killed by the RPF army, Interahamwe, and former government soldiers are not worth mourning (Goujon, AFP, April 7, 1998). Twa who were killed are never mentioned. Every week of remembering the victims of genocide is a reminder of the guilt of Hutus (Cahn, VOA, April 7, 1998). It is not an opportunity to meditate on why this horrific crime took place and under what conditions and how to avoid it happening again. It is like saying we are the victims, we are threatened, our enemies are still here.

There are groups in Rwanda and abroad which were formed after the genocide to remember the victims. They operate like propaganda channels of the current regime. They stage events to impress upon visitors, foreign dignitaries visiting Rwanda, and the general population that Hutu are guilty and that justice, which to them is revenge, must be rendered. People write documents and accusations against innocent individuals who are opposed to the current regime.

Forging aRwandan cultural identity and peace in Rwanda

The West, particularly Great Britain and the United States, support this dictatorial regime, partly because of this genocide discourse, partly because the alternative has no space in which to articulate its views, and partly because U. S. interests are best served by a stable military regime in Rwanda. The genocide discourse has stifled the democratic process which had momentum before 1994 (Clapham, 1998) but ended then.

The road to peace in Rwanda is justice, and in order to have justice, there has to be accountability by all Rwandans who have leadership positions. We are not saying that there was a power that need as much investigation as the genocide committed by some Hutu leaders. Only when justice is rendered with fairness will people believe in it.

International media need to play a role in explaining these complex social issues. Up to now they have tended to present them in ethnic and racial terms using stereotypes. By doing so, they are playing the game of Hutus and Tutsi supremacists who only see the solution of power struggle in Rwanda through wars and violent means.

Gustav Jahoda

Piaget Inspired, but also Misled, by Lévy-Bruhl

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Summary

The similarity of 'primitives' and (European) children was the conventional wisdom of the 19th century, continuing well into the 20th. Lévy-Bruhl, while claiming that the thought processes of 'primitives' are fundamentally different from those of the 'civilised', did not subscribe to that view. However, many psychologists of the inter-war years, including Piaget, linked the two ideas. Piaget had come across Lévy-Bruhl as a young man, and became convinced of the parallel between primitive' and child thought, which led him to adopt some of Lévy-Bruhl's concepts such as 'participation'. In a key paper of 1928 he tried to explain the alleged parallelism, and all the books of his 'pre-operational' period are pervaded by Lévy-Bruhlian ideas. While this influence became less visible in his later years, evidence will be presented showing that he continued to support, perhaps to the end, Lévy-Bruhl's original stance.

Jeroen Jansz

Psychologisation and Psychology: The Case of Psychologising Migration in the Netherlands 1945-1965

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Summary

This paper addresses the psychologization of modern Western culture. The historical process of psychologising is described here as the growing emphasis on the individual's "inner" world of thoughts, feelings, and wishes. Psychologising proliferated widely after the Second World War, although its roots date back to earlier periods.

The question I want to raise in this paper is whether the construction of Psychology as a discipline and profession was a necessary condition for the appearance of twentieth century psychologization. My answer will be in the negative first: the self-reflexive language is older than Psychology, and it sprouted from other sources. But then I will follow Rose and Danziger in arguing that Psychology paid a specific contribution to the process of psychologising by reshaping the vernacular. Psychology provided both psychologists, non-psychological experts, and lay people with a terminology to conceptualise the "inner" world of human individuals, and it offered instruments to map this "inner" reality.

In particular, I will focus on 'the language of psy' that was used by 'psy experts' to analyse the experiences of Dutch migrants after the Second World War. The analysis of the migrants' experiences in terms like attitude, motive, and personality served the political role of translating the societal phenomenon of (forced) migration into an individual problem of adjustment.

Introduction

Nowadays, it is self-evident for most people in Western culture to refer to the so called inner world of thoughts, feelings, and wishes when they try to understand themselves and others. It is not uncommon, for example, to attribute explanatory value to the processes that allegedly unfold 'inside' the individual: most people are after all inclined to accept a particular wish or a feeling as a suitable explanation for an opaque kind of action. The reference to subjective thoughts, feelings, and wishes is also prominent in contemporary self-presentation. Human interest journalism, television talk shows, and soap operas provide a rich variety of examples of this. The Dutch psychologist Zeegers (1988) documented the historical changes in systematic self-presentation in lonely hearts advertisements. His large scale study of advertisements in the post war period showed a gradual increase of a terminology that referred to a subjective inner reality, and a decrease of self-presentational terms that referred to social and demographic status.

In this paper I look from a historical perspective at the reference to the so called inner reality of the individual. I will employ the term psychologization to refer to the growing emphasis on the individual's "inner" world of thoughts, feelings, and wishes that is discernable in Western culture (see also Elias, 1939; Rose, 1990). The question I want to raise in this paper is whether the construction of Psychology as a discipline and profession was a necessary condition for the appearance of twentieth century psychologization. My answer will be in the negative first: the self-reflexive language is older than Psychology, and it sprouted from other sources. But then I will follow Rose and Danziger in arguing that Psychology paid a specific contribution to the process of psychologising by reshaping the vernacular. Psychology provided both psychologists, non-psychological experts, and lay people with a terminology to conceptualise the "inner" world of human individuals, and it offered instruments to map this "inner" reality.

I will develop my argument by first sketching the construction and expansion of Psychology as a discipline and a profession, then I will discuss briefly the long history of psychologisation. In the last part of my paper, I will discuss a historical case in which the language of psy was used to describe the experiences of people that migrated to and from the Netherlands in the post-war period.

The proliferation of psychology

Let me briefly note a couple of developments in the history of psychology in order to set the agenda regarding psychologising. First, it is of utmost importance to note that psychology was a *practical science* from its inception on (Van Strien, 1993): though founding father Wundt focussed mainly on experimental research in the laboratory, pioneers like Ebbinghaus, and Galton were involved in solving practical problems, and so was Freud. Cattell paid an important contribution to the professionalisation of psychology by creating a specific domain of expertise: thanks to Cattell's mental test, psychologists could become the experts of the measurement of mind.

In the first decades of this century professional psychology expanded gradually which resulted in a well established practice of professional interventions in the inter-war period. The US was the first country to see professionalisation on a large scale, countries like Germany, Britain and the Netherlands followed later (Napoli, 1981; Rose, 1990; Van Strien, 1993). In retrospect, we can discern the importance of these so called practical applications for the development of psychology as an academic, experimental discipline. Danziger (1990) has shown that the interactions between profession and academy were conditional for the establishment of psychology as an acknowledged discipline. Danziger's research also shows that the term 'applied psychology' really is a misnomer, because it suggests a one way relation from the laboratory to the practical intervention, and thus neglects the reciprocal relations between so called fundamental and so called applied work in psychology.

Parallel with the institutionalisation of Psychology, experts outside the field of psychology employed the language of psy to analyse the practical, social problems they had to face. The American Mental Hygiene Movement, which spread to Europe in the 1930s, is an important example of this development. Physicians, social workers, ex-patients, and psychiatrists in this Movement used a psychological terminology to describe and analyse what they found in their patients, and they employed psychological theories, like psychoanalysis, to explain the maladjustment of their patients. As a results of this, and related efforts, there 'emerged an ever growing army of experts whose pronouncements on matters psychological were of a special kind, because they were supposedly founded on the indisputable authority of science' (Danziger, 1997, p. 182).

The expansion of Psychology and related human sciences resulted during and after the Second World War in what Rose has called 'the empire of the psy' (1997, p. 232). Professional disputes between and within each branch of the empire were common, but Psychology gained its victory. This is underlined by Herman's conclusion about American psychology: 'At the dawn of the 1970s, psychological experts had reason to feel satisfied with what they had accomplished since World War II. They had become players in far-flung areas of public policy and public culture, bringing their theories and research to bear on the major issues of their day' (Herman, 1995, p. 304). In other countries the empire of the psy may not be as large as the American one, but the conclusion is warranted that Psychology as a profession and a discipline proliferated widely in the second half of the century, and so did the language of psy in the hands of psychologists and other psy experts (Danziger, 1997; Jansz & Van Drunen, 1996; Rose, 1990, 1997).

What is the cultural effect of Psychology's proliferation?

Most commentators who write about the effects of psychology on the culture at large focus on the expansion of psychotherapeutic endeavours in the postwar period. Both Sennett (1977) and Lasch (1979) consider the psychotherapeutic practices, and their popularization as pivotal in the transference to an intimate or narcissistic culture. In addition, the philosopher MacIntyre notes that 'in our culture the concept of the therapeutic has been given application far beyond the sphere of psychological medicine in which it obviously has its legitimate place (...); the idioms of therapy have invaded all too successfully such spheres as those of education and of religion' (1984, p. 30-31). The sociologist Bellah and his team underline this view when they argue that twentieth century American culture is a culture of psychotherapy (Bellah, et al., 1986).

Recently, both Herman (1995) and Rose (1996; 1997) provided us with more focussed evaluations of the relation between psychology and culture. Herman is quite explicit about the cultural effect of psychological expertise when she claims that 'the progress of psychology has changed American society. Americans today are likely to measure personal and civic experience

according to a calculus of mental and emotional health - "self-esteem" in the current vernacular' (Herman, 1995, p. 1). Rose is more careful: first, he says that psychology and related disciplines had a shaping effect on the vernacular: 'the relation (his italics) to ourselves which we can have today has been profoundly shaped by the rise of the psy disciplines, their languages, types of explanation and judgement, their techniques and their expertise' (Rose, 1997, p. 226). But then he warns the reader against a causal interpretation of his statement, by explicitly denying the suggestion that 'the activities of the psy professions are themselves the "cause" of all the mutations involved in the birth of the psychological self' (Rose, 1997, p. 245).

At this point in my argument, I will take the changes and reshaping Herman and Rose observed for granted. In what follows I will focus on the language involved in the transformations. I will try to figure out more precisely in which way the language of psy changed or shaped the self-reflexive vernacular.

The long history of self-reflection and self-objectification

It is obvious that Psychology did not create the language in which the individual's "inner" world of thoughts, feelings, and wishes could be verbalised. In other words, the psychologization of the Western vernacular dates back to earlier periods. In most analyses the psychologising, self-reflexive language is tied up with the history of individualism. The cultural, economic, ethical and political focus on the individual human being originates from a rich variety of historical sources. Let me just note the most important ones. The Renaissance in fifteenth century Italy and The Netherlands contributed the growing emphasis on the individual artistic accomplishments in the upper classes, Protestantism contributed the individualised relation between the believer and god, and Humanism 'began to take delight in man himself, the apex of creation, the master of nature' (Porter, 1997, p. 3). Humanistic values were also central to the confidence in the individual's rationality as exemplified by the Enlightenment. Its political and economic translation in the guise of liberalism pushed the expansion of an individualist conception of man. And, Romanticism, of course, added the artistic fascination with the inner roamings of mind, in particular with its darker sides.

The historical development of individualism is far more complex that this bird's eye view suggest, but despite the controversies over details, we can safely conclude that Western history shows a gradual dominance of an individualistic outlook on humankind. Its development has been materialized over the centuries in different ways. For example in new artistic forms, like the *self*-portrait, the *auto*biography, and the novel. But also in new political, and ethical structures, like for example, the French and American constitutions with their emphasis on individual freedom. Next to this, individual competion on the market became a common way to organise economic traffic. And individualism was also practised in all kinds of endeavours to map individuality, in particular individual differences (for example, craniométrie, phrenology, and psychology's mental testing).

The German sociologist Norbert Elias has explicitly noted the psychologisation that is concomittant with individualism. He sees the first forms of a 'psychological' observation (*Betrachtung*) around 1500 in the works of the Dutch humanist Erasmus, and observes a gradual expansion of this concern with personal feelings, thoughts, and the will to act at the sixteenth century French courts (Elias, 1939, II, 373-374). In later periods, we can discern the language of the inner world of the individual in the aforementioned genres like the autobiography, and the novel, which 'during the eighteenth century (...) established itself as the literary vehicle for the minute exploration of intense inner consciousness, particularly when cast in the form of a first person narrative' (Porter, 1997, p. 5; Mullan, 1997).

The historical sources allow me to conclude that before the construction of psychology as a discipline and a profession, the educated classes had a rather fine-tuned 'psychological' language to their availability. In this language they could reflect on their own "inner worlds", but they could also verbalise and map the individual differences that became a central value in nineteenth century capitalism. I will now look at the role of psychology with respect to this self-reflexive language.

Naming the mind and its consequences

In his recent volume *Naming the Mind* (1997) Danziger makes three important points regarding the question what the profession and discipline of Psychology did to the self-reflexive vernacular.

First, he shows that the question can hardly be answered in general terms. The terminology that is common today in both the vernacular and the psychological jargon shows a varied picture of historical development. Some terms and concepts like, for example 'behaviour', 'learning', and 'motivation' are tied up with the emergence of Psychology at the end of the nineteenth century, but others like, for example, 'emotion', 'consciousness', and 'self' were already part of the everyday vocabulary about human 'inner' subjectivity before psychology was institutionalised. The academic and professional language of psy incorporated these terms without much change in meaning (Danziger, 1997, p. 16, 36). There were also terms that did not survive the transition to the newly constructed language of Psychology. The 'will' is a famous example of this, and 'character' also lost the battle after a heroic struggle in the earliest decades of this century (Danziger, 1997; Van Strien, 1992).

Second, Danziger stresses the importance of the historical melding of the 'new', scientific concepts with social practices. Behavior, for example, was tied up with the development of educational practices, and in the case of motivation professional psychology profited from the popular litterature about 'personal efficiency', directed at managers and salesmen, that had been using the semantics of motivation for some time (Danziger, 1997, p. 113).

Third, Danziger urges us to take the normative nature of psychological categories into account. The language of psy is hardly ever neutral in its political effect. When experts of psy conceptualise, for example, the differences in children's achievement at school in terms of their IQ, or when psychologists categorise workers with respect to their nAch, this particular language legitimizes the dominant way of looking at individual differences: these differences are caused by the workings of some quasi biological mechanism or entity "inside" the individual.

If we take Danziger's contribution into account we can say that Psychology partly reshaped the selfreflexive language of the past. Next to this, Psychology paid a specific contribution to the description of personal experiences when new categories were created. Rose has put the constructive effects of Psychology in a wider perspective by showing that Psychological theories and practices created a new kind of subjectivity: 'a new type of person' was born when Psychology proliferated widely (Rose, 1997, p. 234). Finally, it is important to note that the "scientific" categories of Psychology could only be coined in the culture at large when they suited particular practical contexts, often of a normative, or political kind. As a last step in my argument, I will now discuss a historical case in which the language of psy was used to describe the experiences of people migrating to and from the Netherlands in the post-war period.

Psychologising migration

After the Second World War, the Netherlands became a multi-cultural society in a relatively short period. Dutch social scientists studied the behavior and experiences of the people who migrated to the Netherlands. In this paper I focus on a selection of the studies that were published around 1960. They were concerned with people who migrated or fled from the former Dutch Indies, with refugees who fled from Hungary after the 1956 revolt. In this same period, there were also studies published about Dutch people who migrated to countries, like, for example, Australia and New Zealand. I will first try to illustrate the language of psy that was employed in the studies about people who migrated to and from the Netherlands. Then, I will discuss the functionality of this kind of language (Jansz, 1992).

The inner world

A scientific analysis of the process of migration can be done in many different ways. When we look at the Dutch situation around 1960, it is interesting to note how readily social scientists of different professional backgrounds refer to the influence of internal processes when they analyse the experiences of migrants. The large scale study of the sociologist Kraak is a good example of this. Kraak and his team interviewed people who had to flee from Indonesia (Kraak et al., 1957). They analysed the refugee's experiences of resocialisation in terms of their individual 'attitude of adjustment'. They were able to subsume the attitudes under six categories, for example, the realist attitude, the passive attitude, and the dissatisfied attitude (p. 207-217).

Other experts of psy focussed on personality, or rather on personality *variables* to acknowlegde the professional jargon of those days. The psychologist Kuyer (1963), for example, found in his

research among Hungarian refugees that the variables independence, sociability, intelligence, and being able bodied were important in predicting a succesful assimilation. He also touched upon a question that became a central issue in later decades, notably the causal relation between migration and psychopathological distress: was the serious distress many migrants and refugees reported a consequence of their migration, or was it the distress they experienced at home that caused them to move to another country? Kuyer seemed inclined to embrace the hypothesis that a particular type of personality, or personality disorder predisposes the individual to migration.

A cause to act

Next to the adjustment and assimilation of migrants and refugees, the reasons for leaving the country among emigrants were studied. The psychologist Frijda (1960), for example, analysed the migrant's behavior from the perspective of motivation. In his study, he found that social motives were the most powerful incitement to act, far more important than economic reasons. The power of the individual's motives over other causes for action was underlined by Wentholt's research (1961). In the same period, Menges (1959) developed a diagnostic instrument to select candidates for migration. He introduced the concept of 'emigrability' as a predictor for successful migration. Menges was quite explicit when he argued that migration is always caused by a personal conflict. In other words, people do not leave their "motherland" out of free will.

The language of psy and its effects

We have seen some examples of the rephrasing the behavior and experience of migrants in the language of psy. The effects of this linguistic and practical operation are dependent on the social context. The experts' focus on individual attitudes, motives, and personality variables provides the psychological researcher and the professional counselor or therapist with a useful toolbox to describe and analyse the process of migration and its consequences at the individual level. At the social level, however, the implications of the language of psy are different. The focus on the so called inner world results in a differentiation between migrants: psychological research has, after all, shown, that people with the proper attitude, motive, or personality will assimilate succesfully, regardless of their circumstances. In this sense, the language of psy has created new kinds of subjects within the social category of migrants (Rose, 1990). And in the political realm the language of psy legitimised the Dutch policy of down-playing, and sometimes neglecting migration as a socio-political issue. It took until 1980 before the Government officially acknowledged that the Netherlands had become a multicultural society as a result of migration.

Conclusion

In this paper, I raised the question whether the institutionalisation of Psychology as a discipline and profession was a necessary condition for the appearance of twentieth century psychologization. My answer was in the negative, because historical sources show that self-reflexive, psychologising language is older than Psychology. But, Psychology has definitely reshaped the psychologising vernacular. Historical case studies, like the one I presented about the language of psy with respect to migration are necessary to add detail to the general picture of the new kinds of subjectivity that were created as a result of psychology's proliferation in Western culture.

References

Danziger, K. (1990). *Constructing the Subject. Historical Origins of Psychological Research.* Cambridge: Cambridge University Press.

Danziger, K. (1997). *Naming the Mind. How Psychology Found its Language.* London: Sage.

Elias, N. (1939). *Het Civilisatieproces, deel I & II.* Utrecht: Spectrum.

Frijda, N.H. (1960). *Emigranten. Niet-emigranten.* Den Haag: Staatsdrukkerij.

Herman, E. (1995). *The Romance of American Psychology. Political Culture in the Age of Experts.* Berkeley: University of California Press.

Jansz, J. (1992). De individualisering van migratie. Psychologische analyses van immigratie en emigratie 1945-1983. *Psychologie & Maatschappij*, 16, 276-293.

Kraak, J.H., Ploeger, P., & Kho, F.O.J. (1957). *De repatriëring uit Indonesië*. Amsterdam: ISONEVO.
Kuyer, H.J.M. (1963). *Twee jaar na de vlucht. Een onderzoek naar aanpasssing en persoonlijkheid van Hongaarse vluchtelingen*. Nijmegen: Academisch proefschrift.
Lasch, C. (1979). *The Culture of Narcissism*. New York: Norton.
MacIntyre, A. (1984). *After Virtue*. Notre Dame, IN: University of Notre Dame Press.
Menges, L.J. (1959). *Geschiktheid voor emigratie. Een ondersoek naar enkele psychologische aspecten der emigrabiliteit*. Leiden: Academisch proefschrift.
Mullan, J. (1997). Feelings and novels. In: R. Porter (Ed.), *Rewriting the Self. Histories from the Renaissance to the Present* (pp. 119-131). London: Routledge.
Napoli, D.S. (1981). *Architects of Adjustment. The History of the Psychological Profession in the United States*. Port Washington, NY: Kennikat Press.
Porter, R. (1997). Introduction. In: R. Porter (Ed.), *Rewriting the Self. Histories from the Renaissance to the Present* (pp. 1-14). London: Routledge.
Rose, N. (1990). *Governing the Soul. The Shaping of the Private Self*. London: Routledge.
Rose, N. (1996). *Inventing Ourselves: Psychology, Power, and Personhood*. New York: Cambridge University Press.
Rose, N. (1997). Assembling the modern self. In: R. Porter (Ed.), *Rewriting the Self. Histories from the Renaissance to the Present* (pp. 224-248). London: Routledge.
Sennett, R. (1977). *The Fall of Public Man*. New York: Vintage Books.
Strien, P.J. van (1992). Temperament, karakter en de psychologisering van de Nederlandse samenleving. *Psychologie & Maatschappij*, 16, 218-237.
Strien, P.J. van (1993). Nederlandse psychologen en hun publiek. Assen: Van Gorcum.
Wentholt, R. (1961). *Kenmerken van de Nederlandse emigrant*. Den Haag: Staatsdrukkerij.
Zeegers, W. (1988). *Andere tijden, andere mensen. De sociale representatie van identiteit*. Amsterdam: Bert Bakker.

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Cultural-Historical Theory and Postmodern Contents

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Summary

The social content of a theory distinguishes a traditional scientific approach from a more reflective and critical one. This interest signifies specific commitments, which necessarily contain social contents. But as this lack of interest is also a kind of commitment, it refers to a social content anyway. What is at stake here is the kind of relationship to social contents.

Cultural-historical theory is very saturated with social contents. Its relation to social contents is articulated at different levels: social content is its subject-matter, its explanatory principle, its onto- as well sociogenetic mechanism, part of its methodological reflection.

This elaborated status of the social content in cultural-historical theory is my starting point in the examination I am proposing here. Its aim is to relate the profound changes in social contents we have been witnessing in the last decades - usually labeled as the "postmodern condition" - to the claims raised by cultural-historical theory. My analysis will be led by the following questions:

- Can cultural-historical theory still be a historical theory of psychogenesis in the contemporary postmodern condition?
- Can cultural-historical theory challenge the postmodern age?
- Does postmodernity challenge cultural-historical theory?
- Can social constructionism replace cultural-historical theory?
- Is social constructionism a postmodern reduction of cultural- historical theory

In my view, cultural-historical theory, being developed in a specific social context, has become bound to that context. In order to be a proper historical theory, it should historize its own social content. In order to be a radical developmental theory, it should refer to developments taking place in postmodern conditions (as, for example, fragmentation of the subject, de-differentiation of the signifier and the signified, detachment from subject and from referent in the semiotic process, destruction of the universal meaning or substituting text for reality, even death of the subject and the end of the social).

But in opposition to conceptions which celebrate these developments, cultural-historical theory, using sociogenetic arguments, can show that they are not sustainable - either socially or psychologically. Is there any possibility of social action without the subjects of action? Is there any possibility of social action without the social? How is it then possible to conceptualize social action within the framework of postmodernity? Even postmodernity needs social presuppositions, but it is not able to constitute them.

In my opinion, contemporary cultural-historical theory still requires - as was the case in the original Vygotskian cultural-historical theory - a general far-reaching social action which then provides us with social contents for our inter-individual activities.

To meet these demands would mean to do cultural-historical theory – radically.

Introduction

Since the earliest times questions about the nature of human activity have attracted the human mind. Among the subject-matters of knowledge, those related to human affairs were often intuitively given a special place which reflected their specificity in comparison to the other matters belonging to the realm of, broadly speaking, nature. Whatever the concrete content of these specificity intuitions (one way of articulating it is also a differentiation between theoretical and practical philosophy), they have remained a fruitful source until the recent scientific reflections.

Within the history of scientific knowledge, insight into the specificity of subject-matters related to human activities in general has been articulated as a problem of the foundations of social and human sciences. At the same time the problem of foundations has been approached from another side claiming the unity of science regardless of its subject-matter.

The history of psychology has been bearing witness to the prevalence of the claim to unity. Nevertheless the specificity claim has had its adherents from Dilthey until the present time.

As a matter of fact, in the last decades the insights of the specificity claim started to be applied even in the reflection on natural sciences. As a result, a growing sensitivity to social mediation as a universal mechanism in human knowing, has been developed.

The contemporary theoretical landscape of psychology has been characterized by the emergence of approaches (for example, cultural psychology, discourse analysis, social constructionism – I will focus on social constructionism) which request a reflection on assumptions taken for granted in the main stream of psychological theorizing and researching. What has been taken for granted in mainstream psychology, has been seen in these approaches as just a particular standpoint with a limited scope of perception, interpretation and validity: the granted dissolved into constructed and that constructed in a context which offers tools and possible meanings as transmitters of knowledge, beliefs and values. In this way, society started to enter the process of knowing as its constitutive moment. From the metatheoretical point of view one could speak of the "socialization" of theoretical knowledge. If we continue to use the psychological metaphor of socialization in the metatheoretical context, other questions related to the original use of that concept could be raised. What are factors determining this kind of socialization? What kind of goals are expected to be achieved through it?

The first question about the factors is aimed at a sociogenetic explanation of theoretical development. It is a challenge for an approach which understands itself as socially reflected (as it is the case with the social constructionism) to apply such an analysis to itself. But at this challenge point social constructionism failed: what their adherents offer as a "socio-genetic" explanation, is actually a regression to, what they usually name, traditional theorizing, which means using solely internal schemas, for example, crisis in a discipline, as explanatory principles of scientific development. This failure shows that the reflection on social mediation of knowing and theorizing is still an open task.

One way to approach this task could be to look at conceptions which shared similar commitments but were developed under different socio-historical conditions. Thus, looking for a predecessor, in some sense, of social constructionism is not just a historical search, but, in my view, a kind of theoretical reconstruction.

Cultural-historical theory versus social constructionism

Referring to Vygotsky's cultural-historical theory, I see as a contribution to the reconstruction of the context from which to understand contemporary social constructionist approaches. It should be stressed that their representatives themselves don't refer to Vygotsky. In that sense, what I am proposing here is a construction of a metatheoretical subject-matter and context – not its discovery or reproduction. Though it goes beyond the reflection articulated by social constructionists, the proposed construction is in accordance with the claims constitutive for the social constructionism.

Cultural-historical theory was developed in a unique historical and social context in the years following political and social revolution in Russia. The event itself, the scope of social changes it brought have no precedence in history – radical social changes cannot be denied regardless of their evaluation. This gives reasons to raise the question about the relation of this theory to that social context. Michael Cole and Sylvia Scribner wrote in their introduction to the English edition of Vygotsky's writings published under title Mind in Society: "We have also found it helpful to examine the condition of psychology and society in postrevolutionary Russia, since they were the source of the immediate problems facing Vygotsky as well as a source of inspiration..." (Cole, Scribner, 1978: 1)

What kind of resources has that social context offered? In addition to the resources which define every socio-cultural structure, the socio-historical context in question had some specific features: instead of reproducing existing patterns, the aim was conceived of as development of new, or even radically new socio-psychological patterns. In the semantics of that "new" the negation of the old, previous patterns had an important role, but still there was not a small part lacking concrete means to develop what was set as wanted. The gap between the needs and lack of means to satisfy these needs, was, at least for some time, a strong motivational source for searching, experimenting, constructing. Maybe this can help one to understand, at least partly, enormous creative achievements in many fields, especially in arts, in the Soviet Union at that time. Psychology, in the form of cultural-historical theory, took part in these unique creative developments.

Vygotsky proclaimed as his goal to develop such a psychology which could raise proper problems of the psychology of human beings. This was his reply to the diagnosis of the crisis in psychology which was manifested in bifurcation of psychology into subjective-idealistic approaches to complex processes, on one hand, and reductionistic explanation of simple psychic phenomena, on the other hand. "Historical significance of the crisis in psychology" was the title of the study Vygotsky wrote in 1926. After critical examination of psychological concepts and explanantory schemas (including their philosophical prehistory), Vygotsky pleaded for a general psychology which can bring the then lacking unity in psychology.

Yygotsky explicated his understanding of the development of science as determined by "1) the general social atmosphere of the epoch; 2) general laws of scientific knowledge, and 3) the demands of objective reality." (Veer & Valsiner, 1991/1993: 144; Vygotsky ,1926/1982:I - 302 – in Russian). This is the general level at which social-historical context determines theoretical developments. In Vygotsky's case this is at the same time a part of his metatheoretical reflection – he is aware of the social relatedness of theorizing.

How significant this insight is, could be shown if we compare metatheoretical reflection expressed seventy years later – in the contemporary social constructionism. In a recent book, published under the title Introduction to Social Constructionism, Vivien Burr, after defining her task as: "contributing to what might be called 'the social construction of social constructionism'" (Burr, 1995: 10) summarizes her answer to the question "Where did social constructionism come from?" as follows: "Its cultural backdrop is postmodernism, but it has its own intellectual roots in earlier sociological writing and in the concerns of the 'crisis' in social psychology. Social constructionism is therefore a movement which has arisen from and is influenced by a variety of disciplines and intellectual traditions." (Burr, 1995: 14)

In her attempt to define social constructionism Burr relies on Kenneth Gergen's proposal to "group as social constructionist any approach which has at its foundation one or more of the following key assumptions.

1. A critical stance towards taken-for-granted knowledge: (...)
2. Historical and cultural specificity: (...)
3. Knowledge is sustained by social processes: (...) It is through the daily interactions between people in the course of social life that our versions of knowledge become fabricated. Therefore social interactions of all kinds, and particularly language, is of great interest to social constructionists. The goings-on between people in the course of their everyday lives are seen as the practices during which our shared versions of knowledge are constructed. (...)
4. Knowledge and social action go together: (...) But each different construction also brings with it, or invites, a different kind of action from human beings. (...) Descriptions or constructions of the world therefore sustain some paterns of social action and exclude others."(Burr, 1995: 2-5)

There is a striking discrepancy between the fundamental social constructionist tenets and "the social construction of social constructionism" offered by Vivien Burr: social processes and actions are reduced to interactions with theoretical objects (in the form of sociological or postmodern writings). Explanation of the emergence of social constructionism sounds like an explanation of traditional psychologies – which are permanent target of the social constructionist critique.

Surprisingly enough for an endeavour to offer "a social account of science" (Gergen, 1994: XVII) the shift from the world to text is seen by Gergen as his taking part in "the major intellectual developments" in the last two decades . In the preface to the second edition of the Toward Transformation in Social Knowledge Gergen writes: "Once the link between language and the world is rendered contentious and contingent, the analysis shifts importantly from what is the case about social life, invariably stated in language, to the languages of description and explanation themselves – from the world to text, from the signified to the signifier." (Gergen, 1994: XIII)

The insights into the constitutive role of signifier in the defining the signified cannot be disproved. But it is no less important to bear in mind that signifier is about signified, that signifying

is an intentional act (in Brentano's sense). Once the signifier is focused upon, the role of signified is given almost no attention - it has become obliterated in contemporary theorizing.

In Vygotsky's theory language is one of the main subjects. Concluding his investigation into the relation between thought and language Vygotsky states: " We showed that a generalized reflection of reality is the basic characteristic of words. This aspect of the word brings us to the threshold of a wider and deeper subject – the general problem of consciousness . Thought and language which reflect reality in a way different from that of perception are the key to the nature of human consciousness. Words play a central part not only in the development of thought but in the historical growth of consciousness as a whole." (Vygotsky, 1934/1969: 153)

Instead of operations of focusing and supressing – which we find in contemporary theories- it would be epistemologically more fruitful – and practically more promising – to re-establish interactional relation between the signifier and signified. Language is not a copy-representation of "what is the case", but if it is conceived of as a self-contained system (where signified is just another signifier), it is not possible to understand his pragmatic function which it evidently has. It is of even more importance to recall this relation if we take into account broader social contexts. As Heinz-Günther Vester warned: "Gleichwohl vermögen simulierte Politik und auto-poietisches Theoretisieren über Politik bestimmte Realitäten qua Ignoranz nicht einfach abzuschaffen." (Vester, 1993: 188)

Gergen himself acknowledges "discourse is inherently a by-product of social interchange" and therefore "a focus on discourse alone (too often the case in post-structuralist writings) is insufficient." But even "the function of the sciences within the broader set of patterns constituting cultural life" (Gergen, 1994: XVII) – is not the whole social content of the sciences. It should be added the function of social system or particular social patterns in making possible, producing, making visible or desirable problems, structures, activities, groups, beliefs, values – and supressing others. Community not only describes a person's identity but constitutes it.

Interactionist origin of knowledge – recognized as one of the tenets of social constructionism – is also one of the main theses in the cultural-historical theory. For Vygotsky "the source of human consciousness and freedom should not be sought in the internal world of the intellect, but in the social history of mankind." "The basic difference between our approach and that of traditional psychology will be that we are not seeking the origins of human consciousness in the depths of the 'soul' or in the independently acting mechanisms of the brain (where we shall find nothing). Rather, we are operating in an entirely different sphere – in human's actual relationship with reality, in their social history, which is closely tied to labour and language ." "(...)The very mechanism underlying higher mental functions is a copy from social interaction; all higher mental functions are internalized social relationships. These higher mental functions are the basis of the individual's social structure. Their composition, genetic structure, and means of actions – in a word, their whole nature – is social. Even when we turn to mental processes, their nature remains quasi-social. In their own private sphere, human beings retain the functions of social interaction." (Vygotsky,1931: Development of Higher Psychological Functions, in Russian; in English, 1981; quoted after Burkitt, 1991: 137-8, 142-3, 147)

Vygotsky's starting point could be described as externalization of the psyche (as well as of psychology). Externalization means socialization, because "external" means, as Vygotsky said, "social". Though Vygotsky himself has chosen Goethe's "In the beginning was the deed" (with some variation stressing "in the beginning") (Vygotsky, 1934/ 1969: 153), it would be more representative for the content of his psychological position to add a new variation: "In the beginning was the interaction."

Interaction is certainly the main concept of Vygotsky's social ontology. How did he conceptualize the interaction? Paradigmatically, interaction occurs between the adult and the child, whereby the adult is a generous provider of tools, especially of symbolic tools or signs. Tools and signs trasmit inherited social knowledge, beliefs , values.

It is clear that such conception of interaction cannot be generalized as a universal form sufficient to describe the totality of interactions in a society. Society is built up of different kinds of interactions. Assumptions of benevolence, an active interest in the good of others, an unrestricted access to social heritage and willingness to trasmit it and share it with others – these are to a great extent contrafactual assumptions. Therefore, Vygotsky's concept of interaction needs

differentiation in order to be able to serve generative function in the other realms of social life – beyond the ontogenetic development.

In Vygotsky's theory even the ontogenetic interaction itself is cut off from the ongoing broader social interactions – where new signs could be produced which conflict with those already appropriated by the adults. Then, every tool and sign has more or less explicit connotations derived from the particular interpretations of the world, especially social reality – this could be a source of conflicts too. A very important mediator in any interaction are emotional patterns which are not included in the formulation of the sociogenetic law of cultural development of the higher mental functions - though Vygotsky at the end of his study Thought and Language confessed: "Thought is engendered by motivation, i.e. by our desires and needs, our interest and emotions. Behind every thought there is an affective-volitional tendency, which holds the aswer to the last ‚why' in the analysis of thinking. A true and full understanding of another's thought is possible only when we understand its affective-volitional basis." (Vygotsky, 1934/1969: 150)

These lacks in Vygotsky's theory pose a hermeneutical task. There is a latent text behind Vygotsky's expliciated metatheoretical reflections. It is not so difficult to unearth "Menschenbild" from which Vygotsky derived his assumptions. It is an image of human being as socially bounded, but thanks to this, internally integrated and free, capable of rational self-control .Out of these conditions humans develop as morally good actors. Such a picture of the human being has to be rebuilt and recognized through the unified psychology.

Cultural-historical theory and postmodernity

From the so called postmodern point of view Vygotsky's theory, in its whole content, with its methodological claims to the unity, with its commitments to self-controlled rational agency would necessarily belong to the traditional, outdated and in the meantime delegitimated modern meta-narrative. Though, to my best knowledge, no postmodern author or text ever referred to Vygotsky, if they did, Vygotsky would be a target of their devastating critique.

But due to its social boundness cultural-historical theory is invited to establish a relation to the changed contemporary socio-cultural context. Whether these changes could be best described and conceived of as postmodern era (which would mean giving up modern structures and claims), cannot be discussed here.

I will focus on changes which are of special importance to cultural-historical theory. These are changed patterns of interactions and changes in semiotic mediation, described as characteristic of a postmodern situation. As interaction and semiotic mediation are constitutive for cultural psychological development, according to Vygotsky's theory, it is clear that a cultural –historical theory cannot ignore the changes these structures and activities are undergoing in contemporary social life. What is at stake here is a core of development and consequently also of developmental theory.

Simultaneity, plurality of interactions, their rapid turnover (nomadic character), fragmented interaction patterns, isolation from the global and focus on the local, de-rationalization (cleansing from long-term rationale), detachment from the agency of the subject, dissolution, implosion – or even "the end of the social" (Baudrillard, 1983) – in this way are usually described changes in the social affairs in the last decades. Could cultural-historical theory incorporate these changes and still keep on its commitments? Could cultural-historical theory offer what is missing in the postmodern consciousness - or to say it more radically: could cultural-historical theory overcome postmodernism as a symbolic tool – these are crucial questions concerning the relation between the cultural-historical theory and postmodernity or postmodernism.

Detachment from the rational subject as an epistemic basis of knowledge and from subject as a paradigmatical structure of the social life is the postmodern diagnosis which questions the core structure of modernity. Actually, under modern subject it is understood subject conceived of in socio-psychological terms of liberalism – presupposed self-contained, autonomous individual opposed to society, reproduced through demands of self-control, pursiung of self-interest. It is evident that liberalism (as ideology) is built up on conceptual (and axiological)schemas which argue for the decentration and withdrawal from the social (though a very special socio-historical constellation made possible its emergence and gave it a particular meaning). Thanks to this

decentration it was possible to make the next step – to universalize the subject of liberalism as a transhistorical, which means ahistorical structure.

As the individual and society are correlative notions – they are a way to conceptualize the dynamics going on under conditions of common life, in this case using a dualistic model, liberalism operates with a socially deprived concept of individual subject as well as with a very poor concept of society: a socially unsaturated individual subject is – at the conceptual level – a necessary logical consequence of the unsaturated concept of society.

Exactly these conceptions entered psychology as its implicit assumptions on which edifice of psychological knowledge has been erected. Consciousness as taken for granted is a psychological complement of an autonomous subject. Such conceptions of consciousness were critisized by Vygotsky who argued that instead of being contrafactually taken for granted, consciousness should be analysed from its origin through developmental stages until its mature forms. Thus Vygotsky dissolved the autonomous consciousness into social interactions – as its origin and content. But he has not abandon either the concept or the mental structure of consciousness – on the contrary, he pleads for the functions the consciousness in its most developed forms can perform: regulations of other functions, planning of activity, self-control and appropriation of socio-historical heritage. Consciousness in Vygotsky's theory is very saturated with social contents, which means that his concept of society must be also very saturated.

Vygotsky's theory proves that detachment from the presupposed autonomous subject does not necessarily lead to abandoment of the subject. In this particular case it led to the strengthening of its position by making its social genesis available to its consciousness and activity. Analysing Vygotsky's concept of internalization as a mechanism of development of higher, cultural mental functions, James Wertsch and Addison Stone came to the following conclusion: "Vygotsky's theoretical framework is constructed in such a way that the concept of internalization cannot be discussed independently of the social origins of individual activity. In the final analysis, as Vygotsky says, internal activity is quasi-social in nature. In order to appreciate the import of this claim, the social origins of internal activity must be examined. We have argued that one way to construct an integrated account of these issues is to examine the child's emerging control of external sign forms. This process involves the mastery of socially defined activity through coming to appreciate the full significance of the signs one uses in social interaction." (Wertsch & Stone, 1985/1989: 177). Vygotsky's solution for the consciousness could be formulated in the statement: Consciousness emerges out of human social life (interaction) through the emergence of control over the external signs forms.

Let me come to another issue of postmodernity. Postmodernity is understood as a conceptualization of the conditions and forms of life in the so-called post-industrial societies. As every conceptualization is based on some choices, postmodernity is also bound to choices it made concerning relevant objects of observation, attention, evaluation, interpretation. Culture was its birthplace and its prefered subject-matter. The way postmodernity usually conceptualized cultural changes has meant a detachment from global social structures. Instead of the totality of society, the local context appeared as the proper horizon of postmodern thinking: temporary, local consensus, language game, local meaning (Lyotard, 1979). Thus the local (metaphorical, connotative) meaning – to take the postmoden favorite of language as an example – is cut off from the language as a system which presupposes universal rules as a condition sine qua non of mutual understanding.

In Vygotsky we can find an excellent argumentation proving that communication and generalization are inextricably linked. Even more: communication requires generalization. "Closer study of the development of understanding and communication in childhood, however, has led to the conclusion that real communication requires meaning –i.e. generalization - as much as signs. According to Edward Sapir's penetrating description, the world of experience must be greatly simplified and generalized before it can be translated into symbols. Only in this way does communication become possible, for the individual's experience resides only in his own consciousness and is, strictly speaking, not communicable. To become communicable it must be included in a certain category which, by tacit convention, human society regards as a unit. Thus, true human communication presupposes a generalizing attitude, which is an advanced stage in the development of word meanings. The higher forms of human intercourse are possible only because man's thought reflects conceptualized actuality." (Vygotsky, 1934/1969: 6-7)

Postmodern treatment of the local – consensus, context, meaning – consists of a focus on the local and the conceptual repression of the global, universal. Unwillingness to grasp that background out of which local figures can only apper does not make that background non-existent, but it influences the attitude (or action) to it. Thus "the end of the social", as a possible diagnosis (Baudrillard, 1983) of the contemporary affairs means first of all the end – or exhaustion , whatever the reasons– of the interest in shaping the social according to ideas about the good or just. As the producers of these ideas cannot be but subjects – the end of the social and the death of the subject are two aspects of the same process. As Bourdieu warns: "(...) the existential mood of a whole intellectual generation...seeks in narcissistic self-absorption the substitute for the hope of changing the social world or even of understanding it." (quoted in Michael, 1991: 209)

What would be Vygotsky's attitude toward postmodernity? His way of theorizing gives reason to suggest the following answer. Though it might sound paradoxic, I will argue that Vygotsky would treat postmodernity as he treated traditional psychology, which means he would question its assumptions and then try to reconstruct the paths of their social genesis, to establish relation to the social context – where, for example, texts were produced and then substituted for reality.

Vygotsky explicitly linked consciousness to generalisation through the word as a generalized reflection of reality. There is another condition necessary to bring consciousness to mental functions – this is, to use the postmodern term – intertextuality, or in Vygotsky's words – system of concepts. "In this way, becoming conscious of our operations and viewing each as a process of a certain kind – such as remembering or imagining – leads to their mastery. (...) Scientific concepts, with their hierarchical system of interrelationships, seems to be the medium within which awareness and mastery first develop, to be transferred later to other concepts and areas of thought. Reflective consciousness comes to the child through the portals of scientific concepts. (...) To us it seems obvious that a concept can become subject to consciousness and deliberate control only when it is a part of a system. If consciousness means generalization, generalization in turn means the formation of a superordinate concept that includes the given concept as a particular case. (...) Thus the given concept is placed within a system of relationships of generality." (Vygotsky, 1934/1969: 91-2)

Thus, system of concepts, or broadly speaking, intertextuality, fosters consciousness and consciousness means mastery over internal processes and external signs. Consequently, intertextuality presupposes relation to reality. This would be Vygotsky's reply to postmodern intertextuality which replaces reality.

Conclusion

Cultural-historical theory has to approach postmodernity if it is to be truly a cultural-historical theory– a theory which conceives of human development in terms of forms of interaction and signs as tools of development. Postmodernity is a contemporary pattern of new ways of interaction in culture and everyday practice. Mike Michael gives a condensed description of the postmodern phenomenology. The central characteristics of postmodernism that are to be emphasized are those of transgression and accelerated turnover. As noted above, transgression refers to the postmodern trend of breaking down what had appeared to be established, discrete categories such as art or culture and reality. Accelerated turnover refers to the increasingly hectic throughput of images, texts, categories, both in the media and in the life of the supposed typically postmodern individual. Further, postmodernism addresses the pursuit of these rapid changes in terms of the consumption of spectacle." (Michael, 1991: 207-8) Postmodernity is also a symbolic interpretative tool which mediates our thinking of the world and our attitude toward the world. It construct a consciousness detached from the social and fragmented into many seemingly unrelated local self-contained fields.

Cultural-historical theory could be a symbolic tool or scaffolding to establish – once again – missing links to social origin and genesis of postmodern consciousness. Only this would mean a proper Renaissance of Vygotsky's theory – meaning the historical significance Renaissance once had using the best achievements of the past in order to overcome the then existing crisis and finally building foundations for the announced new world – then named Modern Age.

My choice of the metaphor of Renaissance to describe the possibility of cultural-historical theory of postmodernity recalls Toulmin's plea for humanizing modernity, understood as "our need to reappropriate the reasonable and tolerant (but neglected) legacy of humanism. (...) The current task, accordingly, is to find ways of moving on from the received view of Modernity – which set the

exact sciences and the humanities apart – to a reformed version, which redeems philosophy and science, by reconnecting them to the humanist half of Modernity." (Toulmin, 1990/1992: 180)

At the same time I agree with Habermas' warning: " (...) instead of giving up modernity and its project as a lost cause, we should learn from the mistakes of those extravagant programs which have tried to negate modernity." (Habermas, 1980/ 1994: 351). Thus my plea for modernity joins Habermas' interpretation of modernity in terms of emancipation and equality, and consequently a diagnosis of our time as an unfinished, incomplete modern project.

References

Baudrillard, J. (1983): *In the Shadow of the Silent Majorities, or, The End of the Social and Other Essays*, (trans. P. Foss, J. Johnston and P. Patton)Semiotext(e), New York.

Burkitt, I. (1991): Social Selves: Theories of the Social Formation of Personality, *Current Sociology*, Vol. 39, N.3.

Burr, V. (1994*): An Introduction to Social Constructionism*, Routledge, London, New York.

Cole, M. & Scribner, S. (1978). Introduction, in: L. S. Vygotsky, *Mind in Society*, M. Cole, V. John-Steiner, S. Scribner, E. Souberman (eds), Harvard University Press, Cambridge, London, pp.1-14.

Gergen, K. (1982/1994*): Toward Transformation in Social Knowledge*, Sage, London.

Habermas, J. (1981/1994): Modernity versus Postmodernity, in: J. Alexander & S. Seidman (eds): *Culture and Society*, Cambridge University Press, pp. 342-53.

Lyotard, J.-F. (1979): *La conditon postmoderne*, Minuit, Paris; *The Postmodern Condition*, Manchester University Press, Manchester, 198

Michael, M. (1991): Some Postmodern Reflections on Social Psychology, *Theory and Psychology*, 1991/2: 203-221.

Michael, M. (1994): Discourse and Uncertainty: Postmodern Variations, *Theory and Psychology*, Vol. 4/3: 383-404.

Toulmin, S. (1990/1992): *Cosmopolis*, The University of Chicago Press.

Van der Veer, R. & J. Valsiner (1991/1993): *Understanding Vygotsky*, Blackwell, Oxford, Cambridge.

Vester, H.-G. (1993): *Soziologie der Postmoderne*, Quintessenz, München.

Vygotsky, L. S. (1926/1982): Istoricheskij smysl psikhologicheskogo krizisa, (Historical Significance of the Crisis in Psychology), in: L. S. Vygotsky*: Sobranie sochinenij, Tom 1. Voprosy teorii i istorii psikhologii*. Pedagogika, Moscow, pp. 176-95.

Vygotsky, L. S. (1931/1978): The Development of Higher Psychological Processes, partly in: L. S. Vygotsky: *Mind in Society*, M. Cole et al. (eds), Harvard University Press, Cambridge, London; also in: J.Wertsch (ed) (1981): The Concept of Activity in Soviet Psychology, M.E.Sharpe, Armonk.

Vygotsky, L. S.: (1934/1969). *Thought and Language*, E. Hanfmann & G. Vakar (trans. and ed.), The MIT Press, Cambridge.

Wertsch, J. & C. A. Stone (1985/1989): The concept of internalization in Vygotsky's account of the genesis of higher mental functions, in :J. Wertsch (ed*): Culture, Communication and Cognition*, Cambridge University Press, Cambridge, pp. 62-79.

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Origins Research in Anthropology and Vico's *New Science* (1744)

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Since ancient times, questions about the origins of human cognition, language, religion, agriculture, social inequality, and the state have figured centrally in the ways scholars have thought about human history. During the 19th century, these questions became the foci of anthropology's various areas of 'origins' research. Throughout the 19th and 20th centuries, change in anthropological methods and theory has always been accompanied by efforts to rethink and remodel the field's diverse areas of origins research. The continuing significance of origins research to anthropology's disciplinary definition, conceptual foundations and goals is thrown into relief by Colin Renfrew and Paul Bahn's "world chronology" in *Archaeology. Theories, Methods and Practice* (1994):

> The human story begins in East Africa, with the emergence ...of the earliest hominids of the genus *Australopithecus* around 4 or 5 million years ago. [W]e have increasing evidence for fully modern people--our own species, *Homo sapiens sapiens*...by at least 100,000 years ago...[T]he transition... to food production seems to have occurred...after ca. 10,000 years ago. The urban revolution...reflects profound social changes [including] the development of *state societies* (Renfrew and Bahn 1994:142-148).

The 20th century has seen major change in the methods used to investigate questions about diverse origins 'events'. Today each is the focus of one or even several specialized fields of multi-disciplinary inquiry, each with its own combination of techniques, analytic procedures, and interpretive principles. However, the importance of origins research to the ways anthropology defines is aims and structures its fields of inquiry is not the only manifestation of paradigmatic continuity. Throughout the 19th and 20th centuries, the most polemical debates have turned on three issues, which have engaged scholarship on human history since antiquity, namely, those of characterizing (a) human nature, (b) the causes of socio-cultural change and diversity; and (c) the conditions of historical knowledge. These themes are commonplaces of debate in diverse areas of origins research today. They were commonplaces of 19th century debate over the cultural evolutionary schemes of the founding figures of 'classical' anthropology. And before that, they engaged some of the most influential philosophers of the Enlightenment and Romantic movements.

The basic concepts which divide apparently antithetical theoretical paradigms have not changed in fundamental respects either. Throughout anthropology's history, the nature-culture dichotomy has provided opposing theoretical programs with analytic tools; and the discipline as a whole with an identity marker. The persistence of the nature-culture antithesis is rather remarkable in light of how centrally it figures among the Western dualist categories, which anthropologists have successfully criticized, such as those of mind-body and individual-society. The antithesis has also been challenged by advances in major areas of origins research, which would not have been possible without the co-operation of human and physical scientists. Yet to this day, the most controversial debates turn on opposing theories about human nature, history, and the epistemological status of historical knowledge, articulated in relation to the nature-culture interface. In the 1970s, persisting debate over the relative merits of materialist and culturalist types of determinisms led Marshall Sahlins (1976:55) to justifyably characterize anthropological theory as a "prisoner pacing between the farthest walls of his cell." Dualist paradigms focus on contrasting sides of the nature-culture opposition---nature shaping culture versus culture imposing meaning on nature. But they share several notable features in common, including beliefs that nature and culture constitute ontologically antithetical domains; universalistic conceptions of nature; and notions about cultural diversity and change, which are historically rooted in ideal views of the Scientific Revolution, the Birth of Modernity, and modern Western culture's supposed triumph over nature (Ingold 1996; Descola and Palsson 1996).

To the best of my knowledge, the first scholar of the Enlightenment to call attention to the ways anachronistic modes of reasoning prohibit a satisfactory philosophy and science of human history was Giambattista Vico (1668-1744), in his *New Science of the Common Nature of the Nations* (first edition 1725 [1928 ed. F. Nicolini], third edition 1744 [1948 tr. Bergin and Fisch, henceforth abbreviated to NS]). Vico examined relationships between the diverse changes in Western thought and culture which converged in his times. He also investigated how these changes promoted wrongheaded generalizations about humanity's history.

Vico was not content with negative warnings. The New Science was intended to serve as a critical and constructive alternative to Descartes's rationalist and Bacon's empiricist philosophies, social theories based on a State of Nature-Social Contract antithesis, and the new physical science of Galileo, Leibniz and Newton. Vico (NS/138-140) believed that the realization of a science of humanity hinged on creating new ways to bring the work of philosophers and philologists (human scientists and historians) into fruitful collaboration. The development of such a science had hitherto been impeded by philosophers' and philologists' failure to take each other's work into account. Had they done so, Vico (NS/140) argued, we would already have a science which was both a historiography and philosophy of humanity. The solution Vico proposed, and which is fundamental to his New Science, was to bring the two into fruitful collaboration. This required a new methodology for research on the origins and history of humanity. Essential to Vico's methodology were a novel Newtonian restatement of the pre-conditions of a science of humanity (NS/43-360); and his "discovery" that the earliest forms of human symbolic thought and communication were highly metaphorical, mythological, and dependent on images generated by sensory experiences encoded by the imagination (NS/361-778). What would be lost, according to Vico, without a satisfactory science of humanity would be the ability to address questions about:

(a) the bases of the earliest "poetic wisdom";
(b) the functions of the imagination in humanity's history;
(c) patterns of similarity and contrast among the historical trajectories of civilizations (or the "nations"); and
(d) the pre-conditions of the modern world--"of this world of sciences, which specialized studies of scholars have since clarified for us by reasoning and generalization" (NS/778).

In this talk I wish to focus attention on the relevance of Vico's New Science to research on the pre-paradigmatic history of anthropological theory, and efforts to go beyond the nature-culture dichotomy in the discipline's key areas of origins research. I will discuss Vico's arguments for the possibility of science of humanity, and his approach to the four questions listed above.

The possibility of a science of humanity

The table of contents of the New Science shows the breadth of Vico's conception of what a satisfactory science of humanity would entail. The work consists of a statement on "The Idea of the Work" and five Books. Book I, concerns "The Establishment of Principles" for a science of humanity. Vico was keenly aware that the possibility of such a science had been removed from the agenda of phil

Vico says that the main requirements of a science of history are:

(a) that "philosophy undertakes to examine philology" (NS/7) on the basis of the *verum=factum* principle; and

(b) that sciences "must take their beginnings from that of the matters of which they treat" (NS/314).

Vico's *verum et factum convertuntur* principle combined aspects of Newton's anti-Cartesian philosophy with the ancient notion of 'maker's knowledge' to restate the conditions of a science of history: a science of history is possible because human history and knowledge had been made by human beings themselves (NS/349). His approach to the second requirement was based on his "discovery" that the earliest human thought consisted of mythopoetic images generated by sensory experiences encoded by the imagination.

Imagination, ecology, and the preconditions of the modern world

After "establishing the principles" for a science of history, Vico proceeded in Books II through V to seek answers to hitherto unsolvable questions about that history. Book II, "Poetic Wisdom" concentrates on the subject Vico believed to be the key to a new philosophy and history of humanity. This was his "discovery" that the decisive element in the humanization process was the capacity of the first human beings to create "poetic wisdom" (NS/34), or what we nowadays call mythopoetic thought and the logic of the concrete (Lévi-Strauss 1966). Vico defines "poetic wisdom" as wisdom that is at once made and creative (Mooney 1985). He defines the earliest humans as poets in the Greek sense of 'creators' (NS/400-403). Poetic wisdom is, thus, not simply a product of the mind. It is the very process whereby the earliest humans created their human natures.

Poetic wisdom has several characteristics relevant to efforts to go beyond a nature-culture dichotomy in anthropological origins research. Two notable examples include that poetic wisdom

(a) is structured by topological forms of reasoning; and

(b) has an ecological basis.

According to Vico, poetic wisdom has three structuring dimensions:

(a) the imagination's capacity for topological reasoning;

(b) a grammar or logic motivated by four basic types of poetic tropes (*verba translata* = words with transferred meanings) and

(c) concrete particulars ("certainties") which function as the images or symbols of "poetic wisdom.

These aspects of poetic wisdom make it clear that in the New Science humanity is not a product of State of Nature - Social Contract antithesis, or a universalistic Reason. Humanity developed (to use Descola's 1996 terms) "in the society of nature" through the imagination's capacity for "poetic wisdom." Vico expresses this in strikingly modern terms.

> So that, as rational metaphysics teaches us that man becomes all things by understanding them (*homo intelligendo fit omnia*), this imaginative metaphysics shows that man becomes all things by *not* understanding them (*homo non intelligendo fit omnia*); and perhaps the latter proposition is truer than the former, *for when man understands he extends his mind* and takes in the things, *but when he does not understand he makes the things out of himself and becomes them by transforming himself into them* (NS/405; italics mine)

Book IV, "The Course that Nations Run," presents an ideal model (*storia ideal eterna*) to investigate the long-term sequences of change in socio-political structure, ecology, and cosmology, which constitute the historical trajectories of civilizations. Vico's model does not describe these actual trajectories. It functions as a methodological tool to examine hypotheses about the causes and effects of human creativity in historical processes on the basis of philological evidence (Funkenstein 1986:282). These processes are not irreversible or unilinear. Book V, "The Recourse of Human

Institutions which Nations take They Rise Again," investigates *ricorso*, patterns in the decline and re-emergence of civilizations. A notable feature of Books IV and V, and the New Science in general, is a lack of artificial of nature-culture, and evolution-history antitheses. This is why, as Berlin (1976) puts it, in Vico's science of humanity's history:

> "the nature of man is not, as has long been supposed, static and unalterable or even unaltered;... that men's own efforts to understand the world in which they find themselves and to adapt it to their needs, physical and spiritual, continuously transform their worlds and themselves" (Berlin 1976:xvi).

One argument that runs through the New Science is that without a satisfactory approach to the mythopoetic basis of humanity it is impossible to understand the pre-conditions of the modern world. Vico explains:

> The human mind does not understand anything of which it has no previous impression...from the senses (NS/363). But in the night of the thick darkness enveloping the earliest antiquity, so remote from ourselves, there shines the eternal and neverfailing light of truth beyond all question: that *the world of civil society has certainly been made by men, and that it is in our ability to retrieve its principles from within the modifications of our human mind* (NS/331). [Thus we discovered that the] first founders of humanity applied themselves to a sensory topics (NS/495). In this language of the senses, we discovered the beginnings of this world of sciences, which the specialized studies of scholars have since clarified for us by reasoning and generalization (NS/779).

Concluding remarks

In an essay on "Ecology as Semiotics" (1996), Alf Hornborg calls attention to a relation between two themes in human ecology.

> One is the epistemological contrast between 'dualist' and 'monist' approaches to human ecology. The other is the issue of whether or not traditional, pre-industrial human societies have something to tell us about how to live sustainably (Hornborg 1996:45).

Hornborg's observations make possible two concluding suggestions. One is that Vico's goals to develop a critical and constructive 'monist' alternative to the predominate dualist systems of his times (NS/138-140) may contribute to the relevance of his New Science to studies of the history of anthropological theory. Second, Vico's 'monist' approach to human ecologies and relationships between mythopoetic and modern "modes of time" may contribute to the bearing his ideas and goals have upon the challenges, which face attempts to go beyond the nature-culture dichotomy in anthropology's key areas of origins research

References

Berlin, I. 1976. *Vico and Herder. Two Studies in the History of Ideas*. New York: Viking Press.

Descola, P. and Pálssen, G. (eds.) 1996. *Nature and Society. Anthropological Perspectives*. London: Routledge.

Funkenstein, A. 1986. *Theology and the Scientific Imagination*. Princeton: Princeton University Press.

Hornborg, A. 1996. Ecology as Semiotics. In *Nature and Society*, eds. P. Descola and G. Pálssen. London: Routledge.

Ingold, T. (ed.) 1996. *Key Debates in Anthropology*. London: Routledge.

Lévi-Strauss. C. 1966. *The Savage Mind*, tr. anonymous. London: Weidenfeld and Nicholson.

Mali, J. 1992. *The Rehabilitation of Myth*. Vico's New Science. Cambridge: Cambridge University Press.

Mooney M. 1985. *Vico in the Tradition of Rhetoric*. New Jersey: Princeton University Press.

Renfrew, C. and Bahn, P. 1994. *Archaeology. Theory, Methods, Practice*. London: Thames and Hudson.

Sahlins, M. 1976. *Culture and Practical Reason*. London: University of Chicago Press.

Vico, G. 1948. [1744]] *The New Science of the Common Nature of the Nations of Giambattista Vico*, 3rd edition, tr. T. Goddard Bergin and M.H. Fisch. London: Cornell University Press.

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The "Entbürgerlichungskampagne" in Psychology in the GDR at the End of the 1950s

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Summary

In this paper I will discuss the so-called *Entbügerlichungskampagne* in the academic psychology in the GDR at the end of the 50s, meaning "supplanting bourgeois psychologists from the Institutes of Psychology". Apart from describing an interesting period of the history of psychology in the former GDR I will present a further example how politics of the Socialist Unity Party of Germany (SED) influenced the development of psychology. Firstly, I am going to talk about the efforts and the problems of the SED to increase its influence at the university system until the end of the 50s in general. Secondly, I will describe the policy with respect to the so-called bourgeois intellectuals which started after the Third Conference of Universities in 1958, held by the SED took place. And last but not least, taking one case from the history of psychology I want to show how the policy of the SED with respect to the "bourgeois psychologists" was carried out.

In this paper I will discuss the so-called "Entbürgerlichung" in the academic psychology in the GDR at the end of the 50ies, meaning "supplanting bourgeois psychologists from the Institutes of Psychology". Apart from describing an interesting historical period of the history of psychology in the former GDR I have chosen this topic to give a further example how politics influence the development of a science.

First of all, I have to emphasise that the term "Entbürgerlichung" as well as the phrase "supplanting bourgeois scientists from the universities" are explanations given by contemporary historians (Jessen, 1994 and Ash, 1995). Both the term "Entbürgerlichung" and the phrase "supplanting of bourgeois scientists from the universities" were never used in official documents or statements issued by public institutions at the time. For example, the document mainly used by contemporary historians to prove the existence of the campaign "Entbürgerlichung" is the "Decree about the further Development of the Universities and Vocational Colleges" validated by the Council of Ministers (Ministerrat) on February 13th, 1958. But this decree does not include either the term "Entbürgerlichung" or the phrase "supplanting bourgeois scientists". The decree includes demands to the State Office of the Permanent Secretary for Universities (Staatssekretariat der Hochschulen) which, from a historical point of view, can be interpreted as the basic means to the supplant bourgeois scientists from universities. To illustrate this point the following demands are mentioned:

- the State Office of the Permanent Secretary for Universities must ensure that the number of working class scientists continues to increase at universities;
- in addition, it must guarantee that all institutions in the GDR will inform the Central Committee of the Socialist Unity Party of Germany (SED) about persons working in industry but who are qualified to work at a university;
- it must ensure that the appointment of scientists to professorships has to take into account the interests of the so-called "Arbeiter-und -Bauernmacht" (literally worker´s and peasant´s power), which meant, in fact, that the scientists should be members of the SED (Baske & Engelbert, 1966, 353pp).

When taken seriously, it is obvious that the main goal of such demands was to increase the influence of the Socialist Unity Party of Germany at universities by filling positions with scientists who were members of the SED. Furthermore, the traditional freedom associated with research and teaching at the universities was threatened not only by the demands already mentioned but by the following ones which were also made in the "Decree about the further Development of the Universities and Vocational Colleges" 1958:

- the State Office of the Permanent Secretary for Universities must plan and control all research activities at universities;

- it has to supervise all scientific congresses taking place in the GDR as well as to guarantee that only "certain chosen" scientists will represent the GDR at congresses taking place abroad (Baske & Engelbert, 1966, 353pp).

One may wonder, why the efforts of the SED to increase its influence at the university system occurred so late when compared with much earlier efforts successfully undertaken in areas like industry or agriculture? There are two possible reasons for this delay:

Firstly, we have to keep in mind, that increasing the SED`s influence was always based on boosting the number of scientists at universities who were members of the party. It is possible to prove that as early as 1951, when the Second Reform of the Universities took place, the SED was making several efforts to extend its power. So, for instance in that year the government of the GDR passed a decree demanding that all universities be supervised by a central institution namely the State Office of the Permanent Secretary for Universities (from 1958 onwards it was named State Office of the Permanent Secretary for Universities and Vocational Colleges). The preceding paragraphs containing references to the 1958 decrees issued by the Council of Ministers (Ministerrat) make clear that the Office´s activities grew from the early 50ies on.

In 1952 the Central Committee of the SED also founded a Department for Sciences (mainly pushed by Kurt Hager) in which comrades of the SED represented scientific disciplines (from 1955 to 1989 the Department of Science was run by Johannes Hörnig!). However, the main goal of the Central Committee of the SED until the end of the fifties to establish and organise basic party units at the universities by sending so-called "Party-Workers" failed. This was due to the fact that there was an insufficient number of scientists who were also party members to establish such units. The explanation for the small number is clearly the strictly limited number of approved SED party memberships available to intellectuals. At this time hostility from mostly working-class members of the SED towards the so-called "old intellectuals" continued. To be "old intellectuals" refers to those who had begun their scientific careers before the GDR was founded in 1949.

A second restraint upon the SED`s power was the fact that it was not until the end of the 50ies that the so-called "new intellectuals" were qualified enough to assume academic positions. Most had started their career at the "workers and peasants faculties" which since 1949 had provided an alternative route for those, mainly working-class, students who had not gained the Abitur at school. Most scientists of these "new intellectuals" were of course members of the SED and therefore the number of comrades at universities gradually increased.

Overcoming the two above mentioned problems (the lack of "new" intellectuals and SED-members) led to a new type of policy with respect to the "old" intellectuals. Up to this time, relation between the members of the SED and the "old" intellectuals was based on an alliance, (more or less of) respect and support. This policy changed after the *Third Conference of Universities in 1958* held by the SED took place. At this conference the goal to fill positions at universities with "politically – correct" scientists was officially announced (1958 decree).

As a result, the term "bürgerlich" (literally bourgeois) became a "political term". As a "bourgeois scientist" persons could be categorised: firstly, those who lived in the western world; secondly, those who belonged to the "old" intellectuals in the GDR and thirdly, those who belonged to the so-called "new" intellectuals but were not seen as "real" comrades by the SED. In general the term "bourgeois scientists" simply was used by the SED to classify scientists as persons who were supposed not to support the development of the socialist society and therefore not to support the policy of the SED.

The SED used several strategies to carry out the new policy: firstly, the main strategy was based on the natural attrition and that is after emeritus status was given to a professor the position was filled by a scientist who was a member of the SED or, who, at least publicly supported the policy of the SED. Secondly, there are cases known to us in which public discussions with scientists classified as "bourgeois" were initiated by the SED in order to not only convince people that its policy was on the "right" track but, additionally, to intimidate them (for example the discussions with the philosopher Bloch at the University of Leipzig).

A further strategy used was to weaken the authority of "bourgeois scientists". A precondition necessary of this strategy was the changed organisation of universities in the GDR. As we mentioned before, the universities were controlled and influenced by both the State Office of the

Permanent Secretary for Universities and the Department of Sciences at the Central Committee of the SED. So, for instance, the State Office of the Permanent Secretary for Universities decided how many students would be trained at every institute, it decided each institute´s budget and the establishment of positions.

Taking one case from history I want to show how the strategy to weaken the position of a "bourgeois scientist" was carried out. But before going into details I want to discuss briefly the situation of psychology in general in the GDR at the end of the fifties.

Until 1960 only the institutes of psychology at the universities in Berlin, Dresden and Leipzig trained students in psychology (the total number of students who began studying psychology in the GDR were 16 in 1955, 49 in 1957, 48 in 1958, 53 in 1959 and 56 in 1960). At the same time four other departments of psychology existed at the universities in Halle, Rostock, Berlin and Jena which belonged to the Institutes of Education. These departments exclusively trained students of education in the subject of psychology.

What can be said about the "bourgeois scientists" at the institutes of psychology? The psychologist Maeder, then representative of the Central Committee of the SED´s Department for Sciences, responsible for psychology, stated in 1958 in relation to the situation of psychology:

> "The situation of psychology in the GDR is characterised by the Institutes of Psychology being run by bourgeois professors, who are representatives of a bourgeois psychology. Hence, the students and the new academic generation - including our comrades - are influenced by bourgeois psychology. In addition, they are controlled by the bourgeois directors in both examinations and scientific work" (SAPM. Sign.: N2/ 9.04/217. p.55, signed by Maeder in 1958).

There is evidence that since 1957 the representatives of the Department of Sciences of the Central Committee of the SED and the members of the Scientific Council of Psychology at the State Office of the Permanent Secretary for Universities planned to increase the influence of the new generation of scientists and thereby enhance the influence of the SED in psychology. One strategy used to weaken the position of the so-called bourgeois psychologists was to establish basic party units at the institutes and departments of psychology. This strategy was carried out successfully at the Institute of Psychology at the University of Leipzig, then run by Fischel. At the institute of Dresden a basic party unit was also established. Both in Leipzig and in Dresden the directors of the institutes did not prevent these developments. Contrary to this, efforts to form a basic party unit at the Institute of Psychology in Berlin failed. Gottschaldt vehemently barred members of the SED from establishing a basic party unit. His protest was based on his attitude that science and politics should be separated rather than opposition to the policy of the SED in general.

The second strategy to weaken the position of bourgeois psychologist by the representatives of the official institutions was based on the plan to enlarge and to redistribute the existing resources of psychology in the GDR.

To enlarge the resources meant to open a further institute of psychology were exclusively students in psychology should be trained. This plan was realised in 1960 when the Institute of Psychology at the University in Jena was re-opened (the Institute of Psychology was founded in 1923 by Wilhelm Peters and subsequently lost its autonomy in 1954 being changed into a department within the Institute of Education). By opening a further institute two goals were achieved: Firstly, to increase the number of students of psychology and secondly, to establish a socialist institute of psychology.

To redistribute the resources of psychology meant that the Institute of Psychology at the Humboldt University of Berlin (HUB), then run by Gottschaldt, needed to be relegated to a small research institute. It is on this strategy that I will focus:

I have proof that the plan to relegate one of the biggest and most famous Institute of Psychology to a small research institute was at first pushed by the university unit of the SED at the HUB. In a so-called "strict confidential notice" written by the basic party unit of the whole university of Berlin following information was found (the notice is dated on March, 15th, 1960):

"In the future it is planned that the Department of Psychology (in the Faculty of Education) has to develop a teaching program, which ensures that students of psychology can be trained within the Department. Prof. Gottschaldt shall keep his Institute but he will lose several positions so that in the end he will have just a small research institute" (Schmidt, 1992, p. 260).

One month later (on May, 5th, 1960) an informal meeting took place attended by representatives of the Central Committee of the SED (Department of Science), representatives of the Scientific Council of Psychology at the State Office of the Permanent Secretary for Universities, representatives of the basic party unit of the whole Humboldt University and representatives of the Department of Psychology of the Faculty of Education at the Humboldt University. Here the participants agreed that in future the Institute of Psychology, run by Gottschaldt, had to share its financial budget with the Department of Psychology of the Faculty of Education (SAMP. Sig. N2/9.04/216, p. 9, signed by Junge on May, 14th, 1960).

Two month later Gottschaldt must have been informed about the plans because at the meeting of the Faculty of Natural Sciences and Mathematics of the Humboldt University, which took place on June, 7th, 1960, he mentioned the following demands established by the representatives: first of all, the Institute of Psychology should stop training students of Education in the subject of psychology. These students should be exclusively trained by the Department of Psychology within the Faculty of Education. This demand was supported by Gottschaldt. The second demand was that the Institute of Psychology should also stop training general students of psychology. This demand was refused by Gottschaldt. Further, half of the library and technical resources of the Institute of Psychology should be handed over to the Department of Psychology within the Faculty of Education. The latter demand was refused by Gottschaldt as well. Gottschaldt asked the members of the Faculty of Natural Sciences and Mathematics to form a commission which would negotiate with members of the Faculty of Education. This request was accepted and a commission was founded. From the documents it can be assumed that the members of the Faculty of Natural Sciences and Mathematics did not know everything about the plan to redistribute the resources of psychology at the Humboldt University.

No evidence was found that the commission established by the Faculty of Natural Sciences and Mathematics ever negotiated with members of the Faculty of Education. Perhaps it was not seen as necessary because the idea to redistribute the resources was not carried out in the same way as was planned. The institute went on to train students in psychology and it shared neither its financial budget nor the library or technical resources with the Department of Psychology within the Faculty of Education.

Did the plan to relegate the Institute of Psychology to a small research institute fail in the end? This question cannot be answered by a clear yes or no. The plan did fail in a way, that the institute was not relegated to a small research institute. However, it succeeded in the sense that for instance, the Scientific Council of Psychology at the State Office of the Permanent Secretary for Universities did not allow to increase the number of students of psychology as at the Institutes of Leipzig, Dresden or Jena. In addition, further positions for scientists at the institute in Berlin declined and it can be assumed that the financial budget was reduced as well.

Was the goal to weaken the position of Gottschaldt achieved? To this question two answers can be given. Focusing on his position at the Institute of Psychology in Berlin the strategy that was used, failed. This interpretation can be supported by a letter signed by almost all colleagues of Gottschaldt which was addressed to the Scientific Council of Psychology at the State Office of the Permanent Secretary for Universities, the Central Committee of the SED, the rector of the Humboldt University, the Faculty of Natural Sciences and Mathematics of the Humboldt University and the basic party unit of the Humboldt University. In this letter Gottschaldt's colleagues stressed their belief in the quality of his scientific work and they expressed their lack of understanding that both the number of students and the number of positions were reduced by the Scientific Council of Psychology at the State Office of the Permanent Secretary for Universities.

Focusing on Gottschaldt's position as a psychologist in the GDR it can be said, that the strategy succeeded. Firstly, Gottschaldt no longer took part in congresses or meetings of the national

scientific community in the GDR. Secondly, we assume that the strategy used to weaken his position made it easier for him to decide in favour of taking the offered chair at Göttingen in 1961.

After informing the Humboldt University about his decision the position as head of the Institute of Psychology was taken away from him. We know that in spring 1962 Gottschaldt left the GDR but we do not have any information about how he left the country.

The Institute of Psychology in Berlin was then run by three "new scientists" namely Klix (Jena), Hiebsch (Leipzig) and Rosenfeld (Department of Psychology in Berlin). One year later, in spring 1962, the decision was taken by the Department of Sciences at the Central Committee of the SED to appoint Klix to the University of Berlin. After Klix became head of the institute the Scientific Council of Psychology at the State Office of the Permanent Secretary for Universities gave permission to increase the number of students, the number of positions, as well as the financial budget of the Institute. Already one year later, in 1963, the number of students at the institute was higher than it had ever been before.

Conclusion:

After the influence of the SED increased at the universities which was politically linked to increasing the number of so-called "new" intellectuals, basic conditions were developed for changing the policy with respect to the so-called "old" intellectuals in the GDR. The term "bourgeois" became a political term at this time and was used by the SED to classify scientists who were supposed not to support the socialist idea. As result of this political development the campaign "Entbürgerlichung" at universities was started even though the goal was never named in public. As we have mentioned, different strategies were used to carry out the supplanting of bourgeois scientists from universities. In many cases the goal was achieved when scientists left the GDR. Therefore, it is not surprising that the number of scientists who left the GDR at the end of the fifties and at the beginning of the sixties was much higher than the number of other professionals. Describing the process of the "Entbürgerlichung" at the universities does not examine the consequences both for the sciences in particular and for the universities in general. Addressing this question must wait for another day.

References

Ash, M. G. (1995c). Übertragungsschwierigkeiten: Kurt Gottschaldt und die Psychologie in der Sowjetischen Besatzungszone und in der Deutschen Demokratischen Republik. In Jaeger, S., Staeuble, I., Sprung, L. und Brauns, H.-P. (Hrsg.), *Psychologie im soziokulturellen Wandel - Kontinuitäten und Diskontinuitäten* (S. 286-294). Frankfurt am Main: Peter Lang Verlag.

Baske, S. & Engelbert, M. (1966). *Zwei Jahrzehnte Bildungspolitik in der Sowjetzone Deutschlands.* Heidelberg: Quelle & Meyer Verlag. Band 1.

Jessen, R. (1994). Professoren im Sozialismus. Aspekte des Strukturwandels der Hochschullehrerschaft in der Ulbricht-Ära. In: Kaelbler, H., Kocka, J. & Zwahr, H. (Hrsg.) (1994). *Sozialgeschichte der DDR* (S.217-253). Stuttgart: Klett Cotta.

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

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1898: The Disaster and Spanish Psychology

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Summary

In Spanish history, 1898 is the year of a military defeat suffered in a war against the United States of America. More significantly, however, the date is also associated to the moral blow received by the loss of the country's last colonies, an event that was to become a symbol of the shortcomings of the political regime in vigour.

The Disaster of 1898, as it was soon referred to, gave rise to a kind of collective introspection carried out by those intellectuals who were by then initiating their public action. From very different viewpoints (literary, artistic, scientific), a considerable effort was made to analyze the causes of the decadence of the Spanish nation and to find ways leading to its regeneration.

In such an inquiry, psychological considerations played a fundamental role. They favored the emergence of a literature aiming at the description of the Spanish character or mind, in which - as was to be expected – authors linked to scientific psychology took a very active part.

The Year of the Disaster

1898 has passed into Spanish history as "the Year of the Disaster". It is now clear that the events of that year are a milestone in contemporary Spain, as their consequences have been felt in Spain in many ways throughout the 20th century (Fusi & Palafox, 1998).

By "The Disaster" reference is made to the defeat suffered by Spain in a war against the United States. The war broke out when the American cruiser *Maine* was blown up in the harbour of La Habana on the 15th of February, 1898. Although the causes of this explosion - where 264 people were killed - have never been made clear, the Spanish government was held responsible, and the United States forced a war the that suited the American expansionist interests. Between May and July of that year, the U.S. Navy destroyed the Spanish float before the Cuban coasts. In December of that year a peace treatise was signed in Paris whereby Spain abandoned Cuba and yielded Puerto Rico and the Philippines to the United States. Thus, Spain was reduced to the condition of a small nation with hardly any international influence, and with only a few possessions left in the North of Africa as a last trace of its past colonial splendour.

1898 and Restoration Spain

The defeat came as a hard blow to a country that seemed to be recovering from the social and political convulsions that had characterized its recent history. After a six-year revolutionary experience that had forced Queen Isabel II to go into exile in 1868, a military revolt in 1874 succeeded in reinstalling her son Alfonso XII into the throne. This marked the beginning of a new period in Spanish history which has ever since been known as the "Restoration age" (1875-1902).

As a result of the long period of peace and stability brought about by the new regime, Spanish life improved in a number of ways: There was a rise in living standards, the population grew, and the whole country underwent a remarkable cultural development whose highest and probably best-known expression is the work of Cajal.

However, the Restoration age was not free from shortcomings and inconsistencies that gave rise to many social and political problems. For one thing, its oligarchical regime prevented large sectors of the country from being truly represented in the Parliament. In addition, the slowness in the process of industrialization resulted in a backwardness of economy accounting for a number of internal problems, such as the growth of regionalism, the increasing violence of ideological confrontations, and the emergence of worker revolts. Such internal conflicts, in turn, compelled to adopt an attitude of external withdrawal, leaving the country at the mercy of the expansive tendencies of other nations, as the 1898 war against the United States clearly showed.

A time for reflection

Although the Disaster did not lead to any major political changes, it did cause a deep shock in national consciousness. As the poet Antonio Machado once wrote, Spaniards emerged from the defeat as the man who recovers from a hard blow and, still dazed, exclaims: "Where am I?".

An intense reflection on the "problem of Spain" was then initiated. It consisted in an examination of the causes of Spanish decadence as well as an assessment of the means that may lead to its "regeneration" (to use a then popular term). Politicians, intellectuals, scientists and artists of every sector of Spanish society took an active part in this wide movement of "*prise de conscience*". As a result, the various cultural tendencies of the country received an extraordinary impulse that spread their influence well into the 20th century.

Particularly relevant was the role played by a young group of writers who became first known about the year of the Disaster. Born between 1865 and 1875, their early work represents a powerful reaction agaist the political and social contradictions of the Restoration age, of which the disastrous results of the war against the United States had become a symbol. The philosopher Miguel de Unamuno (1864-1936), the novelist Pío Baroja (1872-1957), the poet Antonio Machado (1875-1939), and the journalists and essay writers Ángel Ganivet (1865-1898), Ramiro de Maeztu (1874-1936), and José Martínez Ruiz ("Azorín") (1873-1967) are probably the most representative names of this "Generation of 1898", as the group has usually been labelled (Franco, 1980).

All of them having a strong individual and literary personality, the work of any one member of this group widely differs from that of the rest. However, a number of common features conferring some unity to their outlook have been usually pointed out: Their bitter love for Spain, a deeply pesimistic view of Spanish political, economic and cultural condition, a praise of Castille (its landscape, its people, its literature, its history) as representative of the true Spanish "essence", and their dream of a new and better Spain, are among the most frequently mentioned (Laín, 1945).

Psychological concerns

In this meditation on the essence and meaning of "Spanishness" - which, as noted, is one of the characteristics of the work of this "Generation of 1898" - a central role was played by an interest in the psychological constitution of Spaniards. To establish the features defining Spanish mentality or mind was seen as a necessary step towards a proper understanding of the decadent condition of the Spanish nation, as well as a path leading to the discovery of its likely remedies.

Only a few years before the Disaster took place, two extraordinarily significant books were published by Miguel de Unamuno (*En torno al casticismo*, 1895) and Ángel *Ganivet(Ideárium español*, 1897). To a certain extent, these two essays marked the starting point of a literature on the "problem of Spain" emerging at the turn of the century, and they succeeded in conditioning subsequent approaches to the subject.

The former was a reflection on national values, where an inquiry on the essential, everlasting features –"intrahistorical", in Unamuno's terms, as opposed to historical or transitory qualities - of Spanish people was carried out. Consistently with the belief - widely shared by the members of the generation – in the essential role played by Castille in the shaping of Spanish nation, Unamuno's focus was on Castillian personality, which he described as sober, sly, monotonous, slow in thinking and uniform in ideas.

Ganivet's book, on the other hand, provides an interpretation of Spanish character on the basis of the influence exerted by the Arabian invasion (in the 8th century) and the subsequent eight-century striving for reconquering the country. Mysticism, fanatism, stoicism, individualism and spirit of independence are, according to Ganivet, some of the most salient features of Spanish mind.

The conclusions reached by these two works are notably different. While for Unamuno a national regeneration will not be possible unless Spain opens its mind to modern European cultural tendencies, Ganivet rather suggests to beware of foreign influences, and to concentrate instead in the development of purely national values. However, both authors agree in seeing "the Spanish problem" as an essentially *psychological* problem. They both believe Spanish society to be suffering from a mental or spiritual crisis, which Unamuno labels as a "dissociative tendency", whereas Ganivet, evidencing Ribot's influence, calls "abulia" (Fox, 1997).

In the following years, other writers of this "Generation" made significant contributions to the analysis of Spanish national consciousness initiated by Unamuno and Ganivet. In 1900, for instance,

a book significantly entitled *El alma castellana [The Castillian Mind]* was published by the journalist and critic "Azorín" (José Martínez Ruiz). In this book, "Azorin" provided a literary recreation of Spanish decadence as expressed by some characteristic 17th century psychological types: the *hidalgo* [nobleman], the *pícaro* [rogue], the beggar and the galley slave. A few years later, in his *Campos de Castilla [Castillian Fields]* (1912), Antonio Machado gave poetical form to his concern with the decadence and the mind of Spanish people, some of whose defects (such as superstition and false religiosity) he also exposed.

Now, this concern for the psychology of Spanish people in search of some impulse for its regeneration was not limited to literary writers. It was rather a concern widely shared by all members of the generation (giving the term a somewhat wider meaning than its usual literary sense). In some oils of the painter Ignacio Zuloaga (1870-1945), for instance, the image of Castille becomes a symbol of the sad, decadent Spain that ought to be left behind (Lafuente Ferrari, 1990). Indeed, these paintings are mean to be expressive of Spanish character, and represent an as efficient a call towards the renewal of the country as any writing might have been.

The scientific approach to this issue is probably best exemplified by the *book Psicología del pueblo español* (1902) [*Psychology of Spanish People*], by Rafael Altamira (1866-1951), the most important late 19th century Spanish historian. Altamira maintained the existence of a characteristic Spanish mentality, i.e. a community of interests, ideas and affections underlying individual and regional differences and making of Spaniards a peculiar psychological type. A disciple of Giner de los Ríos, he conceived the regeneration of Spain in educational and cultural terms that should be adjusted to the basic psychological traits of its people.

Almost half a century later, in an entirely different historical context, a new attempt was made by Ramón Menéndez Pidal (1869-1968), another eminent historian, to define national character on the basis of Spanish history and literature. Sobriety, desinterestedness, humanitarianism, idealism, religiosity, and individualism were some of the features emphasized (Menéndez Pidal, 1947). Menéndez Pidal's contribution illustrates the lasting centrality this issue had in the men of his generation.

Scientific psychology and the Spanish mind

But, what about psychologists proper? What was their stand on an issue that, as we are trying to show, seemed to be shared by all members of Spanish *fin de siècle* generation?

It must be born in mind that, in Spain, scientific psychology was still far from being a socially acknowledged investigative and professional activity. Although a huge task of spreading modern psychological ideas through a variety of translations was most efficiently carried out by intellectuals and publishing houses during the last quarter of the century (Quintana, Rosa, Huertas & Blanco, 1998), Spain still lacked specific institutions capable of promotig the scientific development of psychology. In 1902 a Chair for Experimental Psychology was created at the University of Madrid, but its overall influence in Spanish university life was very scarce (Carpintero, 1987).

It was rather in secondary education settings that, in small laboratories endowed with a few experimental tools, some scientific research in psychology began to emerge. This was possible thanks to the existence of a subject on "Psychology, Ethics and Logic" ascribed to the chairs of Philosophy (Blanco, 1998). At the same time, in the early years of the new century, a number of handbooks – such as those by Verdes Montenegro (1903) and Herrero Bahillo (1911)- showed the effort being made by some high school teachers in order to update their teaching and adjust it to the most advanced psychological tendencies of the day (Carpintero, 1994).

Particularly relevant in this context were the contributions of two Philosophy teachers in secondary education centres, Martín Navarro Flores (1872-1950) and Eloy Luis André (1876-1935), whose works show a good acquaintance with the scientific psychology of the time. They provide a good illustration of two different ways of approaching, from a scientific psychological viewpoint, the problem of Spanish mind that had centered the interest of their generation.

Martín Navarro Flores was the author of the earliest *Handbook of Experimental Psychology* ever published in Spain with such a title (Navarro, 1914). It was an innovative didactical introduction to the problems and methods of experimental psychology, where established knowledge in the field was clearly and successfully systematized. The *Handbook* provided minute descriptions of the experimental procedures used in the various areas of psychological research (sensation, perception,

image formation, memory, feeling, will, and intelligence), and included a list of instruments, journals and bibliographic sources for further studying (Tous, 1984; Sanz Oró, 1984).

Navarro insisted in the need of replicating in Spain the experiments carried out abroad, as a means of obtaining scientific evidence on the real nature of Spanish mind. "It must be admitted – he argued – that there will be no possibility of developing a truly European psychology, not to mention a universal psychology, unless one of its chapters – perhaps one of the most interesting – is devoted to the mind of our country. And the duty and honour of writing such a chapter cannot be taken away from Spaniards themselves" (Navarro, 1914, xvi). The question on the nature of Spanish mind, then, was here posed from an experimental perspective. It was, however, the same issue that was being raised from other intellectual and artistic points of view by many of Navarro's contemporaries.

Eloy Luis André received some training under Wundt in 1910. Within the program on cultural or ethnic studies being developed in Leipzig at the time(*Völkerpsychologie*), he carried out an investigation on "The melody of speech in different languages and dialects". Luis was very much interested in national mentalities, and spent some time studying German collective psychology. In his books *La mentalidad alemana [German Mentality]* (1914) *and La cultura alemana [German culture]* (1916), the character of German people was approached from their educational, philosophical, historical and scientific structures.

Also from this perspective, Luis dealt with the "problem of Spain", and in several of his books he showed his concern with the psychological and ethical caracteristics of Spanish people: *El histrionismo español [Spanish histrionism]* (1906), *Ética española [Spanish Ethics]* (1910); Españolismo, *[Spanishness]* (1931) and *Revolución [Revolution]* (1933). Thus, in his work, interest in Spanish mind and regeneration merges with a Wundtian training, which allows him to face the issue with the scientific instruments provided by his master's *Völkerpsychologie*.

Conclusions

To sum up, in Spanish history, 1898 is the year of a military defeat suffered in a war against the United States of America. More significantly, however, the date is also associated to the moral blow received by the loss of the country's last colonies, an event that was to become a symbol of the shortcomings of the political regime in vigour.

The Disaster of 1898, as it was soon referred to, gave rise to a kind of collective introspection carried out by those intellectuals who were by then initiating their public action. From very different viewpoints (literary, artistic, scientific), a considerable effort was made to analyze the causes of the decadence of the Spanish nation and to find ways leading to its regeneration.

In such an inquiry, psychological considerations played a fundamental role. They favored the emergence of a literature aiming at the description of the Spanish character or mind, in which - as was to be expected – authors linked to scientific psychology took a very active part.

References

"Azorín" [Martínez Ruiz, J.] (1900), *El alma castellana*. In *Obras Completas*, I. Madrid: Aguilar, 1947.

Blanco, F., Castro, J. & Castro, R. (1996), Eloy Luis André (1876-1935). In M. Saiz & D. Saiz (Eds.), *Personajes para una historia de la psicología en España*. Madrid: Pirámide.

Blanco, F. (1998) (Ed.), *Historia de la psicología española desde una perspectiva socio-institucional*. Madrid: Biblioteca Nueva.

Carpintero, H. (1987), El Dr. Simarro y la psicología científica en España. In J.J. Campos & R. Llavona (Eds.), *Los orígenes de la psicología científica en España. Investigaciones Psicológicas, 4*, 189-207.

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

Franco, D. (1980), España como preocupación. Barcelona: Argos Vergara.

Fox, I. (1997), *La invención de España*. Madrid: Cátedra.

Franco, D. (1980), *España como preocupación*. Barcelona: Argos Vergara.

Fusi, J.P. & Palafox, J. (1998), España: 1808-1996. El desafío de la modernidad (3rd ed). Madrid: Espasa Calpe.

Ganivet, A. (1897/1981), *Ideárium español* (11th ed.). Madrid: Espasa Calpe.

Herrero Bahíllo, F. (1911/1917), *Nociones de psicología moderna* (2nd ed.). Avila: Tip. Sucesores de A. Jiménez.
Lafuente Ferrari, E. (1990), *La vida y el arte de Ignacio Zuloaga* (3rd ed.). Barcelona: Planeta.
Laín, P. (1945), *La generación del 98*. Madrid: Imp. Diana.
Luis André, E. (1906), *El histrionismo español. Ensayo de psicología política*. Barcelona: Imp. Henrich y Cia.
Luis André, E. (1910), *Ëtica española*. Madrid: Rivadeneyra.
Luis André, E. (1931), *Españolismo*. Madrid: Rivadeneyra.
Luis André, E. (1933), *Revolución*. Imp. Murillo.
Machado, A. (1912), *Campos de Castilla*. In Poesías Completas. Madrid: Espasa Calpe, 1974 (15th ed.)
Navarro Flores, M. (1914), Manual de Psicología Experimental. Tarragona: Imp. José Pijoán.
Quintana, J., Rosa, A., Huertas, J.A. & Blanco, F. (Eds.) (1998), *La incorporación de la psicología científica a la cultura española. Siete décadas de traducciones (1868-1936)*. Madrid: Universidad Autónoma.
Sanz Oró, R. (1984), *La psicología científica de Martín Navarro. Período tarraconense* (1905-1919)., 6, 25-41. *Universitas Tarraconensi, 6, 25-41.*
Tous, J.M. (1984), *El Manual de Psicología Experimental de Martín Navarro. Universitas Tarraconensis, 6*, 7-23.
Unamuno, M. (1895), *En torno al casticismo. In Obras Completas, I.* Madrid: Escelicer, 1966.
Verdes Montenegro, J. (1903), *Apuntes de psicología científica.* Alicante: Imp. Such.

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Methodological Standardization and the Origins of Modern Psychology

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Historical research in the development of modern psychology as an academic and practical discipline has demonstrated that, despite the enormity of its field of study, remarkable points of commonality are evidenced by many of the main strands of twentieth-century psychology. This unity can be usefully conceptualized as a crisis of identity in asmuch as the disparate nature of psychological research at the end of the nineteenth century gave rise to various competing theories at the beginning of the twentieth. Two such systems are Freud's psychoanalysis and Watson's behaviorism.

Comparison of these fields - so strikingly dissimilar in theory, practice, and the personalities of their leading figures - is fraught with research and theoretical difficulties. The very origins and purposes of these disciplines are so diverse that comparison might even seem foolhardy or useless. Where Freud drew much from Romantic conceptions of science, Watson's formulations were clearly Enligtenment-based. While Freud initially utilized a subjective metholodology to analyze the distant origins of sexuality (later aggressivity as well), Watson rejected such subjectivity as unscientific. Such striking and important differences are numerous. Yet when we recognize that "in the twentieth century, disciplines such as psychology...have been preoccupied, sometimes obsessed, with objectivity, a preoccupation which has most commonly taken the form of a search for objective research methods,"[1] we have found a useful point of departure. For paramount amongst all shared beliefs by Freud, Watson, and their immediate followers is that they were engaged in the construction of the scientific study of behavior (the "new psychology"). If we focus on the development and standardization of methodological practice, the contrasting elements of these movements (given their contemporaneity) can been seen as an aid to comparative analysis. This paper discusses the research and theoretical challenges and opportunities of such an endeavor.

These observations, then, constitute the starting point for such research; they do not, however, address the issue of how one should write such a history. When Thomas Kuhn published The structure of scientific revolutions in 1961 he complained that the modern conception of science was altogether non-historical - even mythical. He furthermore suggested, and proceeded to demonstrate, that the historical record could validate a rather different understanding of how science works. In the quarter-century since Kuhn's work appeared, historians of science have increasingly problemetized our understanding of the origins and dynamics of scientific practice and discourse. Mark Micale, for example, recently argued that the very same paradigms Kuhn identified decisively influence the writing of historical narratives about scientific disciplines.[2] Although there are those who think differently,[3] by and large virtually all historians and a great many psychologists recognize the necessity of historicization and the pivotal role of the narrator in the construction of histories of psychology.

Although the precise nature of society's influence on scientific work remains a topic of (sometimes heated) debate, science is undeniably a part of our culture possessing an identifiable dynamic. This raises important questions for the historian of psychology. What are the prime determinants of the relationship of the practicing psychologist to his or her field? How does work in these disciplines maintain cohesion and achieve the goal of producing useful, accurate knowledge? How can and should an historian address these concerns? Fundamentally, in what manner and to what degree were the standardization of method and technique in these fields shaped by their reception by colleagues, by the idiosyncrasies of the major contributing personalities, and by the process of institutionalization; how does a comparative examination of these processes illuminate the social dynamic and the quest for psychological truth.

What do we mean, however, when we refer to "the standardization of methodology"? Defining such terms is not merely a matter of semantics. Its importance stems in no small part from the supposition that standards apply to science and that progress is an integral part of that endeavor. These issues have long been particularly important in psychology on account of its historic lack of

intellectual and institutional center.[4] The imposition of standards on particular activities (including science) is directly related to the perception of progress in those arenas as being a product of creative energies and utilitarian results. As Kuhn pointed out, the recognition of the creative act as a contribution helps to define progress and validates the label scientific. Here I rely on the thinking of Ludwik Fleck who more than sixty years ago asserted that

> we can define a scientific fact as *a thought-stylized conceptual relation which can be investigated from the point of view of history and from that of psychology, both individual and collective, but which cannot be substantively reconstructed in toto simply from these points of view.*[5]

Fleck's balanced (and qualified) reliance on both history and psychology as tools with which to analyze and understand science is noteworthy and useful. Additionally, the process of standardization in most psychological circles was possible only due to the availability of and an increasing emphasis on the measurement of psychological data.[6] This phenomenon, which began in earnest at the end of the nineteenth century, was part of the statisticalization of modern scientific and public life. Behaviorist research clearly relies heavily on statistical analysis, but psychoanalysis too was also influenced by this trend (recall Jung's association experiments).

Adopted from a number of sources, the term *methodology* refers to those sets of rules and guidelines used by practitioners to regulate inquiry. They need not always be generally agreed-upon, but they must conform either to traditional methods or be recognized as a new attempt to set such methods. The *standardization* of these methods, as I use it here, refers to the establishment of commonly recognized fields of research and modes of inquiry. More precisely, it comprises the exclusion of specific types of research and modes of inquiry.

This process is by nature historical and necessitates investigation of group and individual dynamics. Whiggish (e.g., teleological) constructions of the history of psychology tend - by virtue of ease or ignorance - to focus on individuals and their contributions. Research into the actions and positions of individuals naturally constitute a central part of my inquiry, but Thomas Kuhn gave cogent voice to the importance of examining group dynamics:

> What does the group collectively see as its goals; what deviations, individual or collective, will it tolerate; and how does it control the impermissible aberration? ... Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it.[7]

This is most certainly the case when examining the establishment of methodological standards.

My choice of psychoanalytic and behaviorist schools of psychology demands justification. Is there no [illegible] in comparing two theories so diverse in origin, style, and perhaps most importantly, methodology? Freud [illegible] to be generally dismissive of psychologies, usually viewing them as competition. Psychoanalytic theory [illegible] for observation of many sorts of behavior including body language and, on account of the long duration of analyses, repeated trials of specific behavior.[8] These are naturally not the same as behaviorist experiments, [illegible] is interesting to note that this was not a one-way exchange. Watson's reception of psychoanalysis was [illegible] friendly and in fact it has been argued that Watson's description of Little Albert's fear reaction in his 1920 [illegible] "Conditioned Emotional Reactions" as "a transfer" is a term derived directly from Freud's concept of transference.[9]

This interest was not confined to the pioneer generation as scholarly output on the convergence of these theories continues to grow.[10] Historical perspective on these issues remains essential, for while these approaches may differ in method they seek equally to understand who and what we are, one through introspection the other through statistical measurement. The parallels in theory and the social processes by which methodological techniques were standardized shed light on the meaning of psychology and on the very nature of science itself. Yet the choice of these fields has been made to accentuate the differences in approach and method.

By examining the development of methodological standards in the period 1890-1940 variables in geography and subject matter permit elucidation of those factors responsible for their establishment.

The historical origins of these differences reach back to the eighteenth century when organic and mentalist conceptions of the mind were hotly debated. Behaviorist thinking decidedly rejected Romantic notions of self and its relation to society. This is certainly not the case with Freud.[11] The comparative analysis of the historical origins of these two influential branches of psychology provides as well a field upon which their methodological standardization can be clarified and contextualized. The case for comparative analysis was recently argued forcefully by Kurt Danziger: "The historiography of psychology has gained immeasurably by recent tendencies to investigate the specific social and institutional contexts with which psychologists had to operate." He further argues that

> if one aims at an analytical, rather than an essentially narrative, account of these historical relationships, a comparative perspective must eventually be adopted. ...there may be fundamental aspects of historical situations that are seen to be problematic only when one examines them in the light of comparable situations elsewhere.[12]

Solved problems, in contrast to new techniques of manipulation or interpretation, are crucial - though not fully sufficient in themselves - to the imposition of standards. Progress in psychology and history depends on the rigorous establishment and application of standardized technique and method. Yet as Ted Porter has pointed out methods, too, have an element of conventionality.[13] This work is designed to identify and explain precisely these conventions, to engage in the debunking of historical myths,[14] and thereby illuminate the striking similarities in the social construction of our disciplines.

In his 1968 article "On the Origins of Behaviorism"[15] John Burnham observed that our "understanding of the significance of behaviorism in the history of psychology - and the behavioral sciences - will be incomplete until the origins of the movement are viewed in a fresh way." This research endeavor is designed, in part, to meet that agenda and to challenge through comparative historical analysis our understanding of these important movements.

Notes

1 R. Smith, The Human Sciences (Norton: London, 1997), p. 15.

2 Mark S. Micale, "Paradigm and ideology in psychiatric history writing" *The Journal of Nervous and Mental Disease* 184 (1996):3, 146-152.

3 Hans van Rappard, "On historical continuity." Presentation at the European Society for the History of the Human Sciences, Szeged, Hungary, 29 August - 2 September 1997.

4 Mitchell Ash, "Historicizing mind science: discourse, practice, subjectivity" *Science in context* 5 (1992), pp. 193-207.

5 Ludwik Fleck, Genesis and Development of a Scientific Fact (London & Chicago: U. Chicago Press, 1981. Orig. Entstehung und Entwicklung einer wissenschaftlichen Tatsache (Basel: Benno Schwabe, 1935), p. 83. Emphasis in original.

6 See, e.g., German E. Berrios, The History of mental symptoms: descriptive psychopathology since the nineteenth century (Cambridge: Cambridge U. Press, 1996), p. 423.

7 T.S. Kuhn, The Structure of scientific revolutions (Chicago: U. of Chicago Press, 1962), p. 210. Such work has already begun in many fields, including the psychoanalytic. See P. Loewenberg, "The creation of a scientific community: the Burghölzli, 1902-1914." in Fantasy and reality in history (New York: Oxford U. Press, 1995), pp. 46-89.

8 A certain hostility is manifest, however, in psychoanalytic historiography. In Franz Alexander & Sheldon Selesnick's A history of psychiatry: an evaluation of psychiatric thought and practice from prehistoric times to the present (New York: Harper, 1966) Watson and behaviorism receive barely a mention.

9 Mark Rilling, "Cradling John Watson's behaviorism and Little Albert in the context of psychoanalysis and psychopathology." Presentation at the 29th annual meeting of the Cheiron Society, Richmond, VA. 19-22 June 1997.

10 See, for example, Paige Crosby Ouimette & Daniel N. Klein "Convergence of psychoanalytic and cognitive-behavioral theories of depression" in Psychoanalytic perspectives on psychopathology (Washington D.C.: American Psychological Association, 1993), pp. 191-223.

11 One recent example is Suzanne Kirschner's The religious and romantic origins of psychoanalysis (Cambridge, UK: Cambridge U. Press, 1996).

12 Kurt Danziger, "Social context and investigative practice in early twentieth-century psychology" Psychology in twentieth-century thought and society (Cambridge: Cambridge U. Press, 1987), p. 13.

13 Theodore Porter, Trust in numbers: the pursuit of objectivity in science and public life (Princeton, NJ: Princeton U. Press, 1995), p. 212.

14 See for example H. Ellenberger, "The Story of Anna O.: a critical review with new data." *Journal of the History of the behavioral sciences* 8 (1972): 3, pp. 267-279; and Dora B. Weiner, "Le geste de Pinel: The history of a psychiatric myth." in Discovering the history of psychiatry (New York: Oxford U. Press, 1994), pp. 232-247.

15 John C. Burnham, "On the Origins of Behaviorism" *Journal of the History of the Behavoral Sciences* 4 (1968): 143-151.

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Psychological Expertise and Social Practices

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One of the challenging tasks for historians and theorists of the social sciences is to understand the placement of these psychological experts in the sociopolitical arrangements of liberal democracies. For South African social scientists, the challenge is made more salient by the current attempts in all walks of society to come to terms with the political allegiances of the social sciences, and the problematics that such political entanglement entailed.

Foucault's notion of 'governmentality' or governmental rationality was taken as the major theoretical guiding principle. Foucault (and others, such as Nikolas Rose) have argued that the history of the 'psy' disciplines is intrinsically linked to the history of government. 'Government' is used here to refer to "a way of conceptualizing all those more or less rationalized programs, strategies, and tactics for 'the conduct of conduct' , for acting upon the actions of others in order to achieve certain ends" (Rose, 1996, p. 12). Colin Gordon (1991) indicates that Foucault understood 'government' in both a wide and narrow sense. The general use is reflected in the quote from Rose above: 'the government of one's self and of others'. The more specific sense is in terms of the 'rationality of government': "a way or system of thinking about the nature of the practice of government (who can govern; what governing is; what or who is governing), capable of making some form of that activity thinkable and practicable both to its practitioners and to those upon whom it is practised" (Gordon, 1991, p. 3).

An intriguing aspect of such an analysis is that South African society could not be described as a liberal democracy, but in some ways reflected a strong influence from an Anglo-American background. The question then is what would happen to psychological expertise in such a situation. Three major instances of psychological work in the South African context were identified in the period before 1950: the administration of intelligence tests to different groups; the measurement of race attitudes; and work on the "adaptability" of African mine workers.

Psychological tests

In the application of psychological tests, a powerful component of the appeal of science for these policy debates in South Africa was the representation of science as "neutral expertise". For these scientists, science was to provide objective solutions to social and political problems of the day. Leonard Barnes put it quite succinctly: that the "native question" would end in violence "unless Science, the `cool, gentle, serious spirit of science', science which alone embodies the maturity of the human mind, takes charge of the situation" (1930, p. 212). Thus science could move beyond the divisiveness of politics and contribute to improving social conditions. This was a view largely shared by the intelligence testers.

In cross-race comparisons, the differential performance of black and white powerfully reinforced the idea of a hierarchy of human societies, and as a consequence, treating them differently in terms of education, employment, etc. The tests were "objective" indicators of the way people were; here was evidence untainted by political considerations; "facts" which were not disputable on the basis of one's politics.

Through the psychological test, race became knowable in a particular way: it was positive knowledge. Here was a technique which produced quantitative information about the nature of mental attributes of black and white. It allowed a way of understanding, classifying and calculating (in Rose's terms) a problem of governing a social area.

Fleisch (1995) furthermore has argued that the real significance of such work lay in the way it strengthened the exemplar of the social scientist as policy maker. "Facts" were produced about a social problem, in terms of numbers, failure rates at school, intelligence scores, vocational aspirations, etc. On the basis of these facts, recommendations were made regarding the state's involvement in social problems, and the formulation of social policy: about education, racial segregation, employment, and much more. As a result, psychological testing too was promoted in a

similar fashion. Intelligence tests were introduced in white schools as a diagnostic tool to assign "the feebleminded" to special schools, and to channel pupils into different ability groups. By the end of the 1930s, intelligence testing was firmly established as an instrument of social planning in education for white pupils.

Gold mining and psychology

The psychological tests for the classification of migrant mine workers, and for the selection of African supervisors, took the concept of 'adaptabilityquote and made it into a capacity which an individual possessed. The tests were aimed at detecting how individual mineworkers possessed different abilities to adapt: e.g. the capacity to adjust to new circumstances, and the ability to profit from experience. Adaptability had become one single continuous dimension of the lives of African mineworkers along which any given individual's position could be established. The process by which this was established essentially was an objective one of calculation: the end result was a quantitaive score. This quantitative score could then be used as a basis to rationalize decisions about where a person should be employed, and whether he should be a supervisor. Adaptability had become calculable, and hence manageable.

The developments described here are local illustrations of the general trend in industrialized countries to manage on the basis of objective knowledge: "..part of a wider family of political programmes that sought to use scientific knowledge to advance national efficiency through making the most productive use of material and human resources" (Rose, 1990, p. 59). The efficiency of persons could be improved through the application of expertise, which gave management a rational basis and accorded it the capacity to eliminate waste and thus promote the national interest (again Rose).

Attitudes

I.D. MacCrone's work, particularly as expressed in his 1937 book, *Race attitudes in South Africa: Historical experimental and psychological studies*, reads like a shop-window for psychology. Three major disciplinary themes are interwoven: history; attitudes (in a very technical sense); and psychoanalysis.

MacCrone was a "lone voice" partly because many white people regarded difficult group relations in South Africa as "the native problem", which made questions about white people redundant (Louw & Foster, 1991). By the late 1920s was using the concept of a social "group' in a very up-to-date fashion. The groups he was interested in were of course "racial" groups - indeed, he often vacillated between the terms "race" and "group" for the make-up of the South African population. In terms of "governmentality", Rose has argued that the notion of a "group" made the social life of people into something that can be "known objectively and governed rationally" (1996, p. 118). In this process, the concept of "attitudes" made social actions of persons intelligible in a psychological way, via the various techniques of measurement. Although MacCrone's work was hardly applied in terms of "governing" - in terms of the administration of the population based on objective information - he certainly tried to provide information which would lead to an understanding of social attitudes. He tried to present his research into attitudes in terms of the "problem of the relation between white and black" (MacCrone, 1930, p. 591), and thus tried to encourage an understanding of social concerns via the expert knowledge of psychology.

Conclusions

In an article on "Objectives and Methods of African Psychological Research", published in 1958, Simon Biesheuvel sets out three objectives for psychology in the African context:

(a) "to gain an understanding of the behaviour of African peoples";
(b) "testing the general validity of psychological hypotheses concerning human behaviour" - i.e. cross-cultural psychology;
(c) "to determine the extent to which it (i.e. African behaviour) is modifiable".

Most of Biesheuvel's article is actually devoted to objective (c), and of course, this is what most of his own work at the NIPR was all about. He calls this objective "more practical" than the others.

In this approach one tries to demonstrate the effects of specific environmental variables (e.g. nutrition) on psychological functions. This kind of psychological research is precisely analogous to the agricultural research for which R.A. Fisher developed his statistical methods. In psychology, those who are the objects of this kind of research are treated as objects in precisely the same way as the plants in agricultural research. The ultimate aim is to act upon the conduct of others to increase work efficiency and output. Hence, this kind of psychological research attracted major industrial funding and, within its own terms, must be judged to have been reasonably successful.

Matters such as training, communication, evaluation and education are not "psychological issues". These issues emerge as problems within a particular kind of society, and as such, pre-date psychology. Nothing makes them intrinsically psychological issues: they were transformed into tasks for psychologists by the self-conscious efforts of a new group of knowledge experts. If we look at the work of the NIPR, it is clear that prior to its establishment some kind of selection and classification of African labour took place. For example, some classifications were done on a tribal basis: the Basotho were seen as superior shaft sinkers, and would be allocated that work.
By introducing the possibility that "adaptability to Western working conditions" could have been an important predictor of efficiency of African workers on the mines, psychologists made a new way of thinking about these workers possible. These workers were no longer one undifferentiated mass of people, who would be selected and trained on a trial-and-error basis: they were now individuals who could be assigned a position on a continuum of "adaptability", which was a strong indicator of how successful they would be in a variety of jobs on the mines. Psychology was the 'intellectual technology' which made this possible.

Biesheuvel's objective (b) never prospered in the same way. It was difficult to sell, both to industrial sponsors, and to mainstream American psychologists to whom it carried the implied threat of challenging the naturalness of psychological categories.

In a sense, Biesheuvel's objective (a), as exemplified by MacCrone, is the counterpart to objective (b). In the latter, observations in Africa are used to test the universality of psychological generalizations originating in the West; in the former, the validity of (mostly) American and (sometimes) European psychological concepts is taken for granted, so that these concepts may be used to provide an understanding of observations in Africa.

Objective (a) is an interesting one. One can infer from MacCrone's relevant statements that this was his objective. However, he is primarily interested in Whites, more particularly Afrikaners, and why they act in a prejudiced manner towards others. He does this, basically, by showing that their prejudices are only "natural". Firstly, for historical reasons, and secondly, for all kinds of psychological reasons. He draws on an unbelievable variety of psychological concepts and methods, but their one common feature is that the behaviour in question is made to appear "natural".
Thus, prejudiced attitudes do not differ qualitatively from unprejudiced attitudes, but only in degree (this is the basic assumption built into his elaborate technology of attitude scale construction). Also, Afrikaner anti-black attitudes are explained in turn by means of group psychology (Le Bon via Freud), by the Oedipus Complex, and even by behaviourist habit theory. Obviously, the specifics don't matter - it is the convergent message that counts: these are natural responses to certain stimulus situations.

"Understanding African behaviour" has a very specific meaning in MacCrone's context. The phrase "African behaviour" indicates that the target of psychological research does not consist of intergroup or even inter-individual realities, like conflict or oppression, but is always defined in terms of attributes of groups (or individuals) taken singly, isolated from the field of intergroup relationships. Members of these groups are then invariably understood as reagents never as agents, in a particular historical situation. In fact, there is no room for agents in this mode of understanding. Unlike American psychologists, MacCrone does have a role for history, but it is a very peculiar role. History becomes the equivalent of fate: It is something that happens to people, not something they make. Moreover, it is something that happened a long time ago (his history stops at the end of the 18th century), not something that is happening now, let alone something extending into the future.

Clearly, this project of psychological "understanding" is very different from the project of psychological manipulation that Biesheuvel was more interested in. The question that now needs to be asked is whether this "understanding" project fits Foucault's category of "governmentality".

There is enough evidence in the studies covered here to indicate that in all three there was an interest in more or less rationalized schemes to enable an understanding of human beings in the name of certain desirable objectives: rational administration, industrial efficiency, and harmonious group relations.

But there is a major difference as well. In the work of particularly Biesheuvel and MacCrone, but also to some extent in the in the intelligence testing work, the concern is with a question of using psychology to understand "the other one"; the issue of self-understanding never comes up. So it is very difficult to interpret this psychological approach in terms of "technologies of the self". There is little evidence of an invitation 'to act upon one's self' in this work; to pay attention to the nuances of the experience of oneself. It is unlikely that MacCrone thought that his work on prejudice would help the self-understanding of prejudiced people; it was a way for the less prejudiced to understand the more prejudiced; of understanding the behaviour of a group of trouble makers with whom one is condemned to live.

The concept of governmentality therefore has a bearing on the South African case, but only in so far as it relates to "the disciplining of human difference" (Rose, 1996). The "technologies of the self" are absent at this stage: subjectivity in the sense of being reflective and meditative about one's self; in the sense of constituting oneself as a subject, striving towards the attainment of happiness, fulfillment or perfection.

References

Barnes, L. (1930). *Caliban in Africa. An Impression of Colour-Madness*. London: Gollancz.

Fleisch, B.D. (1995). Social scientists as policy makers: E.G. Malherbe and the National Bureau for Educational and Social Research, 1929 - 1943. *Journal of Southern African Studies*, 21, 349-372.

Foster, D. (1983). *Group Relations in South Africa: The Contribution of I.D. MacCrone*. Paper presented at the National Psychology Congress, Pietermaritzburg, September.

Gordon, C. (1991). Governmental rationality: An introduction. In G. Burchell, C. Gordon and P. Miller (Eds.), *The Foucault Effect*. Chicago: University of Chicago Press.

Louw, J. and Foster, D. (1991). Historical perspective: Psychology and group relations in South Africa. In D. Foster and J. Louw-Potgieter (Eds.), *Social Psychology in South Africa*. Johannesburg: Lexicon.

Maccrone, I.D. (1930). Psychological factors affecting the attitude of white to black in South Africa. *South African Journal of Science*, 27, 591-598.

Maccrone, I.D. (1937). *Race Attitudes in South Africa: Historical Experimental and Psychological Studies*. London: Oxford University Press.

Rose, N. (1985). *The Psychological Complex*. London: Routledge.

Rose, N. (1990). *Governing the Soul*. London: Routledge.

Rose, N. (1996). *Inventing Our Selves*. Cambridge: Cambridge University Press.

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Historiographical Invisibility and Invisibilizations: Examples from the History of Social Psychology[1].

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Summary

In this paper, we examine mechanisms of invisibilization of ideas and practices, primarily within social psychology; these may be of concern to historians of the human sciences tracing the developments of ideas and professional careers. Some invisibilizations are quantitatively linked to limitations and biases in such professional databases as PsycLit, Psychological Abstracts, or Social Sciences Citation Index. Some invisibilization processes reflect voluntary and not-so-voluntary publication and avoidance strategies by authors themselves; others involve active editorial gatekeeping. Examples of invisibilizations are drawn from quantitative, case and interview studies, and revisibilization strategies are briefly discussed.

The notion of invisibility is of course not new to historians. Critical or "new" historians (Furumoto, 1988; Lubek, 1993a) of the human sciences, for example, help make visible "lost", hidden or suppressed precursor ideas (or their pioneer formulators)[2]; through careful archival work they unmask hidden socio-historico-cultural contextual factors affecting disciplinary development and the evolution of research (and publication) practices and productivity.[3] Earlier critical analyses of scientific power relations (Lubek, 1993b, c; 1995, 1997; Lubek & Apfelbaum, 1987) sought to uncover exclusionary practices restricting the movement of ideas, for the most part, within social psychology; this sub-discipline has stimulated a wide variety of critical historical re-evaluations, recontextualizations and "revisibilizations"[4].

Following Lubek (in press), we suggest that the term "invisibility" may provide the historian with several overlapping meanings and/or mechanisms: (i) unavailability, the simplest form of invisibility, involves a lack of accessibility to a set of ideas, a publication, a translated version, or an inclusive professional publication indicator such as Psychological Abstracts; (ii) voluntary self-invisibilization may involve a choice not to submit ideas to particular outlets viewed as not reflecting the author's concerns, perspective, paradigm, or primary intended audience; (iii) not-so-voluntary self-invisibilization may involve an (almost "conditioned") avoidance response (Lubek & Apfelbaum, 1987) or withholding of submissions due to prior aversive rejections or blockages; (iv) invisibilization is an active process which results in the blockage of ideas from their intended readers through gatekeeping procedures[5]; (v) re-visibilization involves active efforts to promote and disseminate ideas to a community, and to resist or undo the invisibilizing effects of (i) to iv) above[6]. Most of the cases described involve some combination of these visibility processes.

Public and private visibility indicators: A sample of Michigan-trained social psychologists

Our recent research has focused upon all 338 PhDs in social psychology (53 of them women) trained between 1949 and 1974 in the University of Michigan's interdisciplinary Joint Program in Social Psychology (or in the Psychology Department). We have examined the relative visibility of their career-long work, in relation to such contextual factors as who their mentors were and cross-gender mentoring effects. Their publications were collected and evaluated for their choices of research practices (both theoretical and empirical), their strategies of scholarly publishing, and the funding of their research. In addition, their professional visibility was indicated by publication and citation counts.[7] Generally, it was found that when compared to other mentor-cohort groupings of students, the 6 (male) students of one male mentor became the most salient in terms of visibility and productivity (especially concerning senior-authored mainstream journal articles); they also appeared more likely to adopt the experimental research practices of the mainstream, and were more often

cited in the research literature scanned by the Social Sciences Citation Index. Febbraro et al (1996), for example, had shown in their Michigan samples generally lower publication and citation rates for the 53 women PhDs when compared to the rates for 53 matched men, trained by the same mentors at the same time.[8]

Febbraro et al (1997b) however then argued that some quantitative indicators of professional visibility did not reflect simple objective "bean-counting" exercises, but rather could show variability or bias according to gender and social contextual factors. Lubek et al (1998) then reviewed the effects of PhD mentoring on various indicators of productivity/visibility and examined more closely the " two most frequent Michigan women mentors with their 14 students, and two prominent male mentors, along with their 61 students." However, in that study, a decision was made concerning statistical concerns about "outliers": only students who had produced in their careers more than 1 senior-authored journal article were included. This decision effectively eliminated or invisibilized 43% of the women mentors' students compared to 28% of the students mentored by men!

In considering the notion of visibility or availability, Febbraro et al (1998) compared the publicly visible disciplinary production rates as archivally recorded in Psychological Abstracts or its computerized version, PsycLit, against a sample`s self-reported productivity on their Curriculum Vitae.[9] Using the publicly visible professional database, women had 63.2% of the publications of the matched men (although the mean difference between the women and men -- 9.1 vs. 14.4-- was non-significant); what is interesting is that 48. 3% of the women`s publicly visible work was senior- or sole-authored journal articles, while men were seen by this indicator to publish a significantly higher percentage of their work-- 65.9%-- in the form of these highly visible (and citable) publications. When the same questions were asked of the private record--the Curriculum Vitae-- women showed just 53.8% of the mean publications of their male colleagues, listing on average 24.3 publications, significantly fewer than their male colleagues, with 45.2. And when it came to senior- or sole-authored articles, men listed significantly more (21.1) than women (11.0), with the average woman listing only 52.1% of the amount of these highly visibilizing articles reported by the average matched man. For this sample, it seems as if both men and women shared similar low "official" visibility counts in psychology of about one third of those listed on their CVs: 31.9% , for men and 37.4% for women.

But an important element in women's lower visibility emerges from these CVs, where not only publications are listed but also other formats for dissemination of ideas, including in-house technical reports, conference presentations and colloquia, which we counted all together under the generic term "conferences, etc." For women, 53.2% of their CV listings were conferences; for men, this figure is significantly lower at 28.5%. The the men's mean (19.3) career conference presentation figure was just under 65% of the women's (29.7), although the difference was not significant. (Febbraro et al, 1998)

Such results may indicate different visibilization strategies used by women and men for knowledge dissemination. Will women's devotion of a greater proportion of their idea-dissemination/visibilizing activities to publicly speaking their ideas, rather than publishing them as senior or sole authors in a mainstream journal, create a gender-relative disciplinary invisibility? Our preliminary studies thus raise some questions about the limitations and constraints upon visibility accorded by such public disciplinary archival sources.

Citation counts are thought to indicate collegial reactions to a researcher`s ideas (varying from "polite" acknowledging of paradigm hierarchies, to in depth evaluation or generative elaboration, or to strong critique of paradigmatic heresies). They are often used as a comparative indicator of relative professional visibility. Febbraro et al (1997b) used the Social Sciences Citation Index to obtain citation counts for a small sample (n=4) of their Michigan social psychology PhDs.[10] A gender bias effect was noted whereby the men`s citations were over-counted while the women`s were under-counted.[11] We hope to expand this study, to see whether such a possible gender bias can be replicated; unfortunately, such citation "visibility" counts are sometimes used, as in my (IL`s) own department, for administrative decisions concerning promotion, tenure, etc. We hope as well to review our own previous findings with larger Michigan sub-samples, uncorrected by CV verification, which had generally found the matched men more frequently cited than women, sometimes with differences in the order of 10 to 1.[12]

Individual cases: Voluntary and not-so-voluntary self-invisibilization, and gatekeeping

While some of the aggregate data described above may provide fruitful working hypotheses, some clearer idea about the actual social decision-making processes involved may be gleaned from individual case studies concerning researchers' strategies for, and negotiations about, the research publications upon which their professional visibility rests.

A series of 47 in-depth, semi-directed interviews conducted by Febbraro (1997) with a sample of men and women social psychologists trained at Michigan, illuminates several forms of professional invisibilization and self-invisibilization within mainstream social psychology. When participants were asked about publication strategies and practices, some women and men reported, for example, that they chose not to publish in the mainstream journals (e.g., the Journal of Personality and Social Psychology) because their research topics or approaches did not fit what they perceived as the narrow focus of such journals. Some of the women, in particular, rejected not only specific mainstream journals, but also the academic publishing "game" more generally. Several women mentioned that many of their publications were book chapters rather than journal articles; some emphasized their desire to do research that was personally meaningful and central to their interests, rather than merely careerist or instrumental; and some noted their turn towards publishing in feminist journals. Thus one woman stated:

> "I have published a lot of chapters in books, rather than in peer-reviewed journals....Some of it's ideological... There are so many special journals now, and often they have a theoretical orientation or a methodological orientation. So that if you want to target that particular publication, you have to toe the line."

They also reported experiences in which their manuscripts were rejected by mainstream journal editors (e.g.,on methodological or theoretical grounds).As one woman recounts:

> "I've written a lot of book chapters, which is probably not the most career-oriented thing to do in a way.... I was easily discouraged. I didn't really sort of take it just as a game, that you get rejected and you try again and so on. That was a mistake....I think that if I were to do it over again, maybe I'd try to do more journal things."

A more complex invisibilization pattern was described by one woman:

> "More of my stuff has been published in books than in journals.... I hate the review process for journals.... I get very frustrated with reviewers who don't seem to understand what I'm doing or [who] raise irrelevant critiques.... So, I do a lot of invited chapters. But when I have published in journals it's...a lot in the feminist, but not a lot in the mainstream [psychology journals]. And I think that is a result of the kind of research that I do now.....I haven't published in JPSP at all, the primary journal of the field, because I'm not willing to do the research that they publish."

Interview accounts such as this allow the historian to begin to see finer distinctions concerning the different career trajectories, strategy decisions, and resultant degrees of visibility of men and women social psychologists. Some of the invisibility has been due to rejection and blockage from mainstream outlets as well as avoidance and/or voluntary abstinence from participating in the mainstream professional visibility system.

We may also look to case studies for information about more active editorial gatekeeping, for example. Lubek & Apfelbaum (1987) looked at this form of invisibilization concerning John Garcia's14-year period of invisibilization (i.e., rejections from APA and other mainstream journals)which occurred after he challenged the established (and then-dominant) neo-behaviourist paradigm of taste-aversion learning.[13] Recently, Lubek (in press) turned the analytical tables on that study's co-author, Apfelbaum, and documented two cases of her own professional invisibilizations. Her chapter (Apfelbaum, 1979), "Relations of domination and movements for liberation: An analysis of power between groups" had originally appeared in Austin & Worchel's (1979) textbook, but did not reappear in the subsequent revised and updated version 7 years later (Worchel & Austin,1986).[14] Its disappearance was noted by a number of researchers and teachers who found its theoretical formulation helpful for analyses of, and courses on, sexism, racism, and inter-group

relations. For years, photocopied "Samizdat" versions circulated until the journal Feminism & Psychology took the decision to republish the chapter with contextualizing commentaries from a number of colleagues who continue to make use of these ideas. Here perhaps is a case of re-visibilization of a set of interesting and generative ideas.

It is interesting to note that a similar topic produced a flurry of "invisibilization" activities, when a special issue on "gender and power" was commissioned for the Revue Internationale de Psychologie Sociale/ International Review of Social Psychology. Elsewhere, the invisibilization treatment accorded the two guest editors, feminist social psychologists Marie-Claude Hurtig and Marie-France Pichevin, is documented (Lubek, in press) as they were "dispossessed" of their functions by the regular editor after creating the special issue. In addition,Erika Apfelbaum's paper (1997) on "Social psychology under women's scrutiny: on not thinking about domination relations", was accepted by the guest editors and reworked after feedback. Then the regular editor intervened, sent the paper out to additional referees--one of whom judged the paper polemical and not scientific-- and then suggested rejection. It eventually appeared, but banished to the back of the issue in a special section "Debates and Counterpoints", never before used in this journal. In so moving, the article was also stripped of its title in the table of contents, and "of both its French and English abstracts, thus effectively invisibilizing it for bibliographic searches (especially for English-language searches in PsycLit or Psychological Abstracts) " (Lubek,in press).

Another interesting case concerns the rejection by the European Journal of Social Psychology (EJSP) of an article critical of its research practices and raises similar questions about possible gatekeeping or invisiblization (Spears, 1994; Ussher, 1994; Stringer, 1994). In the rejected article, Michael Billig, through an examination of two issues of the EJSP, aimed to show that the pages of the journals were "depopulate", that is, "devoid of individual characters" (Billig, 1994, p. 309). Moreover, Billig argued that such depopulation was undesirable and could and should be corrected. However, despite the article's apparent thematic relevance to the journal, the EJSP rejected it. Therefore, the paper did not appear directly before its prime audience of the social psychologists who contributed to the "depopulating" effect reported, and was in that sense "invisible".
But eventually one version of the paper was published in Theory & Psychology, (Billig, 1994), and a second (Billig, 1998) appeared as a chapter in Bayer & Shotter's (1998) Reconstructing the psychological subject , although neither of these outlets directly targets those interested in social psychological experimental studies. The editorial process involved in the rejection of Billig's paper by the EJSP has been partially "visibilized". Billig (1998) mentions the rejecting "seven page review" from one anonymous EJSP reviewer described as "critical" (p. 127), containing "crushing criticism" (p. 144), and "hostile" (p. 146).[15] While the journal's "Note To Contributors" for 1994 suggests that in this "international forum for theoretical and empirical work. ... the editors welcome innovative, well-designed research in all areas of social psychology", and hence presumably open for theoretical works such as Billig's, no visible mention is made of any exclusive focus on the 'nomothetic' approach that his paper lacked, thereby presumably "meriting" its rejection. Fortunately, Billig also found other outlets for these important ideas.

We have discussed how historiographic attempts to recount the visibility of scientists, primarily men and women social social psychologists, can be adversely affected, in a variety of ways:
i) the chronicler may choose of a biased professional visibility indicator which does not make a scientists`s production readily available; ii) the researcher`s own strategies, including the choices of a problematic critical of the mainstream, and subsequent voluntary self-insibilation in mainstream outlets; iii) not-so- voluntary avoidance , and iv) confrontation with editorial gatekeeping.The gatekeeping examples concerning Garcia, Apfelbaum and Billig also demonstrate how extra efforts, sometimes persistent over the years, by the author and/or by colleagues, is sometimes needed to visibilize certain critical ideas; sometimes such revisibilization must occur, at first through a back or side-door approach, when ideas have been blocked from directly reaching, and made invisible to, their originally intended audience.

References

Apfelbaum, E. (1997) Contrepoints et Debats: La psychologie sociale à l'épreuve des femmes: l'impensédes rapports de domination. [Counterpoints and debates: Social psychology as

challenged by women: The ignored concerning relations of domination] Revue Internationale de Psychologie Sociale/International Review of Social Psychology , 10, (2), 153- 169

Apfelbaum, E. (1995). Cross-cultural issues in social theorization: The case of feminism. In I. Lubek, R. van Hezewijk, G. Pheterson & C. W. Tolman (Eds.) Trends and issues in theoretical psychology. (pp. 55-59). New York: Springer.

Apfelbaum, E.(1986) Prolegomena for a history of social psychology: Some hypotheses concerning its emergence in the 20th century and its raison d'être. In K. S. Larsen (Ed.). Dialectics and ideology in psychology. Norwood, N.J.: Ablex, 1986, pp. 3-13.

Apfelbaum, E. (1981). Origines de la psychologie sociale en France: développements souterrains et discipline méconnue. Revue française de Sociologie, 22 (3), 397-408.

Apfelbaum, E. (1979). Relations of domination and movements for liberation: an analysis of power between groups. In W.G. Austin & S. Worchel (Eds.) The social psychology of intergroup relations. Belmont: Brooks/Cole. pp. 188-204.

Apfelbaum, E. & McGuire, G. (1986) . Models of suggestive influence and the disqualification of the socialcrowd. In C. Grauman & S. Moscovici (Eds.). Changing conceptions of crowd mind and behavior. New York: Springer Verlag. (pp.)

Austin, W. G. & Worchel, S. (Eds.). (1979) .The social psychology of intergroup relations Monterey, CA: Brooks/Cole.

Bauer, N., Thoms, H., & Lubek, I (1995). Social influences on scientific development: Funding patterns for a cohort of University of Michigan social psychologists. In R. Stachowski & A. Pankalla (Eds.), Studies in the history of psychology and the social sciences. Proceedings of the 12th Conference of Cheiron-Europe, August 31st-September 4th, 1993.(pp. 20-31). Poznan, Poland: "Impressions" Publishing Agency.

Billig, M. (1994). Repopulating the depopulated pages of social psychology. Theory & Psychology, 4 (3), 307-335.

Billig, M. (1998). Repopulating social psychology texts: Disembodied "subjects" and embodied subjectibity. In B. M. Bayer, & J. Shotter (Eds.), Reconstructing the psychological subject (pp. 126-152). London: Sage.

Bohan, J. S. (Ed.) (1992). Re-placing women in psychology. Readings toward a more inclusive history. Dubuque, IO: Kendall/Hunt Publishing Co.

Cherry, F. (1995). The "stubborn particulars" of social psychology. London: Routledge, Chapman & Hall.

Cherry, F. (in press) Entangled Lives: Letters from Jenny, the Allports and the Watsons. History of Psychology,

Cherry, F., & Borshuk, C. (1998) Social action research and the Commission for Community Interrelations. Journal of Social Issues.

Cohen, A. G., & Gutek, B. A. (1991). Sex differences in the career experiences of members of two APA divisions. American Psychologist, 46 (12), 1292-1298.

Danziger, K. (1990). Constructing the subject. Historical origins of psychological research. New York: Cambridge University Press.

Febbraro, A. (1997). Gender, mentoring, and research practices: Social psychologists trained at the University of Michigan, 1949-1974. Doctoral Dissertation, University of Guelph.

Febbraro, A. , Lubek, I., Bauer, N., Ross, B., Thoms, H., Brown, S. & Hartt, M.A. (1996). Incidence du genre et du mentor sur la production scientifique et la carrière des psychologues: La perspective de la psychosociologie de la science. (trad. N. Apfelbaum-Lubek). [Gender differences, mentors, scientific productivity and careers among psychologists: The perspective from the social psychology of science.] Cahiers du GEDISST, 16, 123-160.

Febbraro, A., Ross, B., Thoms, H., Bauer, N., & Lubek, I.(1997, a). Inter-generational theory generation: Gender effects in a career-long sampling of journal articles of PhD students and their supervisors/mentors. Paper presented at the 7th Biennial meetings of the International Society for Theoretical Psychology, Berlin, May 2, 1997.

Febbraro, A., Bauer, N., Ross, B., Thoms, H. & Lubek, I. (1997, b). Historiographical bean-counting: A reflexive account of the social construction of numbers. Paper presented at the Annual Meetings of the Canadian Psychological Association, Toronto, June 14, 1997.

Febbraro, A., Samotowka, D., Bauer, N., Lubek, I., Ross, B., & Thoms, H. (1998). Historiographical bean-counting, part ii :Further explorations in the social construction of numbers. Paper presented to the Annual Meetings of the Candian Psychological Association, Edmonton, June 3-7, 1998.
Finison, L. (1986). The psychological insurgency, 1936-1945. Journal of Social Issues, 42,(1), 21-33.
Furumoto, L. (1988). The new history of psychology. In: COHEN, I.S. (Ed.). G. Stanley Hall Lecture Series (9). Washington, D.C.: American Psychological Association, pp. 5-34.
Good, & Still (1992) The idea of an interdisciplinary social psychology: An historical and rhetorical analysis. Canadian Psychology/ Psychologie canadienne, 1992, 33 (3), 563-568.
Harris, B. J. (1986). Reviewing 50 years of the psychology of social issues. Journal of Social Issues, 42,(1), 1-20.
Lubek, I. (1981). Histoire des psychologies sociales perdues:le cas de Gabriel Tarde. Revue française de Sociologie, 22 (3), 361-395.
Lubek, I. (1993 , a). Some reflections on various social psychologies, their histories and historiographies. Sociétés contemporaines, 13, 33-68.
Lubek, I. (1993, b). The view from "Social psychology of science". In H. van Rappard, P. van Strien L. Mos and W. Baker (Eds.) History and Theory: Annals of Theoretical Psychology. Vol. 8. New York: Plenum Press, pp. 249-260.
Lubek, I. (1993, c). Social psychology textbooks: An historical and social psychological analysis of conceptual filtering, consensus formation, career gatekeeping and conservatism in science. In H. J. Stam, L. P. Mos, W. Thorngate & B. Kaplan (Eds.) Recent Trends in Theoretical Psychology (Vol. 3). New York: Springer-Verlag, pp. 359-378.
Lubek, I. (1995). A "social psychology of science" approach towards an inter-personal history of social psychology. In H.-P. Brauns, S. Jaeger, L. Sprung & I. Staeuble (Eds.). Psychologie im soziokulturellen Wandel-Kontinuitäten und Diskontinuitäten. Frankfurt: Peter Lang.
Lubek, I. (1997). Reflexively recycling social psychology. A critical autobiographical account of an evolving critical, social psychological analysis of social psychology. (Pp. 195-228) In. T. Ibañez and L. Iñiguez (Eds.).Critical social psychology. London: Sage.
Lubek, I. (In press) Revisibilizing Erika Apfelbaum. Feminism & Psychology,
Lubek, I. & Apfelbaum, E. (1987). Neo-behaviorism and the Garcia effect: A social psychology of science approach to the history of a paradigm clash. In M. Ash and W. Woodward (Eds.). Psychology in Twentieth Century Thought and Society. Cambridge: Cambridge University Press, pp. 59-91.(Reprinted, 1989; paperback).
Lubek, I. & Apfelbaum, E. (1989). Les études de psychologie sociale de Augustin Hamon. Hermès: Cognition, communication, politique. 5-6, 67-94.
Lubek, I. & Apfelbaum, E. (1995). Will the real Gordon Allport please stand up? One author's changing histories of social psychology. Paper presented at the Annual Meetings of the Cheiron Society, Bowdoin College, Brunswick, Maine, June 23.
Lubek, I., Samotowka, D., Febbraro, A, Brown, A., Bauer, N., Ross, B., Thoms-Chesley, H., & Edmonds, S. (1998). "Pssst! Pass this on about social psychology!" : A cross-gender and cross-generational comparison of mentoring. Paper presented at the 30th Annual Meetings of the Cheiron Society, San Diego, CA. (June 18).
Lubek, I. & Stam, H. (1995). Ludicro-experimentation in social psychology: Sober scientific versus playful prescriptions. In I. Lubek, R. van Hezewijk, G. Pheterson & C. W. Tolman (Eds.) Trends and issues in theoretical psychology. (pp. 171-180). New York: Springer.
Miller, A. G., Collins, B. E., & Brief, D. E. (1995). Perspectives on obedience to authority: The legacy of the Milgram experiments. (Special Issue). Journal of Social Issues, *51* (3).
Minton, H. (1984). J. F. Brown's social psychology of the 1930s: a historical anteceedent to the contemporary crisis in social psychology. Personbality and Social Psychology Bulletin, 10, 31-42.
Over, R. (1982). Research productivity and impact of male and female psychologists. American Psychologist, 37, (1), 24-31.
Ross, B., Febbraro, A., Thoms, H. Bauer, N. & Lubek, I. (1996). Is there a season for theory? Theoretical and methodological writings of men and women social psychologists over the lengths

of their careers. In. C. Tolman, F. Cherry, R. van Hezewijk & I. Lubek (Eds.) Problems in Theoretical Psychology. (pp. 228-241). Toronto: Captus Press.
Samelson, F. (1974). History, origin myth and ideology: Comte's "discovery" of social psychology. Journal for the Theory of Social Behavior, 4, 217-231. Revised and updated in K.S. Larsen (Ed.) (1986). Dialectics and ideology in psychology. Norwood, N.J.: Ablex, pp. 14-29.
Scarborough, E. & Furumoto, L.(1987).Untold lives: The first generation of American women psychologists. New York: Columbia.
Spears, R. (1994). Theory & Psychology, 4 (3), 336-344.
Stam, H. J., Lubek, I., & Radtke, L. (1997). Repopulating social psychology texts: Disembodied "subjects" and embodied subjectivity. In B. Bayer & J. Shotter (Eds.). Reconstructing the psychological subject. (pp. 153-186).London: Sage.
Stringer,P. (1994).A letter to Michael Billig. Theory & Psychology, 4 (3), 353-362.
Ussher, J. M. (1994). Sexing the phallocentric pages of psychology: Repopulation is not enough. Theory & Psychology, 4 (3), 345-352.
Van Elteren, M. (1993). Discontinuities in Kurt Lewin's psychology of work: conceptual and cultural shifts between his German and American research. Sociétés Contemporaines, 13, 71-93,.
Van Strien, P. J. (1997) The American "colonization" of Northwest European Social Psychology after World War II. Journal of the History of the Behavioral Sciences, 33 (4), 349-363 .
Winston, A. S. (1996). " As his name indicates": R.S. Woodworth's letters of reference and employment for Jewish psychologists in the 1930s. Journal for the History of the Behavioral Sciences, 32 , 30-43.
Winston, A.S. (1998,a). "The defects of his race": E.G. Boring and anitsemitism in American psychology, 1923-1953. History of Psychology, 1, (1), 27-51.
Winston, A.S. (1998,b). Science in the service of the far right: Henry E. Garrett, the IAAEE, and the Liberty Lobby. Journal of Social Issues,
Worchel, S. & Austin, W. G. (1986). The psychology of intergroup relations. Chicago: Nelson Hall.

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2 For example, on French turn-of-the-century (proto)-social psychologists, see Apfelbaum (1981), Apfelbaum & McGuire (1986); Lubek & Apfelbaum (1989).

3. See Scarborough & Furumoto (1987), Bohan (1992), Cherry (1995) and Febbraro (1997) concerning gender; Winston (1996, 1998 a, b) on racism, antisemitism, and extreme right ideology; Apfelbaum (1986; 1995), van Strien (1997), van Elteren (1993) on historico-cultural influences; and Good & Still (1992) on cross-disciplinary effects.

4 See, for example, studies on the contributions of Gordon Allport (Samelson,1974/ 1986; Cherry, in press; Lubek & Apfelbaum, 1995) , J. F. Brown (Minton, 1984), SPSSI (Finison,1986; Harris,1986); Kurt Lewin (van Elteren, 1993; Cherry & Borshuk, 1998); or Stanley Milgram (compare Stam, Lubek & Radtke,1998 vs. Miller, Collins & Brief,1995).

5 These involve inter-personal asymmetric power relations between a dominant paradigmatic community's (s)elected editorial decision-makers and authors submitting their ideas for the scrutiny of that community (Lubek, 1995).

6 We are generally not concerned with available but ignored ideas, cases where ideas may seem to be invisible, when in fact they are available, read, but ignored by readers at a particular moment. Although such ideas may not be cited, discussed, nor used to generate further enquiry, they do remain relatively visible for future scholars to rediscover or recontextualize.

7 See Bauer, Thoms & Lubek (1995); Febbraro, Lubek, Bauer, Ross, Thoms, Brown, & Hartt (1996); Febbraro, Ross, Thoms, Bauer, & Lubek (1997 a); Febbraro, Bauer, Ross, Thoms,& Lubek (1997 b) Ross, Febbraro, Thoms, Bauer & Lubek (1996); Lubek, Samotowka, Febbraro, Brown, Bauer, Ross, Thoms- Chesley, & Edmonds (1998).

8 Cohen & Gutek (1991) had earlier shown that self-reported number of publications for some psychologists was generally higher for men than for women throughout their careers, both before and after the PhD, with post-PhD publication rates for women overall around 58% of men's. Over (1982) cited earlier work showing that men psychologists published twice as frequently as their women colleagues, 15 years after their doctorates.

9 From the original 53 matched pairs, 28 men and 23 women sent CVs (Febbraro, 1997).

10 For each target person, 8 researchers counted independently. Then two researchers recounted, verifying each citation against the list of publications on the Curriculum Vitae

11 The men's "visible count" in SSCI was initially recorded as 5.45/year, but in fact, only 1.25 citations/ year actually corresponded to their CV-verified publications. For the women, inital counts were reported as 3.22 citations/year. Verification revealed, however, 6.17!

12 From 1966 to 1990, the 6 [male] students trained by Leon Festinger at Michigan (1949-1951) averaged 91 citations each per year; while the 7 women and 7 matched men trained at the same period had 5.9 and 59.4 annual citations respectively. For the 9 women and 9 matched men who received their doctorates in Cohort 3 (1959-1963), citations average 1.5 and 24.6 respectively. In Cohort 5 (1969-1974), men, with 2.2 citations yearly, seem to be out-cited by the women, with 3.8 (See Fig. 4, Febbraro et al., 1996)

13 Garcia's taste-aversion findings bothered/challenged mainstream learning theorists, contradicting major tenets of the dominant neo-behaviourist paradigm. Successfully publishing his ideas between 1951 and 1964 in mainstream journals, Garcia, then took a stand strongly in favour of a cognitivist paradigmatic rethinking, between 1966 and 1979. Those same years, his work was systematically rejected by editors of mainstream journals and their chosen expert reviewers--including the American Psychological Association's.While his published work became all but invisible to APA journal readers, his work was being increasingly cited by other researchers in those same pages. Finally, the APA awarded him its Distinguished Scientific Research Medal in 1979, and re-opened its journals to his ideas!

14 Of 6 chapters not reprinted two were by the Sherifs;Carolyn Wood Sherif had died in 1982. Of the 4 remaining chapters, three authors, including Apfelbaum reported not having heard from the editors about the new book; one author, informed, could not make the deadline.

15 The reviewer, reportedly "embarrassed" by portions of Billig's paper (p. 127; p. 144), first suggests that the EJSP was to provide space "for a nomothetic approach to psychological topics" (p. 146), and then that "the paper should not be accepted by the EJSP and that its publication anywhere else would be ill-advised as likely to hurt the reputation of the author" (p. 146)

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Part-Whole Theory in Vygotsky and Lévy-Bruhl

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Summary

Husserl's *Logical Investigations* (1900) exerted a profound influence in a number of academic disciplines during the period 1900 to 1915 and just after the First World War. The present study examines this influence with respect to two distinct Husserlian schemes: part-whole theory and the pre-theoretical character of the 'naive' standpoint. The influence of these phenomenological insights is traced through the writings of two seminal thinkers in the early 20th century, Lev Vygotsky and Lucien Levy-Bruhl. The formal schema of parts and wholes, especially the concepts of *founding* and *fusion* of dependent and independent parts, permits a radical new framework in which different conceptual strategies can be articulated. This is because it expresses the isomorphic relations which hold between the components of linguistic or symbolic utterances, intentional 'objects' and material things. Part-whole theory provided Gestalt Psychology, and thus Vygotsky, with the crucial notion that all perceptual experiences uncover 'objects' as already embedded within an horizonal context: that gestalt qualities are the result of construing prominent features as dependent parts of a greater whole; and that transformations of these wholes can take place irrespective of any change in the 'objects', such that the whole is interpreted in the same way as long as the structural relations remain compatible. For critical anthropology, the phenomenological method revealed a pre-theoretical world of naive physics and prelogical relations - a vague medium with isolable, self-contained regularities of its own, not susceptible to explication or classification in rational, natural scientific terms.

The publication in 1900 of Edmund Husserl's *Logical Investigations*, (2nd ed. 1913; 3rd ed. 1920), exerted a profound influence in a number of academic disciplines during the period 1900 to 1915 and just after the First World War. The present study proposes to illuminate this influence with respect to two distinct Husserlian schemes: part-whole theory and the pre-theoretical character of the 'nauve' standpoint. The influence of these phenomenological insights will be traced through the writings of two seminal thinkers in the early 20th century, Lev Vygotsky, one of the great pioneer Soviet psychologists, and Lucien Levy-Bruhl, one of the first radical theorists of a 'new' French anthropology. In Investigation III, Husserl introduces two pairs of terms: part and whole, dependent and independent - it is the permutations of these terms which endow the theory with such powerful logical scope. Every intentional 'object', i.e. everything considered as the 'content' of a cognitive act, can be related to another as part to whole, whole to part, or as parts of one whole. It is the way in which parts are related to parts or in which parts compose wholes that reveals whether they are dependent or independent. An independent whole is a complex 'object', i.e. divisible into parts, which can exist alone in that it does not require the existence of any other 'object'. A dependent whole is also a complex 'object' insofar as it is divisible into parts, but cannot exist alone; it requires some greater whole of which it is a part. The great power and scope of this schema, which has inspired so many later workers in the field, lies in Husserl's essential insight into the purely formal a priori character of the relations which hold between any sort of part and any sort of whole.

Recent studies by Elmar Holenstein have uncovered the profound influence which Husserl's part-whole theory played on contemporary investigations in linguistics. The period 1900-15 saw a concerted effort by linguists to reject the philosophical assumptions which underpinned comparative philology in the late 19th century. One person at the forefront of this 'revolution' was Roman Jakobson, then a student at Moscow University. In fact, the first translation of Husserl's *Logical Investigations* was into Russian (1909), followed two years later by the translation of his lengthy essay, "Philosophy as Rigorous Science". In 1936, Jakobson refers to the L. I. as "a work whose breadth of importance for language theory can never be sufficiently emphasized", and as late as 1963, he singles out its Second Part as "still one of the most inspiring contributions to the phenomenology of language". Holenstein admirably demonstrates the subsequent close links which Jakobson had with Husserl and with Husserl's students, especially through the Prague Linguistic Circle in the 1920s. An equally important figure in this story is an almost unknown character,

Gustav Shpet (1879-1937), whose constant exposition of Husserlian phenomenology decisively shaped Vygotsky's thinking in ways which recent studies of Vygotsky have failed to bring out.

Shpet wasn't the only contact that Vygotsky had in his academic career with Husserl's new vision of phenomenology. When Vygotsky, Luria and Leontev were invited to join the Psychological Institute in Moscow by its director Kornilov, they had been witnesses to the divestiture of its previous director Chelpanov. It was Chelpanov who organized and taught seminars in 1915-16 on Husserl's phenomenology and on Koffka's *Analysis of Ideas and Their Laws*, a work in which its author explicitly and repeatedly commends Husserl's shaping ideas. During the war, German imprints were severely restricted by the Russian authorities and students had to illegally obtain Husserl's works from Amsterdam. But the L. I. had already been translated into Russian by E. A. (or N. A.) Berstein and S. L. Frank - is this the same Nikolai Bernstein who was Vygotsky's colleague at the Psychological Institute in the 1920s? The other outstanding Husserlian presence during Vygotsky's years of intense theoretical and experimental research was through the mediation of early texts by the Gestalt Psychologists: Kurt Koffka, Wolfgang Kohler and Kurt Lewin - all of whom Vygotsky studied with great attention and turned to his own novel purposes, and all of whom were decisively influenced by Husserl's teaching and research.

In November 1935, Husserl gave a lecture to the Prague Linguistic Circle on the intersubjective constitution of language. Jakobson had already presented Husserl with his own paper, "Folklore as a special form of creation", and Holenstein speculates that this paper may have stimulated Husserl's own thoughts, since Jakobson's essay bears on the same topics as Husserl's later "The Origins of Geometry", composed in 1936 and published as an appendix to *The Crisis of European Sciences*. This was not the only direct point of contact with the new anthropology's study of folklore and 'primitive' mentality. In a letter of March 1935, Husserl credited the French social anthropologist Lucien Levy-Bruhl with having anticipated his own conception of the horizons of the lifeworld, and thus shown the way for a genuine science of social and cultural forms. A later section of this paper will explore the connections between Husserl's part-whole theory and the lifeworld through Levy-Bruhl's thesis of prelogical mentality.

There were, therefore, three pivotal paradigm changes from which Vygotsky drew inspiration: Gestalt Psychology, Levy-Bruhl's theory of primitive mentality, and Jean Piaget's recent work in child psychology. Although Vygotsky was certainly aware of Husserl's writings and viewed him as the epitome of what he understood as transcendental idealism [Vygotsky (1987): 93], he was perhaps not aware of the enormous influence which Husserl had exerted on Gestalt Psychology and on Levy-Bruhl, and hence mediately on his own conception of cognitive development. Piaget himself applauded Husserl's principled campaign against empirical psychology's explanation of logical laws and, early in his own career, recognized Husserl's influence on Gestalt Psychology. [Piaget (1971): 102-15]

In his 1930 work with A. R. Luria, *Ape, Primitive and Child*, Vygotsky examined empirical data in three fields in connection with his theoretical picture of human cognitive development. In the first part, he explored in some detail Kohler's observations and experiments with chimpanzees, focusing on Kohler's conclusion that primates exhibited some of the basic processes of problem solving and the manipulation of simple tools. One crucial conclusion which Vygotsky drew from this material is that an ape is only able to solve those problems when it can re-establish the situation or context where the obstacles and pathways in the control setting would have been found in its natural habitat. The sticks, branches, vines and so forth form an integral whole which is relatively independent from any change in the individual elements - this is a basic axiom in Gestalt Theory. [Vygotsky (1992): 21-2]

In the second part, he discusses recent research by Levy-Bruhl and Richard Thurnwald into 'primitive' mentality; the forms of behaviour, languages and practices exhibited in so-called 'primitive', pre-literate societies. He postulates a gestural or behavioural 'intentionality' which is parallel to the phenomenological concept of the intentionality of consciousness, that is, in their overt behaviour there is movement *from* something and *toward* something. [ibid: 39] He endorses Levy-Bruhl's postulation of prelogical or 'mystical' cognition which is neither contrary to logical forms nor beyond the bounds of logical thinking. Rather, such this sort of 'prelogical' cognition is insensitive to the law of non-contradiction and instead subscribes to the law of participation. In his own summary conclusion, Vygotsky agrees with some contemporary critics, including Levy-Bruhl,

who castigate previous anthropologists for evaluating all symbolic forms of primitive 'rationality' in terms of western canons of logic; at the same time, he disagrees with the heavy-handed critics of Levy-Bruhl who had accused him of a thorough and unrepentant relativism. In this latter case, Vygotsky is very much in accord with some recent reappraisals of Levy-Bruhl which assign him a prominent place in the historical emergence of critical anthropology.

Vygotsky remarks that for the primitive person, perception is much more acute than for the 'civilized' person and also intrinsically connected with the affective reactions and expectations which accompany perceptions. The primitive's memory is also much more accurate in recall but is also equally charged with affective overlays. Memory "retains representations with a greater abundance of detail, and always in exactly the same order in which they are really connected with each other.... The mechanism of memory supplants the mechanism of logic: if one representation reproduces another, the latter is assumed to be a consequence or a conclusion. Signs are therefore almost always interpreted as causes." [Vygotsky (1992): 50-1] He thus agrees with Kohler, Levy-Bruhl and Wertheimer (whose research on numerical concepts he cites) and characterizes the 'primitive' conceptual framework as one of 'naive' physics, a pre- theoretical and concrete orientation towards the world. As such, this echoes Husserl's reference to the 'naivete' of the natural attitude which has to be uncovered and made thematic through the phenomenological reduction. In the third part of the text, Luria discusses the behaviour and cognition of the child, relying on both their own experiments and those of Jean Piaget.

The ontogenetic development of the forms of cognition is taken up at greater length by Vygotsky in *Thinking and Speech* (1934), better known perhaps under its previous English title, *Thought and Language*. The most obvious influence of Husserl and Gestalt part-whole theory on this work is to be found in the first chapter, "The Problem and Method of Investigation". Here he criticizes psychological theories which divorce thinking from speech in order to study each system separately. He is quite adamant that only an understanding of thinking and speech as features of a unified whole will yield and adequate and fruitful psychology. He applauds recent research in structural linguistics for recognizing the distinction between dependent and independent parts of a whole, and demands that the same distinction be brought into the psychological vocabulary. The Russian editor of the original text points out that this contrast of unit and element was one of Vygotsky's favourite notions. The significance of these paired terms is very similar to that of Husserl's discrimination of independent part (piece) and dependent part (moment) of a complex whole. It takes up the argument first posed by Ehrenfels in "On Gestalt Qualities"

Before examining Levy-Bruhl's contributions to the debate about 'primitive' mentality, it will perhaps be instructive to consider Husserl's other influential work from these formative years, "Philosophy as Rigorous Science" (1910). Here Husserl continues and expands his criticisms of empirical psychology, but with more explicit attention to the programmatic claims of such fact-based psychological theories to emulate the axiomatic character of lawful regularities in the natural sciences. The world investigated by the natural sciences is fully open to the observation and abstraction of determinations of necessary relations between objects and events, irrespective of the symbolic or mediated framework in which such relations are expressed. But in the conscious domain, i.e. the world as investigated by the psychological 'detective', all 'objects' of enquiry are such as are given to consciousness before they are 'subject to' the fixation of terms in a methodology. It is the very essence of psychical phenomena that they are always mediated, always already given as expressed in some form or other, which are "fluid and ambiguous" until they are made determinate by the terms of a psychological schema. "in its purely psychological concepts, which it now cannot at all dispense with, it necessarily gives a content that is not simply taken from what is actually given in experience, but is *applied* to the latter." [Husserl (1981): 177]

The genuine phenomenological method directs the investigator to focus on precisely that *naive* experience of the psychical, as the natural scientist does with the naive data of the physical world, but then to *leave it alone*, to pay attention to exactly the way in which naivete is manifest in all that which appears to consciousness. The pre-theoretical, 'unscientific' fact that 'objects' are given sometimes as vague, unclear or even deceptive, before scientific mentality 'corrects' these appearances is itself extremely important and indicative of the unique 'nature' of the psychical domain. "To follow the model of the natural sciences almost inevitably means to reify consciousness - something that from the beginning leads us into absurdity." Physical realities exist in a unified

spatio-temporal world, a world with one space and one time, within which these realities are or are not causally connected. Each physical thing has its own nature as point (or juncture) within all of one nature, subject to causal laws; in fact, it is often the case that a physical thing is only identifiable with respect to those laws, e.g. sub-atomic particles. In contrast, psychical 'realities' are given as unities of immediate experience, of diverse sensible changes, each of which may be given originarily as an 'object' within its own spatio-temporal field, that is, as co- determined only by all those other 'objects' which appear with it. Stabilities, changes and the relations between changes within the phenomenal field function for cognition "like a vague medium", through which the 'objective', physical nature shines forth (*phainomena*) and according to which scientific thought elicits and constructs general statements.

It is this "vague medium" of the phenomenal world which is the proper domain of a strictly phenomenological investigation. This pre-theoretical sphere, with its instabilities, occlusions, deceptions, and so forth, where 'objects' and relations are always only given within a self-founding perceptual field, is the proper domain of *a concrete science.* Concrete in that this study makes available pre-reflective apprehensions of 'objects' and their relations, and pre-predicative judgements based on them. Any psychology (or social science) which pays strict attention to this concrete, sensuous world has all the greater chance of allowing the 'objective' nature of the phenomena described to 'shine through'. It was to a concrete science of the social world that Levy-Bruhl turned to after years of study and writing in the history of western philosophy. His earliest work, *La Morale et la Science des Moeurs* (1903), was an attempt to apply empirical models to a cross-cultural analysis of ethical behaviour and beliefs. His exposure to these culturally bound codes of conduct and morality drew him towards a more in-depth study of the most basic features of social reality.

For Levy-Bruhl, these basic features were most clearly revealed in the functions of 'primitive' mentality, part of the original French title of his seminal 1910 work, translated into English in 1926 with the title *How Natives Think.* Vygotsky's research with Luria in *Ape Primitive and Child*, and his 1932 *Studies in the History of Behaviour* referred to both Levy-Bruhl's and Wertheimer's studies, but were critical of their conclusions which he felt reduced or transposed cognitive operations into an entirely mechanistic dimension. Throughout this period from the early 1920s to the 1930s, although Husserl and phenomenology were very much at the forefront of French philosophical interest, Levy-Bruhl's work in the 'applied' area of the lifeworld was under constant attack. How much more reassurance and perhaps surprise he must have felt when he received Husserl's admiring letter of March 1935.

C. Scott Littleton, the most recent editor of *How Natives Think*, identifies two fundamental developments which influenced Levy-Bruhl's critical approach to anthropology: structural linguistics, via de Saussure and Jakobson, and Husserlian phenomenology. Littleton, however, has only the most simplistic notion of the meaning and scope of Husserl's project and leaves unstated the many shaping conceptual strategies to be found in Levy-Bruhl's works. In many respects, *How Natives Think* is paradigmatic of Levy-Bruhl's entire oeuvre; most of his fundamental theoretical insights are to be found in this text, worked out in terms of then current ethnographic research, especially with regard to 'prelogical' cognition. The author abjures the reader to banish any received idea of representation: one must abstain from or hold in 'suspense' one's own preconceptions of what this idea means in order to come to an adequate understanding of 'primitive' cognition.
The employment of the concept of 'suspension' points directly to the Husserlian demand for a phenomenological bracketing of the 'natural standpoint' which presumes both the thesis of the existence of the external world lying over against the perceiver and the natural scientific account of a causal connection between 'object' and thought. In the 'primitive' world, the nature of a physical thing is at least partly determined by circumstances in which it was made or found. The thing then is bound up with an agent's motor activity, the thing's previous uses, so-called 'mystic' properties with which it is imbued. As such, the thing is always an integral part of some greater whole; it is always given to consciousness within a *gestalt* structure.

Prelogical thinking remains indifferent to the resemblance in form between any two or more objects. An arrangement of specific objects that signifies one thing when employed in connection with a certain operation signifies something completely different when employed in connection with another. In this arrangement, "the place occupied by a person, an object or an image is of paramount importance.... To the primitive mind, space does not appear as a homogeneous unity, irrespective of

that which occupies it, destitute of properties and alike everywhere." [ibid: 120] The emphasis here on relative place within a given structure is consonant with a gestalt psychological interpretation, and the context-specific, non-homogeneous spatiality of the structure with Husserl's sketch of a naive physics of space-time. So this story has come full circle: Levy-Bruhl's *How Natives Think* in 1910 inspired by (amongst others) Husserl's part-whole theory, taken further after the publication of"Philosophy as Rigorous Science" and *Ideas First Book* in his second work, *La Mentalite Primitive* (1922), one of Levy-Bruhl's works read by Husserl and commended in his letter of 1935, after the Prague Conference, and which possibly served to turn Husserl's own attention to the structures of the lifeworld in his final, forward-looking thesis.

In conclusion, to gather together the many strands of this story, we can identify at least two distinct sources of influence within Husserl's phenomenological project: the first is part-whole theory, an exceptionally powerful formal ontology; the second is the pretheoretical foundations of the lifeworld, the source of many current attempts at a constructive theory of social reality. The formal schema of parts and wholes, especially the concepts of *founding* and *fusion* of dependent and independent parts, permits a radical new framework in which different conceptual strategies can be articulated; this is because it expresses the isomorphic relations which hold between the components of linguistic or symbolic utterances, intentional 'objects' and material things. In linguistics, it allows the generation of the concepts of minimally differentiated features on the phonemic level, and the functional stratification of meaning, content and 'object' on the morphemic level. In addition, the latter source helps to account for the a priori laws which govern relations between possible meaning forms, as well as the intersubjective constitution of language. Part-whole theory provided Gestalt Psychology with the crucial notion that all perceptual experiences uncover 'objects' as already embedded within an horizonal context; that gestalt qualities are the result of construing prominent features as dependent parts of a greater whole; and that transformations of these wholes can take place, such that, irrespective of any change in the 'objects', as long as the structural relations remain compatible the whole is interpreted in the same way. For critical anthropology, the phenomenological method revealed a pretheoretical world of naive physics and prelogical relations - a vague medium with isolable, self-contained regularities of its own, not susceptible to explication or classification in rational, natural-scientific terms.

References

Haardt, Alexander (1992). Husserl in Russland: Phanomenologie der Sprache und Kunst. Munich: Fink.

Holenstein, Elmar (1975). "Jakobson and Husserl", in *Human Context*. vol. 7. pp. 61-83.

Holenstein, Elmar (1976). *Roman Jakobson 'sApproach to Language*. trans. by Catherine & Tarcisius Schelbert. Indiana Univ. Press.

Husserl, Edmund (1970). *Logical Investigations*. Trans. J. N. Findlay. London: Routledge & Kegan Paul.

Husserl, Edmund (1981). *Shorter Works*. Ed. by Peter McCormick & Fred Elliston. Univ. Notre Dame.

Husserl, Edmund (1983). *Ideas First Book*. Trans. by Fred Kersten. Dordrecht: Kluwer Academic.

Lévy-Bruhl, Lucien (1985). *How Natives Think*. Trans. by Lilian Clare. New Intro. by C. Scott Littleton Princeton Univ. Press.

Piaget, Jean (1971). *Insights and Illusions of Philosophy*. trans. by Wolfe Mays. London: Routledge.

Shpet, Gustav (1991). *Appearance and Sense: Phenomenology as the Fundamental Science and its Problems*. Trans. by Thomas Nemeth. Dordrecht: Kluwer Academic.

Simons? Peter (1987). *Parts: A Study in Ontology*. Oxford Univ. Press.

Smith, Barry (Ed.) (1982). *Parts and Moments*. Munich: Philosophia.

Smith, Barry (Ed.) (1988). *Foundations of Gestalt Theory*. Munich: Philosophia.

Spiegelberg, Herbert (1971). *The Phenomenological Movement*. Third edition. The Hague: Nijhoff.

Spiegelberg, Herbert (1971). "From Husserl to Heidegger", in *Journal of the British Society for Phenomenology* (2) pp. 58-83.

Van der Veer, Rene & Valsinger, Jaan (Eds.) (1991). *Understanding Vygotsky: A Quest for Synthesis*. Oxford: Blackwell.

Vygotsky, L. S. (1987). Collected Works. Vol. 1: *Problems of General Psychology*. Ed. by R. W. Rieber & A. S. Carton. N. Y. & London: Plenum Press.
Vygotsky, L. S. (1993). *Collected Works*. Vol. 3: *Problems of the Theory and History of Psychology*. Ed. by R. W. Rieber & Jeffrey Wollock. N. Y. & London: Plenum Press.
Vygotsky, L. S. (1998). *Collected Works*. Vol. 5: History of the Development of Higher Mental Functions. Ed. by R. W. Rieber. N. Y. & London: Plenum Press.
Vygotsky, L. S. (1992). *Ape, Primihve and Child*. Trans. by Evelyn Rossiter. Harvester-Wheatsheaf.
Wertsch. James (Ed.) (1985). *Culture, Communication and Cognition*. Cambridge Univ. Press.

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Theodor Ziehen's Contribution to Psychology: A Preliminary Assessment

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Summary

The aim of the present paper is to take a closer look at the psychological work done by the German psychiatrist Theodor Ziehen (1861-1950). Starting with some biographical notes about Ziehen a bibliometrical analysis of his publications is performed where his special interest in Psychology comes into sight. To get a closer look at the content of his psychological system a historical analysis of his famous textbook *Leitfaden der physiologischen Psychologie* (Ziehen, 1891) is finally presented.

As one of the eminent contributors in Psychology (see Watson, 1974), the name of Theodor Ziehen appears quite often in any book on History of Psychology. Specially when dealing with associative psychology his name, together with that of Ebbinghaus and G.E. Müller seems nearly a standard reference. But, on the other hand, we find rarely detailed explanation of his Psychology beyond the citation of his name. The aim of the present paper is to fill that gap and get closer to what the name of Ziehen stands for, as a person and as a psychologist. Specially interesting seems a reappraisal of Ziehen as a representative of the associative paradigm in Psychology taking into account the formulation his psychological system receives in his famous *textbook Leitfaden der physiologischen Psychologie*. Let us start with some biographical notes about Ziehen.

The German psychiatrist Theodor Ziehen was born 1861 in Frankfurt a son of a religious and intellectual family with rather limited economical possibilities. Although he soon recognized his interest in Philosophy, economical reasons lead him to study Medicine. His studies took him from Würzburg to Berlin, listening to lectures of Westphal and the physiologist Munk who directed Ziehen's doctoral thesis about electrical stimulation of the cerebral cortex (Ziehen, 1930). After finishing his career he immediately started to work as a psychiatrist, first at Görlitz and soon after this (in 1886) he followed an invitation of Otto Binswanger to come to the University of Jena. There he stayed for 14 years, which he later described as the most happy and productive years of his life (Ziehen, 1923 and 1930).

Beside his ocupation as a psychiatrist, his interest in Philosophy and Psychology took him again and again to do some work in those fields too. So he installed a small psychological laboratory in Jena where he was able to undertake some research. His lectures and research in Psychology helped him to get a broader knowledge in that field which found systematization in his *textbook Leitfaden der physiologischen Psychologie* published 1891. In Jena he was promoted to a professorship doing his "Habilitation" in 1887 with a study on mental illness (Ziehen, 1930). In 1896 he left the university to work in a privat practice, but his collaboration in several scientific journals helped him to keep linked to recent research in Psychiatry and Psychology.

In 1900 he went as professor in Psychiatry to the University of Utrecht where he stayed for three years. After that he went for one year to Halle where he received a prestigious offer: the professorship in Psychiatry and Neurology at the University of Berlin, a position which involved the direction of the Clinic Charité. By accepting this new charge he started to face a great organizational responsibility which, together with his lecturing duties and psychiatric work, kept him very busy. For this reason he resigned 8 years later (1912) to change to a more quite life and retired to his home in Wiesbaden where he could fully dedicate himself to his philosophical and psychological interests. His isolation came to an end in 1917 when he accepted a professorship in Philosophy at the University of Halle. In 1930 he finally retired. The last years of his life were rather sad. The national socialist government tried to avoid any homage because of his opened protest against their ideas. The Second World War brought him terrible personal loses as his wife died and his house was completely destroyed by a bomb. Desperated and economically ruined he asked to resume his work

again at the University of Halle, but because of his delicate health, he finally could not accept the offer and died in 1950 in Wiesbaden.

As this biographical notes about Ziehen show, althought his formation and work was mainly in the field of Medicine and Psychiatry, he always tried to get time for Philosophy and Psychology. During his whole life he was able to publish a lot of works which can be classified thematically, like it is shown in table 1, into five categories: Psychology, Psychiatry/ clinical Psychology, Philosphy, Anatomy and Physiology and other subjects.

Table 1. Thematical classification of the works published by T. Ziehen

WORKS PUBLISHED BY ZIEHEN	NUMBER OF PUBLICATIONS	%
Psychology	43 (2*)	37,4
Psychiatry/Clinical Psychology	27 (3*)	23,4
Philosophy	24	20,9
Anatomy and Physiology	12	10,4
Other subjects	9 (1*)	7,8
Total number of publications	(115 - 3) = **112**	100

At first sight appears the great amount of psychological works done by Ziehen. To get a closer look at this works we show in the second table a thematical classification of these.

Table 2. Thematical classification of Ziehen's work in the field of Psycholgy

PSYCHOLOGICAL WORKS OF ZIEHEN	NUMBER	OF WORKS	%
1. psychological capacities and processes		**18**	40,9
sensation and perception	8		
attention	2		
intelligence	2		
feeling and emotion	2		
in general	1 (1)		
memory	1		
thinking	1		
will	1		
2. theoretical aspects		**9**	20,5
3. general Psychology		**5**	11,4
4. differential Psychology		**5**	11,4
child and youth Psychology	4		
anthropological studies	1		
5. criminal Psychology /characterology		**4**	9,1
6. industrial Psychology/ professional orientation		**2** (1)	4,5
7. military Psychology		**1**	2,3
TOTAL		**44**	100

- the number in brackets refers to the works which are classified into two categories.

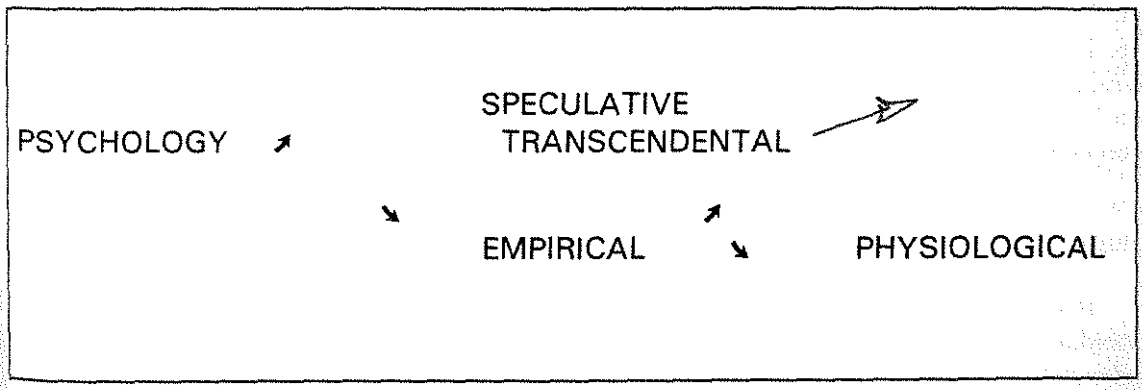
Most of his publications treat subjects related to different psychological capacities and processes like atention, intelligence, feeling and others. He is specially interested in research about human visual and tactile sensation and perception. One work included in this first category behalf the keyword "in general" is a work of Ziehen which treats the problem of heredability of different psychological capacities in relation with professional orientation. For this reason it has been included into two categories (1) the first (psychological capacities and processes) and the sixth (industrial Psychology/ psychological orientation). So the definitive sum of his psychological works is again 43 (see table 1). The next category, with 9 publications, ist the one we called "theoretical aspects" which includes different writings of Ziehen about the relation between Psychology and other disciplines like Psychiatry and Medicine. There is also a revision of the past Psychology and Philosophy in foreign countries and other works of Ziehen about more concrete theoretical aspects like a reflection about the body-mind relation and a comment to the Psychology of Herbart, etc.

Ziehen published five works dealing about the research in the field of general Psychology like his two textbooks: his *Leitfaden* we have already cited (Ziehen, 1981) and his *Grundlagen der Psychologie* (Ziehen, 1915), together with some shorter writings on that subject. The same number of works apear in the category called "differential Psychology" where Ziehen shows a clear interest in child and youth Pychology. The table 2 made clear he was also interested in criminal Psychology, a field he worked in at the end of the twenties using a caracterological perspective. Finally, in the last categories apears the number of his works on industrial Psychology and military Psychology.

The bibliometric analysis of Ziehen's work showed the high number of publications this author produced in the field of Psychology. But it would be impossible to expose here a detailed historical analysis of the whole work published by Ziehen in that field. To solve that problem we decided to get in touch with the content of his work with the help of an historical analysis of one of the most known textbooks in Psychology, his *Leitfaden der physiologischen Psychologie*. As he himself stated (Ziehen, 1930), in the first edition of this handbook he already developed his own psychological system comunely called "associative Psychology". This book is based on his lectures on Psychology given at the Faculty of Medicine since his arrival at the University of Jena in 1886. But the book is not only directed to those who study Medicine or Psychiatry. Ziehen remarks in the introduction of his textbook that its content is also directed to the natural scientist.

Right at the beginning of his textbook Ziehen includes the physiological Psychology in the broader field of Psychology, which is divided into empirical and speculative Psychology as shown graphically in table 3.

Table 3. Graphical representation of the field of Psychology defined by Ziehen (1891)



The physiological Psychology is that kind of Psychology which studies all the conscious mental processes that have correlating physiological brain processes. This way the hypothesis of psychophysical parallelism is of great importance for Ziehen in any research in that field.
He emphasizes the consciousness of the processes that are to be studied, a fact that implies that all kind of reflexes and automatic acts are excluded of the studies in physiological Psychology.

How should the human mind be studied? Ziehen, as well as many other of his contemporaries, was convinced that introspection still represents the main way to get to the consciousness of a human being, but it should be combined with experimental methods like Psychophysics and psychophysiological measurements. In general he follows the commun conviction at that time about the approachment of such a research: at first some concrete psychological phenomenon that are connected to the external world (like sensations) should be studied before getting to more complicated, hidden processes.

Until now the psychological research programm proposed by Ziehen does not differ significantly from other proposals of that time like that of Wundt, for example. But there are fundamental differences between the two appoaches when getting to their general views about scientific research. Whereas Wundt adopts a neoKantian approach which leads him to develop his theory of apperception, fundamental for his psychology of will, Ziehen follows a more empirist and positivist perspective, rejecting completely any theory of apperception. This way Ziehen appears cited as one of the German postivists like E. Mach (Hehlmann, 1967; Sachs Hombach, 1993). But there are more differences between the Psychology of Wundt and Ziehen. The most differentiative aspect of Ziehen's system is undoubtely his way to include feeling in form of a "emotional tone" following primary sensations or representations. As well as Titchener and Külpe he only differenciated between pleasure and unpleasure (Lust / Unlust) against the threedimensional theory of Wundt.

As he links his physiological Psychology very closely to the study of physiological processes, he is described also as a materialist (see, for example, Woodward and Ash, 1982). But Ziehen would not have been happy with that description of his approach. In his autobiography he says that he follows a tendency towards idealism. He may not be conscious of how far behind he had left idealism, as there is no such tendency to be found in his textbook.

Ziehen already prepares the reader of his textbook about the fact that he will expose a Psychology different from that of Wundt, a psychological system more connected to the Psychology of association developed in Britain. Following that approach he converts the mecanism of association into the main principle of functioning of the mental processes. By that way his psychology can be described as empirist, atomistic, sensualistic and mechanicistic.

References

Hehlmann, W. (1967). *Geschichte der Psychologie*. Stuttgart: Körner.

Sachs-Hombach (1993). *Philosophische Psychologie im 19. Jahrhundert*. Freiburg: Alber.

Woodward, W. and Ash, M. (1982). *The Problematic Science: Psychology in the 19th century thought*. New York: Praeger.

Watson, R.I. (1974). *Eminent contributors to Psychology*. New York: Springer.

Ziehen, T. (1891). *Leitfaden der physiologischen Psychologie*. Jena: Fischer.

Ziehen, T. (1915). *Die Grundlagen der Psychologie* (Band 1 und 2). Leipzig: Teubner.

Ziehen, T. (1923). Autobiographie in R. Schmidt (Hrsg). *Philosophie der Gegenwart in Selbstdarstellungen* (Band 4) (pp. 219-236). Leipzig: Meiner.

Ziehen, T. (1930). Autobiography in C. Murchison (Ed.). *A History of Psychology in Autobiography*. Worcester (Massachusetts): Clark University.

Lisa M. Osbeck

Nineteenth Century Critiques of "Intuitionism" in Relation to Notions of Direct Cognition

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Notions of "direct" cognition, as central, for example, to the ecological theory of perception (Gibson, 1979) may be viewed within the wider context of arguments suggesting that our most fundamental apprehension is in some way "immediate", free of intervening inference or justification, entailing an irreducible intellectual "grasp" of some aspect of experience. Such arguments are traditionally found within dominant philosophical accounts of intuition. They may be traced at least to Aristotle, who clarified a distinction between two types or sources of knowledge. Some is said to be directly "given" or rather "picked up" from the nature of the world and its relationships; the other is derived or acquired through inference from what is given (*Metaphysics, Prior Analytics*; in Barnes, 1984). This distinction is upheld and its implications expanded within both rationalist and empiricist epistemologies at least until the 19th century (e.g. Descartes 1928/1994, Locke 1690/1964).

For various reasons direct cognition remains a controversial notion. An assumption that mental activity is fundamentally indirect is evident even in psychology's portrayal of intuition. The element of the directness, the immediate "grasp" by which intuition is characterized philosophically, is lost in prominant psychological accounts (Osbeck, 1998). In its place is a notion of intuition as an unconscious inferential process, appropriate for particular kinds of tasks (e.g., Kleinmutz, 1990; Hammond, 1997). The framing of intuition as inference represents a distinct departure from its epistemological heritage.

The predominance of indirect, central processing models is noted to reflect the heritage of a Cartesian mind-body dichotomy (e.g., Gibson, 1979; Harre & Gillett, 1994). But since Descartes (1628/1994) considers direct apprehension to be the most fundamental act of mind, the departure from "directness" itself in cognitive models reflects the additional influence of 19th century epistemological developments, particularly critiques of "intuitionism" (i.e. Mill, 1865/1979; Peirce, 1868). Through these the notion of direct knowledge and more broadly, direct cognition was explicitly rejected (intending a departure from Descartes), with implications for the method and focus of psychology into the latter half of the twentieth century. This paper briefly examines the critiques of Mill and Peirce, their viability, and their ongoing influence in relation to contemporary notions of direct cognition.

Associationism

J.S. Mill It should be first noted that empiricism is not sufficient to account for resistance to notions of intuitive apprehension or direct cognition (see Ockham, *Sentences*; Bacon, *Novum Organum*; Locke, *Essay*, 1690/1964). Hume's legacy, however, channeled into an enduring doctrine of associationism through which the notion of intuition was restricted to Cartesian accounts of indubitable knowledge and thus denounced as obsolete (Mill, 1865/1979). Though Mill's original view of sensation suggests an endorsement of sensory intuition or direct knowledge through sensory channels, he later shifts to one of sensation as physiological facts or *effects*. In An Examination of Sir William Hamilton's Philosophy (1865/1979), Mill rejects the notion of "direct truth" (though truth and knowledge are certainly distinguishable notions!). Intuitive knowledge is connected with the "introspective" method, while acquired knowledge (inference) is the subject of the "psychological" method Mill endorses. Mill's primary target is Sir William Hamilton, whose ideas Mill considers to reflect those of Kant (1781/1990) and Reid (1764/1975), but who has since been acknowledged to offer a questionable interpretation of these (Reed, 1994).

The central tenet of Mill's psychological method is that mental events can be entirely explained in terms of inferences arising from sensory experience and their associative links. His main contention is with is the alleged necessity of claims said to be derived intuitively. But his critique exhibits several conceptual shortcomings. Most relevant to this discussion, Mill neglects or

misinterprets revised understandings of necessity as they relate to intuitive apprehension, as offered most notably by Thomas Reid (1764/1975; 1785/1975) and Kant (1781/1990). Among the most compelling of Reid's points, for example, is that survival requires us to take certain assumptions for granted, that direct understanding of the so-called (contingently true) first principles of "common sense" is characterized by a sort of practical or ecological necessity (Reid, *Inquiry*, Essay 6). From anyone's actions we can infer that certain presuppositions are taken for granted as true. These are necessary only in the special sense that they are requisite to survival. Such belief, therefore, "is not grounded on evidence. It is the result of his constitution" (Reid, *Powers*, Essay 4). Since there are no mediating influences, these fundamental assumptions are in some sense *im*mediate, or direct.

Though references both to direct *perception* and to Reid's account of intuitive understanding of *principles* risks collapsing two very distinct notions, arguments offered by Reid for the necessity of contingently true first principles are not radically different in spirit from Gibson's account of information directly picked up by affordances. Both reflect an assumption that some constitutionally determined "grasp", not mediated by previous inference, is implicit in the act of survival and fundamental to activity.

Mill also fails to effectively confront Kant's "synthesis" of the two traditions of metaphysics, whereby the very conditions for cognition (or experience itself) are directly provided through intuition, or more specifically its "pure forms" (i.e. space and time) (Kant, 1781/1990). As conditions of sensibility, the forms of intuitions are not logically necessary or morally authoritative but without these there could be no "logic". For Kant, as for Reid, the criterion of "thinkability" is not relevant to the sense in which intuitive knowledge is considered necessary.

Finally, Mill appears to overlook developed accounts of intuition within his own empiricist tradition. In particular, Mill's conviction that intuition and association are contrasting notions does not take into account Locke's understanding of knowledge as "nothing but the perception of the connextion and agreement, or disagreement and repugnancy of any of our ideas", which when direct is called "intuitive" (*Essay* IV, 2). But Mill's explanation of necessity in terms of unvarying association does not quite capture Locke's meaning. Though all knowledge originates in experience, Locke distinguishes intuition both by the immediacy of the perceived association and its function of furnishing grounds for reasoning, for which reason he considers intuition a "first level" of knowledge.

Denial of Direct Cognition in Pragmatism.

Peirce introduced the term "pragmatism" and was the first to develop this into a comprehensive system, designed to clarify traditional metaphysical questions and abolish meaningless notions (Ayer, 1968). One such notion for Peirce is intuition, understood in one specific sense acknowledged by Descartes, as an act in which we "see all together" the truth of a directly known premise and the necessity of certain conclusions. Peirce's denial of intuition is based not only on a conviction that all beliefs are fallible but also that no belief arises directly (immediately) from sense experience. Instead, inference is the mind's essential function. Allegedly intuitive/direct knowledge of sense data is something that admits testing by evidence, by its "practical effects" (5.234). It has, according to Peirce, the nature of a hypothesis, the test of which is provided by its consequences.

Peirce's reply to the infinite regress argument (traditionally raised in defense of intuitive knowledge) appeals to mathematical principles, since all deductive reasoning is said to be "of the nature of mathematical reasoning" (5.147). The chain of regress requires and admits no final term at which the chain of inference ends, beyond which it can go no further. There is, Peirce emphasizes, always the potential for continuation.

Peirce's commentators acknowledge that his position itself offers no positive grounds for assuming that our thoughts follow a continuous sequence in the strict mathematical sense, and that this represents a faulty analogy. For example, to say that the time involved is infinitely dividable does not mean that the cognition itself is (Ayer 1968). Moreover, the denial of intuition is at odds with other work in which Peirce does admit a form of direct knowledge . Peirce maintains that all deductive reasoning "is of the nature of mathematical reasoning" (5.147), "which derives no warrant from logic. It needs no warrant. It is evident in itself" (2.191). Also, Peirce acknowledges "the evidence of my senses" to be perceptual facts "we do not feel at liberty to avoid

acknowledging". But he does not develop the of implications of intuition in these remarks and the sense in which these contradict his notion of all cognition as inferential.

As for Mill, targeting Descartes does not accurately represent "intuitionism" at large, nor does it adequately portray the criterion of directness. Although Descartes' account of intuition is among the most detailed, intuition as direct apprehension is acknowledged as foundational by theorists who, at the very least, can be described as having a different agenda to Descartes, in that they are not committed to establishing the certainty of some subset of knowledge claims (Locke, 1690/1964, is a clear example.)

Secondly, Peirce, as Mill, fails to deal sufficiently with ambiguities arising from different senses of intuitive necessity. Particularly relevant to Peirce is the point made by Reid (1764/1975) (a reworking of which is evident in Wittgenstein, 1969) that "effects" understood in terms of actions, presuppose intuitive presumptions that are necessary only in the special sense that one could not act without them. These must be recognized as qualitatively different from inferences. Though they arise through sensory and, particularly social experience, they reflect understandings grasped in some form even in infancy through interactive involvement in the world. Peirce, then, as Mill, unnecessarily assumes an incompatibility between the notions of behavioral "effects" and direct knowledge.

Historical influences.

The notion of intuition as a "subjective" phenomenon (from Descartes) was embraced by "Romantic Idealists" as a means of acquiring direct knowledge of the nature of the Absolute (e.g. Fichte, 1794/1889), engendering a view of intuition as the purveyor of mystical/spiritual insights. In contrast, the "spiritual" flavor of earlier accounts of intuition had more to do with what is beyond understanding in the sense that it cannot be further explained, not that which gives knowledge of the nature of the Absolute. Romantic idealist understandings of intuition may be regarded, then, as a departure from the mainstream epistemological treatment of intuition and the function of direct apprehension.

Conclusions

The meaning of "direct" is far from clear, neurologically or epistemologically. But the essential assumption seems to be that direct apprehention is (by definition) not mediated by other events or processes. In principle, it entails a logically irreducible and cognitively fundamental grasp of some feature of experience, whether internal or external, physically "real" or socially constructed.

Nineteenth century arguments against the notion of direct cognition may provide some insight into the historical predominance of inferential explanations of cognition and what is arguably a lingering discomfort with the notion of directness. The intention here is not to dispute experimental evidence supporting indirect models or to downplay conceptual difficulties in the very notion of directness. It is to point to early influences on the notion of indirect cognition as fundamental, and to acknowledge the larger historical and epistemological contexts in which resistance to direct cognition may be embedded.

References

Ayer, A. (1968). The origins of pragmatism. London: MacMillan.

Bacon, F. *Novum Organum*, LXIV. In Hurd and Houghton (1878), The works of Francis Bacon, vol. 1. Cambridge, University Press.

Barnes, J. (1984) (ed.). The complete works of Aristotle, v. 1-2. Princeton: University Press.

Descartes, R. (1994). Rules for the direction of the mind (*Regulae ad Directionem Ingenii*). (J. Cottingham, R. Soothoff, and D. Murdoch, trans.). Cambridge: University Press. (Originally published 1684; Assumed written 1628).

Fichte, J. (1889). Popular works. W. Smith (Trans.).,London: Trubner. Originally published 1794).

Gibson, J. (1979). The ecological approach to visual perception. Boston: Houghten Mifflin.

Hammond, K. (1996). Human judgment and social policy. New York: Oxford University Press.

Harre, R. & Gillett, G. (1994). The discursive mind. Thousand Oaks, CA: Sage.

Kant, I. (1990). Critique of pure reason. Buffalo: NY: Prometheus. (Originally published 1781).
Klienmutz, B. (1990). Why we still use our heads instead of formulas: Toward an integrative approach. Psychological Bulletin, 107(3), 296-310.
Locke, J. (1964). An essay concerning human understanding. New York: William Collins. (Originally published 1690).
Mill, J. (1979). An examination of Sir William Hamilton's Philosophy, and of the principal philosophical questions discussed in his writings. J. Robson (Ed.). Toronto: University of Toronto Press. (Originally published 1865).
Ockham. (1957). Sentences. Philotheus Boehner, (Trans.). Edinburgh: Thomas Nelson and Sons.
Osbeck, L. (1998). Irrational unconscious inference: A critical look at intuition in psychology. Unpublished doctoral dissertation. Georgetown University, Washington, D.C.
Peirce, C. (1868). In Collected papers of Charles Sanders Peirce. C. Hartshorne and P. Weiss (eds. v. 1-6). Cambridge, MA: Belknap Press.
Reber, A. (1989). Implicit learning and tacit knowledge. Journal of Experimental Psychology: General, 118, 3, 219-235.
Reed, E. (1994). The separation of psychology from philosophy: Studies in the sciences of mind 1815-1879. In C. Ten (ed.). Routledge history of philosophy, vol. VII. London: Routledge.
Reid, T. (1975). An inquiry into the human mind on the principles of common sense. (K. Lehrer and R. Beanblossom, eds.). Indianapolis: IN.: Bobbs-Merrill. Originally published 1764.
Reid, T. (1975). Essays on the intellectual powers of man. (K. Lehrer and R. Beanblossom, eds.). Indianapolis: IN. Bobbs-Merrill. Originally published 1785. Routledge.
Wittgenstein, L. (1969). On Certainty. (G. Anscombe and D. Paul, trans.). Oxford: Blackwell.

Graham Richards

History of Psychology in the United Kingdom: Some Reflections on the Establishment of The Centre for the History of Psychology (C.H.O.P) at Staffordshire University

Staffordshire University

Summary

Despite the importance and distinctive character of British Psychology, the study of History of Psychology has remained relatively undeveloped by comparison with the United States, Germany, the Netherlands and Canada. While the History of the Human Sciences group at Durham has made an invaluable contribution over the last decade, particularly in establishing the journal *History of the Human Sciences*, we have lacked a properly resourced research centre for History of psychology. CHOP, which is opening for business in September 1998, is intended to redress this situation. The present paper will offer some general thoughts on the state of the sub-discipline in Britain and describe the CHOP initiative and our hopes for its future.

Graham Richards

Imagining Life Without Freud

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Summary

If history of Psychology is also the history of its subject matter then historians of Psychology bear a special responsibility to undertake some kind of imaginative hermeneutic effort. The period between the Two World Wars has recently been recieving increased attention and several people with whom I have talked about this agree that recapturing the Zeitgeist (if I dare use the term) of that period is now strangely difficult - more so in many respects than the 19th century. Recently tackling the popularisation of Psychoanalysis in Britain during these two action-packed decades I was particularly struck by the need to understand the atmosphere of the immediate post-Great War period, say 1919-1925. This required imagining living in a world which was in some respects recognisably "Modernist" but one in which psychoanalytic language had yet to penetrate everyday psychological language (or "folk psychology"). What then was it like to be living without Freud and encountering psychoanalytic ideas and language for the first time? What was it about these ideas, this language, that was so immediately seductive? What needs was Psychoanalysis meeting? I suggest that in order to ascertain this we have to grasp what it was like to experience those years during which collective "post-traumatic stress disorder" (as it were) and utopian scientific optimism were so peculiarly fused. To do this there is, in the end, no other method but immersion in the sundry primary sources.

Irmingard Staeuble

Colonialism, Culture, Human Sciences: The Debate on Decolonizing Knowledges and Minds

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Summary

Recent debate on colonialism and culture, ramified among comparative literature, historiography, anthropology, and cultural studies, shows signs of renewed attempts to decolonize knowledges and minds. In my paper I take into view the ensemble of the human sciences, from the 1940s to the present, trying to trace the meandering path of the critical debate on colonialism. I discuss crucial phases of this debate in terms of participants, the politics of theory involved, and the kind of scholarship generated. Special emphasis will be on the interplay between European and non-European participants, i.e. intellectuals and scholars from empire and colony background, and amount to the proposal that it was "Europe's Others" who not only initiated but also renewed the thematic foci of the debate.

Recent debate on colonialism and culture, ramified among comparative literature, historiography, anthropology, and cultural studies, shows signs of renewed attempts to decolonize knowledges and minds (Dirks 1992; Carrier 1992; Breckenridge And Veer 1993; Barker & Iverson 1994; Thomas 1994; Young 1992, 1995; Kaplan 1995). According to a widely shared premise that colonialism and European culture are deeply implicated within each other, decolonizing European thought is not seen as an issue of erasing from it colonial thinking but rather as an issue of radical restructuring of European systems of knowledge.

In this paper I go beyond previous concerns with decolonizing anthropology (Staeuble 1992, 1995, 1996, 1997), taking into view the ensemble of the human sciences, from the 1940s to the present. The pattern I have arrived at so far is one of a meandering of the critical debate on colonialism, sharpening at some points to pleas for mental decolonization and petering out again in subtle scholarship. I will discuss selected crossroads of this debate in terms of participant interplay, the politics of theory (premises and envisaged aims) involved, the kind of scholarship generated, and the structure of academic production implicated. Special emphasis will be on the interplay between European and non-European participants, i.e. intellectuals and scholars from empire and colony background, and amount to the proposal that it was "Europe's Others" who not only initiated but also renewed the themes of the debate. .

If the narrative of colonialism and culture has "a rich array of characters and situations but a simple plot" (Asad 1991, 314), the same holds true for the critique of colonialism, culture, and human science involvement. Subsequent to a polymorphous beginning, there has been but one major shift of thematic configuration: that is the shift from domination and exploitation to knowledge, power, and imagination; from oppression and resistance to the discursive production of a non-European world for both the West and the cultures thus represented; from the stated aim of unmasking human science involvement in imperialist affairs to that of displacing the very categories that constitute colonial discourse. As easily noticeable, the inspiration behind this shift is from a Marxist to a Foucauldian paradigm.

African-French beginnings: intellectuals in Paris

To begin with the challenge from colonized intellectuals one may begin in Paris in the 1930s where a group of French-speaking black students, among them Léopold Senghor of Sénégal and Aimée Césaire of Martinique, articulated their experience of degradation by the universalism of a civilizing mission that means but French liberty, culture, and bread (cf. Senghor, cit. in Sorum 1977, 212) and turned to a search for black dignity. Césaire's poem of "négritude" (1939) set the themes of a movement that spread after the war and found strong support from leftist French intellectuals who, sensitized by fascism, turned into combatting racism. According to Sorum, "when Diop's journal *Présence Africain* was launched in autumn 1947, with the aim of presenting its African and French readers with both scholarly studies about Africa and literary texts by African writers", its

committeee of patronage included André Gide, Sartre, Camus, Leiris, "as well as Césaire, Senghor, and the black American novelist Richard Wright" (1977, 219). There was considerable tension in this interplay as most French intellectuals were unwilling to grant négritude the concrete reality claimed by the black writers. As far as they valued and attibuted to all blacks the features of emotion, rhythm, and intution, did they not adopt the racist stereotype of the black and turn their supporters into "antiracist racists" (Sartre)?

To cut a very complex story short, this ambiguity led the Négritude movement to African nationalism and universalistic-minded supporters like Sartre to Marxism, and it still resonated in the interplay of a younger generation of black students with both Négritude writers and European intellectuals. When still a medical student in Paris, Frantz Fanon of Martinique felt desperate after reading Sartre's *Orphée noir* (1948), "robbed of my last chance ... to live my Negrohood" (cit. Sorum, 223). Yet soon Fanon (1952) would himself criticize the n,gritude movement as stuck in a reaction to white racism and argue in favour of psychological revolt, later (1961) of political and social revolution.

It was Memmi's and Fanon's radical critiques of colonialism that set the lasting task of analyzing the structure of colonial relationships. In his essay *Portrait of the Colonized Preceded by a Portrait of the Colonizer* (1957), Alfred Memmi, an Italian-Arabic Jew of Tunis, presented a sharp characterization of colonization's inevitable disfiguring and corruption of both colonizers and colonized. He ends with the message that "there is no way out other than a complete end to colonization" (1965/1991, 150). Memmi envisages a national liberation "as a prelude to complete liberation" (151) that was to render the once colonized into a free human.

At the height of the Algerian war for independence, Frantz Fanon who worked in Algeria as a psychiatrist, quit to join the FNL. As is well-known, his book *The Wretched of this Earth* (1961) presents a relentless Manichean picture of colonial space divided into bright European city and dark kasbah, underlining the charge that European wealth and progress, at home and abroad, had been built upon the sweat and corpses of Negroes, Indians, Arabs and Asians. To European leftists, the oppressed by capitalism peoples of the earth were rendered visible by this concrete voice, and from hindsight his envisaged as necesary passage from nationalist liberation to a transnationalist social and political consciousness assumes nearly prophetic quality with regard to the problems of Third World nationalism.

Fanon's manifesto was strongly supported by Sartre, yet scorned by liberals for both his irreconcilable condemnation of colonialism and for what was read as a plea for violence. His notion of transnationalist social and political consciousness recommended him to the internationalist vision of liberation in the New Left and for some radicals in the 1960s Fanon reshaped the view of metropole and periphery, but it was to take a long time until the polymorphous structure of his argument was discussed seriously (Said 1993; Parry 1994).

Colonialism and anthropology: radical scholarship

Critical reflection on anthropology's involvement in colonialism started with the publication of Michel Leiris' essay on Ethnography and Colonialism (1950) in *Les Temps Modernes*. Leiris who in the 1930s participated in Griaule's expedition from Dakar to Djibouti but became increasingly disenchanted, explicitly states in this essay a compliance - if unintentional - of ethnography with colonialism. As an alternative, he proposed that the ethnographer is to act as an advocate of the self-defined interest of the societies studied. Stressing the transformational character of these societies, he argued for a realist ethnography concerned with the everyday life of a society, i.e. the relationship between natives and Europeans and its mutually demoralising effects. In favour of bilingual education both to minimize alienation and to foster self-consciousness among the colonized, Leiris ended with a plea for joint struggle against bourgeois exploitation and oppression.

When, in the late 1960s, the charge of complicity was reiterated in Britain and the US, in terms of anthropology as the "child" or "daughter" of "Western imperialism" (Gough 1968), and a form of "scientific colonialism" (Galtung 1967), concern with the practical complicity of anthropology with colonial governments dominated the agenda as is documented in both Leclerc's (1972) analysis of indirect rule and Asad's (1973) edition of selected papers from the International Congress of Anthropological and Ethnological Sciences in Chicago. First results proved somewhat meagre yet seemed to suffice because no further evidence was provided.

This was the period of radical critique of positivism in the human sciences, of attempts at Marxist and Marxist-feminist gearing of the disciplines toward an unconcealed political project - the emancipation of the oppressed. Analyses of monopoly capitalism, dependency theory, the political economy of knowledge production, and history from below flourished, and new ventures into participant or action reserach gained momentum. As remembered by Ann Stoler, the impact both of colonialism "on various domains of indigenous agrarian structure, household economy, kinship organization, and community life" (1992, 319) and of colonial politics on the theory and method of ethnography were focal in anthropology. Soon, however, Marxist and feminist claims to speak from the viewpoint of the oppressed found themselves in turn charged with concealing "a politics of knowledge, a claim to authority which gave Western academics the power to define problems and solutions" (Pels & Nencel 1991, 10). Anti-imperialist theory, the last grand narrative, turned into a chimera as soon as an increasing number of black US women, male and female Third World intellectuals claimed their own voice as critical subjects able to theorize their experience and to design paths toward emancipation.

The radical debate on colonialism and anthropology was to recede in favour of reinventions of the discipline. The first voice of a South Pacific Islander accessible in international publications (Hau'ofa 1975; 1979) and the attempts of Papua New Guinean newcomers on the scene of independence at decolonizing anthropology (Morauta 1979) went nearly unnoticed.

Orientalism and Colonial Discourse

A new impulse came from the field of comparative literature, with the publication of Edward Said's critique of Orientalism (1978). In this book Said, a Palestinian educated in Egypt and the US, took up a body of classical scholarship, providing rich evidence for the establishment of a "cumulative and corporate identity of Orientalism" that "drew its strength from the mix of traditional learning (classics, Bible, philology), public institutions (governments, trading companies, geographical societies, universities) and general cultural writing (travel books, fantasy, exotic description)" (205). Thus demonstrating that the "Orient that appears in Orientalism emerged as a distillation of essential ideas about the Orient - its sensuality, its tendency to despotism, its aberrant mentality, its habits of inaccuracy, its backwardness (205), Said put forward his thesis that orientalism was a means of the West to represent the other of itself, part of the strategies to reproduce occidental global power.

Said's point that the Orient is no natural "given" but rather produced by the very categories of our cultural and human science knowledge as shaped by colonial rule generated a new focus on the discursive practices of "othering". Concern with the modes of representing the Orient - style, figures of speech, setting, narrative devices, historical and social circumstances - even overshadowed the more pressing question of the very possibility of representing other cultures raised by Said: "What is another culture? Is the notion of a distinct culture (or race, or religion, or civilization) a useful one? Do cultural, religious, and racial differences matter more than socio-economic categories, or politicohistorical ones?" (1978/1995, 325)

At the 1982 annual Sociology of Literature Conference in Essex, Homi Bhabha elaborated on Said's argument and coined the notion of "colonial discourse". Two years later, the conference on "Europe and its Others" saw Said and Bhabha in controversy, Bhabha contesting Said's view that power and discourse is possessed entirely by the colonizer, a view soon revised by Said himself who in *Culture and Imperialism* (1993) proposed a complex interpretation colonial vision, resistance, and chances of liberation.

Orientalism, and Occidentalism, Colonial and Counter-Colonial Discourse

As the debate on Orientalism generated into a debate on the means and strategies of Othering (Fabian 1983), attention was also drawn to Occidentalism as the implied construction of a WE. Olivia Harris (1991) in asking "Who are >WE<?" points to the interchangeable use of terms like "Europe", "the West" or the "Judaeo-Christian tradition" that "invoke a shared set of meanings grounded for some in the Enlightenment, for others in Greek philosophy, but always abstracted from real social relations" (155). Soon elaborated by James Carrier, Harris' plea was for "anthropologizing ourselves", for instance by distinguishing the historical construction of "objective time" from its political and ideological uses, and "from the multiplicity of time concepts and experiences which fashion the lives of men and women in real places in Europe" (1991, 159). In a similar vein, Roger

Keesing (1994) reinterpreted his puzzling fieldwork experience in Melanesia where he encountered a group of Kwaios who met regularly on Tuesdays concerned with discussing and writing down KASTOM and addressing some as "chiefs", though for the rest of he week they lived their customs in a clearly anti-hierarchical society. Further exploration of "how colonial discourse has imposed categorical structures and specific forms on the counter-hegemonic discourse of resistance and decolonization" (41-42) would thus suggest itself.

Flourishing scholarship, marginal interplay

As scholars of colonial history, anthropology, and literature took up the concepts of Orientalism and Colonial Discourse, they soon noticed a danger of totalization that might tend to ignore the heterogeneity of local and temporal colonial projects. Rich evidence has since been provided of the the array of sites, situations, characters, and cultural technologies of control that make up colonialism's culture. For instance, contributions to *Colonialism and Culture* (Dirks 1992), *Colonial Situations* (Stocking 1991), *History and Tradition in Melanesian Anthropology* (Carrier 1992) emphasize the transformations of social identity in both the colonized and the colonizers in colonial cultures that were not simple transplantations but new constructions of Europeanness (Stoler 1992, 321), the active agency of the colonized, and the different visions of the "civilizing mission" held, for instance, by governors and missionaries. *Orientalism and the Postcolonial Predicament* (Breckenridge and van der Veer 1993), a volume resulting from a South Asia conference at Pennsylvania University, documents in detail the uses of Orientalist knowledge in the colonial administration of India, including the cultural politics of language and literature, which is also the main concernn in *Colonial discourse/postcolonial theory* (Barker, Hulme & Iversen 1994).

As noted by Dirks, "colonialism is now safe for scholarship" (1992, 5). Yet who participates? Prominent scholars like Said and Bhabha underline that they are not and do not speak as Western intellectuals. Spivak, the former co-editor of the Indian "Subalternm Studies" and promoter of a "decolonisation of the imagination" (Spivak 1992, 129) and the Indian historian Ashis Nandy are well-known. A few more names from literature studies could be listed.

Editorial comments show some unease with categorizations that range from "postcolonial" to "diasporic" intellectuals. The problem of categorizing these welcome others reminds of still other "others" as at least indicated by Dirks:

> "what does it mean that Edward Said, or Ranajit Guha and the Subaltern Studies collective of Indian historians, take the very same texts by Gramsci, Foucault, or Williams as fundamental that are recited everywhere else in the Western academy ... (while) in provincial universities in Asia and Africa ... these theorists would all signify elitist forms of exclusion, new Western forms of domination" (Dirks, 1992, 12)

Concern with the international structure of academic production is still marginal: "what does this imply about the voice and ground of our own academic and political practices" (Dirks 1992, 12)? So far, the "paradox" stated from a Third World perspective "that it takes anticolonial struggles to produce neocolonial conditions" (Pechey 1994, 153) have remained a puzzle for critics on various sides. There is still a long way to go in decolonizing knowledges and minds.

References

Asad, T. (ed.) (1973) *Anthropology and the Colonial Encounter*. London

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.

Ashcroft, B.; G. Griffiths & H. Tiffen (eds) (1989) *The Empire Writes Back: Theory and Practice in Post-Colonial Literatures*. London: Routledge

Barker, F., Hulme, P., & Iverson, M. (eds.) (1994) *Colonial Discourse - Postcolonial Theory*. Manchester: Manchester University Press

Breckenridge, C. A. & van der Veer, P. (eds) (1993) *Orientalism and the Postcolonial Predicament. Perspectives on South Asia*. Philadelphia: University of Pennsylvania Press.

Carrier, J. G. (1992 b) Occidentalism: the World Turned Upside-Down. *American Ethnologist* 19:195-212.
Carrier, J. G. (ed.) (1992 a) *History and Tradition in Melanesian Anthropology*. University of California Press
Dirks, N. B. (ed.) *Colonialism and Culture*. Ann Arbor: The University of Michigan Press.
Fabian, J. (1983) *Time and the Other: How Anthropology Makes its Object*. New York: Columbia University Press.
Fanon, F. (1961/1968) *Die Verdammten dieser Erde*. Frnkfurt: Suhrkamp.
Galtung, J. (1967) Scientific colonialism. *Transition* 30: 11-15.
Gough, K. (1968) New proposals for anthropologists. *Current Anthropology* 9: 403-407.
Harris, O. (1991) Time and Difference in Anthropological Writing. In L. Nencel and P. Pels (eds.) *Constructing Knowledge: Authority and Critique in Social Science*, pp. 145-161. London: Sage. i
Hau'ofa, Epeli (1975) Anthropology and Pacific Islanders. *Oceania* 45(4): 283-289.
Hau'ofa, E. (1982) An anthropologist at home. In F. Hussein (ed*) Indigenous anthropology in non-Western Countries*. Durham, N.C.
Kaplan, M. (1995) *Neither Cargo nor Cult: Ritual Politics and the Colonial Imagination in Fiji*. Durham: Duke University Press.
Leclerc, G. (1973) *Anthropology and Colonialism* (German transl. Munich: Hanser)
Leiris, M. (1950) Ethnographie und Kolonilismus. In: M. Leiris, *Die eigene und die fremde Kultur. Ethnologische Schriften* I. Frankfurt: Suhrkamp 1985.
Memmi, A. (1957/1990) *The Colonizer and the Colonized* (transl.Greenfeld). London
Morauta, L. (1979) Indigenous Anthropology in Papua New Guinea. *Current Anthropology* 20, 561-576
Parry, B. (1994) Resistance theory / theorising resistance - or two cheers for nativism. In Barker, Hulme, & Iverson (eds.*) Colonial Discourse - Postcolonial Theory*, pp. 172-196. Manchester: Manchester University Press
Pechey, G. (1994) Post-apartheid narratives. In Barker, Hulme, & Iverson (eds.*) Colonial Discourse - Postcolonial Theory*, pp. 151-171. Manchester: Manchester University Press
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.
Said, E. (1978, 1995) *Orientalism. Western Conceptions of the Orient*. London: Routledge & Kegan Paul.
Said, E. (1993) *Culture and Imperialism*. New York: Alfred A. Knopf.
Sorum, P.C. (1977) *Intellectuals and Decolonization in France*. Chapel Hill: The University of North Carolina Press.
Staeuble, I. (1992) Wir und die anderen - ethnopsychologische Konzepte im Wandel. Geschichte und Psychologie 4: 140-157.
Staeuble, I. (1995) Subjektivit„t 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. (1996) 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.
Staeuble, I. (1997) Decolonizing Anthropology - Toward what Prospects? Paper presented to the Anthropology Department, University of Papua New Guinea
Stocking, G. W. Jr. (ed) (1991*) Colonial Situations*. Madison: The University of Wisconsin Press. (History of Anthropology, vol. 7)
Thomas, N. (1994) *Colonialism's Culture: Anthropology, Travel and Government*. Cambridge: Polity Press
Young, R. (1992) Colonialism and Humanism. In J. Donald and A. Rattansi (eds.*) Race, Culture, and Difference*, pp. 243-251. London: Sage.

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From George Washington to Myself: A Century of Research on Children's Ideals

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Summary

Around the turn of the century, large-scale questionnaire studies of children's ideals were popular both in America and Western Europe. We trace the fate of these and similar studies from 1898 until today, and report changes in children's answers, in the way questions have been asked, and how the answers have been catgorized and interpreted. Finally we report the result of four contemporary studies which have repeated the original question of "who would you like most to be like" to adolescents and young adults. The studies show a development from historical and national 'great men' to glamorous media figures and insistence on being like nobody but oneself. They also reveal stable sex differences over time (despite an increased availability of female models). Researchers appear to have changed their opinions on the relative value of public vs. private models, but may have exaggerated the influence of glamorous models on adolescents' views of desirable person characteristics.

Four ages of hero worship

I. 1898-1916: The great men. In the new wave of questionnaire studies on child development, instigated by Stanley G. Hall at Clark University around the turn the century, large-scale studies of children's ideals became one of the favorite topics. These studies are very characteristic of what Kurt Danziger (1990) has described as "the Clark method" of scientific investigation: A great number of subjects studied in a very brief time interval, yielding large amounts of quantitative data, to be averaged over subjects. The program intended to yield objective, and, with a modern phrase, 'ecologically valid' observations, drawn from children's lives in non-laboratory settings, with the purpose of discovering developmental regularities, hopefully reflecting evolutionary important trends (White, 1990).

The first study on ideals was conducted by Estelle M. Darrah (1898), who asked 1,440 school children, aged 7-16, from San Mateo County, California, and St. Paul, Minnesota, the following simple question: "What person of whom you have ever heard or read would you most wish to be like? Why?" This study was described by a subsequent investigator (Chambers, 1903) as "the pioneer study on this topic, the first of scientific value, and it has marked out the lines on which practically all subsequent studies have been conducted" (p.140). There was no shortage of subsequent studies. Earl Barnes (1900) posed the same question to 2,100 London 8-13 year olds, and later to 1,900 children in New Jersey. Catherine I. Dodd (1900) conducted a second English study with 700 children, A. Young conducted an unpublished study on 2,500 Scottish children, and J. Friedrich published in 1901 the first German study. When Chambers (1903) published a new study on the ideals of 2,500 6-16 year olds in Pennsylvania, the subject appeared to be so thoroughly researched that he felt he had to start with an apology, why, "at this late day", a new study on children's ideals should be offered for publication. But his plea for why these studies should continue in all parts of the world appeared to be well accepted. New studies continued in the next decade to appear in USA (Goddard, 1907; Hill, 1911), Germany (Lobsien, 1903; Meumann & Hösch-Ernst, 1907, Richter, 1912), and, finally, in Sweden (Brandell, 1913), Norway (Reymert, 1916), and Denmark (Lehmann, 1916).

Despite the diversity in locations and populations, all these studies show some remarkably similar patterns of results, concerning age as well as sex differences.

The youngest children in all the studies tended to choose their ideals among personal acquaintances, like parents, relatives, and playmates. These choices decreased steadily in higher age groups, being gradually replaced by the category of Public Characters. Most of these were famous people from the national history. In the American samples, George Washington emerged as a clear number one, followed by other presidents: Lincoln, McKinley, and Roosevelt. Generals Grant and Lee from the civil war were also often mentioned, along with the poet Longfellow.The English

studies showed a more scattered picture, with Gladstone, Nelson, Florence Nightingale and members of the royal family on the top of the list. In Norway, the most popular character was an adventurous war hero from the 1700's (Tordenskjold), followed by the national poets Wergeland and Bjørnson. Religious characters, especially God and Jesus, formed a special category with high variations from sample to sample. In some samples, answers in this category increased, in others they decreased with age.

All investigations showed strong sex differences. First of all, a high percentage of girls (between 25 and 50%), as opposed to boys, chose ideals of the opposite sex. This tendency appeared to grow with age, because of the far greater availability of male public figures. The London sample was an exception, they had Queen Victoria and Florence Nightingale, whereas American girls had a rather poor substitute in mrs. Washington, mrs. Lincoln, or another first lady. Second, girls chose more often personal acquaintances and religious characters, and were slower than boys in developing a preference for public and historic figures. In this category, girls tended to a greater extent than boys to choose poets, artists, and composers. Most researchers commented on the sex differences as a matter of great concern. History, contemporary society, and the schools were all to blame for not providing the female half of the population with appealing and appropriate heroines. And the girls were eyed with suspicion because they tended lag so far behind the boys in developing a preference for public ideals, a development that was so general that it acquired a normative status, reflecting broadened horizons characteristic of the maturing youth.

II. 1920-40: Clara Bow and Charles Lindbergh. After this initial burst of investigations, and wealth of results, the study of children's ideals was brought to an abrupt end around World War I. Perhaps the topic was regarded as exhausted, or perhaps the general disillusionment following the war contributed to less favourable conditions for a continued study of ideals. An study in India by Hoyland (1926) attempted to show that the Indian child is more susceptible to religious and ethical ideals than the more materialistic oriented western children. A few new American studies appeared in the 1930's, one by David S. Hill (1930), one of the early pioneers in the Clark questionnaire studies, and another by a catholic nun, Sister Mary Phelan (1936). Both studies essentially replicated the developmental trends and sex differences of earlier studies. In Hill's study of 8,813 Alabama children, we can detect a turn towards more contemporary ideals, with Charles Lindbergh coming second after Washington for boys, and Washington, Clara Bow and Lindbergh leading for the girls. Sister Phelan changed the question by asking: "Who is your ideal?" As a response to this, she received more religious answers than in most earlier investigations. Meltzer (1932) asked 200 American "problem children": Who is the greatest man who ever lived? The answers were, rather stereotypically, Washington and Lincoln (in all 64%), with Jesus on the third place.

Returning to an early idea by Goddard (1912) about "negative ideals", Schmeieng (1934, 1935) explored in Germany the question of which persons children would prefer not to resemble. These Counter-ideals could, according to his opinion, be equally important for personality development than were the positive ideals.

Even if the general picture painted by these studies were quite similar to the ones conducted 20-30 years earlier, we may detect some changes in the emphasis on contemporary figures, many of them known from movies and newspapers, rather than from the history books and the school. Especially the girls appear to have received more female celebrities to look up to (Clara Bow, Mary Pickford, Shirley Temple and more). Equally interesting is that a number of boys and girls did not give an answer fully in accordance with the directions, but described an abstract or composite ideal rather than naming a particular person.

III: 1945-1960: The Composites and the Glamorous. Both these tendencies reappear in studies conducted after World War II. Stoughton and Ray (1946) studied children in grades 2, 4, and 6 and replicated the broadening of horizons trend. Havighurst, Robinson, and Dorr (1946) introduced a methodological innovation by replacing the fifty year old question about a person mentioned by name, with the more flexible question: "Describe in a page or less the person you would most like to be like when you grow up. This may be a real person, or an imaginary person. He or she may be a combination of several people ..". Havighurst et al. found that their oldest subjects (adolescents above 16) gave most of their responses in the form of a composite imaginary person.

They accordingly regarded this answer as more mature than naming a particular person. It is also of some significance that they reshaped the traditional category of Public figures (usually divided into a historical and a contemporary subcategory) into Heroes ("people with a substantial claim to fame, usually tested by time") and Glamorous adults (movie stars, athletes, imaginary characters like Superman etc). It turned out that the historical Heroes (like Lincoln and Nightingale) were a dying race (5-10%), compared to the Glamorous (20-40%), who reached their highest popularity between 10 and 16.

The Havighurst study was replicated in several populations (Havighurst & Taba, 1949), including New Zealand 13-15 year olds (Havighurst & MacDonald, 1955) and a parallel sample of Western Australian youth (Wheeler, 1961). In all these studies, the group of Historical heroes is almost extinct, the Glamorous decrease from 20% at age 13 to about 10% at age 15, whereas the Composites form the most frequent category, covering more than 50% of the ideals from 15 onwards. Ideals of the "glamorous" sort appeared to increase their appeal, and at least their availability, with the advent of television. Campbell (1962) surveyed a large sample of 12 and 15-year-olds in Sydney few months before the introduction of television in Australia in 1957, and then again three years later, finding an increase in glamorous adult ideals from about 5% to 15% over this critical three year period.

IV: After 1960: Myself. With the demise of historical heroes, to be replaced on hand by more ephemeral media celebrities, and on the other, by more abstract, composite self ideals, on the other, interest in the study of children's ideals waned. In the thirty-odd years since the early sixties, only a couple of studies can be found. Simmons and Wade (1985) compared answers from English and Continental children to the question: "What sort of person would you like to be like". Notice that this question, like the question posed by Havighurst and his successors, does not expressly ask for an identifiable person. In line with this, Simmons and Wade found that nearly half of their 15 year old informants did not come up with named models. Still more interesting, nearly 20% asserted that they primarily wanted to be like themselves. This is a new category of answers. Going back to the earlier studies, we discover that the answer itself is not completely new. It is noted by Chambers (1903) as a very rare answer, given by about 0.5%. Havighurst referred to it as one of the more common answers in the "Miscellanous" category, and regarded it with suspicion as a possible manifestation of "unconscious resistance to recognizing the nature of the ideal self" (p. 257). Wheeler (1961) found a somewhat greater number of such answers (about 5%), allocated to the Non Classifiable category. Not completely endorsing Havighurst's interpretation, Wheeler thought that they could alternatively be thought of as the "shortest and easiest way of satisfying the investigator without going to the length of refusing the answer" (p . 167). What is interesting here is two things: the increased frequency of self answers, and the changed awareness of these answers on the side of the researcher. It is not implausible that the new ideology of self-acceptance, so strongly and sometimes aggressively promoted in the new wave of humanistic psychology from around 1960, has contributed to the increasing number of such responses, and even more, to its growing recognition by the researchers. Bull (1969) found that up to 40% of his sample of English 15-17 year olds preferred to be themselves, but that was after calling their attention to the possibility that their chosen ideal "may be yourself".

So far, this overview has shown a simultaneous change in the content of the answers, in the way the questions are posed, and in the way responses are interpreted. To highlight these changes, we will briefly report four recent studies which have attempted in different ways to present modern subjects with the original task of naming a person one would like to resemble.

Four contemporary replications

I. Norwegian young adults 1916-1988. The studies reported above focused exclusively on school children, below 16-17 years of age. However, Martin L. Reymert had in 1916 performed a parallel study on 800 Norwegian teachers' college students, aged 18-25. The question posed was simply: "Which person would you most like to be like, and why?" The answers were in line with the established developmental trends, 95% of the men and 68% of the women naming public figures, with famous writers as the preferred category. It also appears that world history, with Shakespeare,

Sokrates, Napoleon, Luther, and Darwin, had acquired a greater appeal than the more local models from national history (Reymert, 1918).

As a small-scale modern parallel, Storå and Teigen (1990) asked 60 teacher's college students in Bergen, Norway, the same question. The students were informed that this was, in fact, a replication of an old study. Despite of this, several students objected to the question, which they found silly, contrived, and difficult to answer. About 13% mentioned personal acquaintances, 28% public characters (mostly contemporary), 20% described trait composites, 30% wanted to be like themselves, as they were, and another 17% themselves, with minor or major improvements. This study gave us a strong indication that not only the models have changed, but even more the attitudes towards the models. Despite the question, that clearly asks for concrete others, most answers fall in the "new" categories of composites and myself.

2. Australian teenagers in 1988. In a study, primarily motivated to study the effects of TV and media figures as positive and negative role models for children, Julie M. Duck (1990) asked 11-15 years old Australian children to make a list of up to three people "you would most like to be like if you could". They were then given a hint that these may be family members, friends, people seen on TV, or read about. "They could be famous people, heroes, superheroes, sports stars, musicians, pop stars, or maybe even imaginary people .." (p. 21). After answering this question, they were similarly asked to make a list of people they would "least like to be like".

For this sample, the question of listing people seem to have reduced the number of Myself-responses (1-2%, mostly with girls), and composites (3-4%).
The dominating category is now called Media figures, encompassing more than 75% of the boys' answers and 45%-63% of girls answers, the number increasing with age. For boys, this category consisted of sports stars, actors like Sylvester Stallone, Arnold Schwarzenegger, and Superman, followed by pop stars. Girls in the youngest groups preferred pop stars, the oldest models (Ellen Macpherson) and film stars. Same-sex ideals were chosen by practically all the boys and 83% of the girls.

With the question of negative ideals, the picture was reversed. Media figures accounted for 25-35% of the selections, whereas people known personally became the majority. Duck concludes with some concern that media figures appear to have "taken over" as the prime source of ideal models, and than, in comparison, real acquaintances are viewed less positively.

It is interesting to notice the changed evaluation of personal acquaintances, who at the beginning of the century were regarded as a developmentally early phenomenon, to be replaced by public (hopefully worthy) models. In Duck's study, the Public category is renamed Media figures. Most of these are clearly similar to the Glamorous who accounted for 10-20% of the models around 1950-60. Naturally, a three- or fourfold increase in this type of superficial and commercial ideals in the course of a thirty years period looks like a revolution and a cause for alarm.

3. Norwegian 16-18 year olds in 1994. As a part of an extensive questionnaire presented to a large, representative sample of urban and rural schools in western Norway, Helland and Bjørkheim asked 2,572 Norwegian 16-18 year olds the following question: "Who would you most like to be like? Mention someone who has the qualities you would prefer to have". They were then in two separate questions asked to list three qualities they valued particularly highly, and to name adults who, to their knowledge, possessed some (or all) of these qualities.

The results showed an exceptionally large gender difference, in that nearly half of the boys named a public figure, against 13% who named acquaintances. In contrast, one third of the girls chose acquaintances, and only 17% a public figure. Boys chose sports stars and action movie heroes (Schwarzenegger and van Damme), with Albert Einstein, perhaps surprisingly, not far behind; girls chose Hollywood actresses and models. But in addition, about 15% (both boys and girls) answered "Myself". Another 10% wrote "Nobody".

The question of highly valued qualities disclosed less superficial and glamorous ideals. Most frequently mentioned were moral values, like honesty and loyalty, then came a group of social values (outgoing, easy to get along with), empathy (warmth, a good listener), prosocial qualities (helpful, caring), humor, and finally: intelligence, talents, and physical strength and appearance. The qualities displayed by the most popular media figures were, in other words, near the bottom of the list. So when the informants finally were asked to give examples of persons displaying their most

valued traits, they almost never mentioned a celebrity, but rather a friend, a parent, or another highly respected adult they knew personally.

4. Norwegian 13-14 year olds in 1996. In the two Norwegian investigations reported above, respondents were older than in most previous studies. It is therefore difficult to conclude whether, for instance, the high proportion of Myself answers is a function of cohort or age. In the Australian study, such answers were relatively infrequent, which however may be attributed to procedural differences (the respondents were asked to list several models, with some hints given in the instructions). We decided therefore to run a new study on a population of 7th graders in a town in Northern Norway.

Altogether 216 13-14 year olds answered the question: "Who will you most like to be like"? Half of them were then given the traditional follow-up question: ".. And why"? The other half were asked the questions about personal qualities, described in the previous study (for details, see Normann, 1997).

Fifty percent of the boys, and 38.5% of the girls preferred to be like public figures. These were, like in the two above-mentioned studies, mainly taken from sports, movies, and other media. True to the ephemeral nature of popularity, Rambo, Schwarzenegger and Claudia Schiffer are already exchanged with more recent media idols. Only 10% chose acquaintances. But even in this young, but rather self-assertive group, 25%, from both sexes alike, chose Myself.

In line with this, the most popular reason for choosing a model, turned out to be this model's capacity to be himself/herself. At the second place came the model's particular talents and skills (mainly boys), and at the the third, physical properties (mainly girls). Those who were asked to give a separate list of highly valued traits preferred social qualities (both sexes), moral qualities (mainly girls), skills and abilities (boys), and humor (girls). Again, appearance and physical qualities were ranked rather low. Those coming most close to possessing the cherished qualities were acquaintances rather than media idols.

We are then in the position to compare a sample of Norwegian teenagers to their great-grandparents given the same questions 80 years ago. Some sex differences appear to have gone (both girls and boys mention exclusively same-sex models), but only to reappear later (in the 17 year sample, 1/3 of the girls mention male models). Historical heroes are exchanged by contemporary glamorous media figures. Yet, the qualities appreciated most highly, appear to be of a more traditional kind, to be found more often in parents and friends than in media idols. Perhaps the most interesting and important change is the new insistence upon being oneself. This value is probably shared by a great number of psychologists, and may have been a contributing factor to the contemporary scarcity of studies of this kind.

References

Barnes, E. (1900). Children's ideals. Pedagogical Seminary, 7, 3-12.

Bull, N. J. (1969). Moral judgment from childhood to adolescence. London: Routledge and Kegan Paul.

Brandell, (1913) Svensk Arkiv för Pedagogik, 1, 109-120.

Campbell, W. J. (1962). Television and the Australian adolescent. Sydney: Angus and Robertson.

Chambers, W. G. (1903). The evolution of ideals. Pedagogical Seminary, 10, 101-143.

Darrah, E. M. (1898). A study of children's ideals. Popular Science Monthly, 53, 88-98.

Dodd, C. I. (1900). School children's ideals. National Review, 24, 875-889.

Duck, J. M. (1990). Children's ideals: The role of real-life versus media figures. Australian Journal of Psychology, 42, 19-29.

Friedrich, J. (1901). Die Ideale der Kinder. Zeitschrift für Pädagogische Psychologie, 3, 38-64.

Goddard, H. H. (1907). Die Ideale der Kinder. Zeitschrift für experimentelle Pädagogik, 5, 156.

Havighurst, R. J. & Macdonald, D. V. (1955). Development of the ideal self in New Zealand and American children. Journal of Educational Research, 49, 263-273.

Havighurst, R.J., Robinson, M.Z. & Dorr, M. (1946). The development of the ideal self in childhood and adolescence. Journal of Educational Research, 49, 241-257.

Havighurst, R. J. & Taba, H. (1981). Adolescent character and personality. New York: Wiley.

Hill, D. S. (1911). Comparative study of children's ideals. Pedagogical Seminary, 18, 219-231.

Hill, D. S. (1930). Personification of ideals by urban children. Journal of Social Psychology, 1, 379-393.
Hoyland, J. S. (1926). An enquiry into the comparative psychology of Indian and western childhood. Indian Journal of Psychology, 1, 45-47.
Lehmann, A. (1916) Om Børns Idealer. Det Kongelige Danske Videnskabernes Selskabs Forhandlinger, 8, 55-107.
Lobsien, M. (1903). Kinderideale. Zeitschrift für pädagogische Psychologie, 5, 323.
Meltzer, H. (1932) Personification of ideals and stereotypes in problem children. American Journal of Orthopsychiatry, 2, 384-399.
Meumann & Hösch-Ernst (1907). Das Schulkind. Leipzig: Verlag Nemnich.
Phelan, M. (1936). An empirical study of the ideals of adolescent boys and girls. Washington: Catholic University of America.
Reymert, M. L. (1916). Zur Frage nach den Idealen des Kindes. Zeitschrift für Pädagogische Psychologie, 17 , 226-250.
Reymert, M. L. (1918). Über Persönlichkeitsideale im höheren Jugendalter. Zeitschrift für Pädagogische Psychologie und experimentelle Pädagogik, 19, 10-28.
Richter, A. (1912). Statistische Erhebung über die Ideale von Volksschulkindern. Zeitschrift für pädagogische Psychologie, 13 , 252.
Schmeieng, K. (1934). Ideal und Gegenideal in der jugendlichen Entwicklung. Zeitschrift für pädagogische Psychologie und experimentelle Pädagogik, 35, 104-09
Schmeieng, K. (1935). Ideal und Gegenideal. Beihefte der Zeitschrift für Angewandte Psychologie und Charakterkunde (70), 138.
Simmons, C. V. & Wade, W. B. (1985). A comparative study of young people's ideals in five countries. Adolescence, 20, 889-898.
Storå, B. & Teigen, K. H. (1990). Hvem vil du helst ligne [Who will you like most to be like]? Journal of the Norwegian Psychological Association, 28, 21-26.
Stoughton, M. L. & Ray, A. M. (1946). A study of children's heroes and ideals. Journal of Experimental Education, 15, 156-160.
Wheeler, D. K. (1961). Development of the ideal self in Western Australian youth. Journal of Educational Research, 54, 163-167.
White, S. H. (1990) Child study at Clark University: 1894-1904. Journal of the History of the Behavioral Sciences, 26, 131-150.

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Helmholtz and Sechenov on the Origins of Human Thinking

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On 8th July du Bois-Reymond (1974) gave a paper on the limits of natural knowledge at the Leibniz conference of the Academy of Science in Berlin, which was later published under the title "The Seven Mysteries of the World". The agnostic tendencies of this work triggered fierce, and at the same time, extremely contradictory reactions in the scientific community. Human thought - according to du Bois-Reymond at the time - is set limits, which cannot be overstepped, to explain the essence of phenonmena of the consciousness. That concerns primarily the problem of the simplest sensations and of the freedom of the will, however an explanation of the thinking processes and of language is not completely out of the question in the near future. A greater trust in the human ability to gain knowledge was communicated by Sechenov in his Moscow paper (1956d) on "Hermann of Helmholtz as a physiologist" to the Society of Friends of the Natural Sciences, Anthropology and Ethnography on 16th November 1894. The "Father of Russian psychology" emphatically acknowledged the first completed step, in Helmholtz's handbook of physiologist optics, to include higher mental processes in the analysis of sensory processes. The outlines drawn up on the thinking process, which Helmholtz (1867, 1909), and following him, Sechenov (1956b,c), produced more than a 100 years ago, cannot be regarded as no longer relevant, rather they are of remaining value for psychological analyses whether concerning the ability to judge morally, the formation of the will, the self-confidence of the personality or the forming of theory in general, its methodological and epistemological basis.

Helmholtz steers sensory physiological works onto the psychological field, however he delimits the psychological part of the physiology of the senses, which he applies himself to, from "pure" psychology which, according to him, devotes too much space to self-observation instead of the experimental method, and in addition to this, doesn't take enough notice of the findings of anatomy and physiology. The analysis of sensory perception leads Helmholtz (1867) from a well-grounded and exact natural scientific research of sensational material, which is the basis of perception, to psychological consideration of mental activities, which alter the sensations to perceptions. This psychological point of view is supposed to help him to order, to systematise and theoretically to generalise expansive and empirical material facts.

Empiricism, the theory of sensory perception developed by Helmholtz, characterises the transformation of originally given sensations to perceptions as an individually acquired process of adaption dependent on experience. The sensory memory, the processes of attentiveness and inference are typical of this learning process (1971a,b). Traces remain in the memory as a result of mental activities in the consciousness, which with uniform repetition are linked to firmly rooted associations of ideas. Events are preferred between which law-governed relations exist, since that which is coincidential occurs seldom, it cannot be fixed in our consciousness as "sure" and "independent", in comparison to that which appears regularly (Helmholtz 1896, S.598). The idea of a 3 dimensional body contains a large number of elementary observations of the sense of sight and the sense of touch, which blend into a overall picture of the object. "Such an idea of a single individual body is then in fact already a concept" Helmholtz emphasises (1867, S.445).

Language and concepts are no preconditions for the activity of the memory, they are indeed poor in comparison to the diversity of natural objects. Not only varied reproducible impressions make an impression on our ability to remember but also vivid associations of ideas. This "sensory memory", which contains all processes of reproduction and association, is capable by means of "unconscious and instinctive actions" "to create associations of ideas, in us the consequences of which correspond on all basic features with those of conscious thought" (1896, S. 602).

Analogous to the logical inferences, which are based on words and propositions, unconscious processes of inference proceed in the sensory memory. It is well-known that the inductive inference is based on the premise that "previously observed and law-governing behaviour will prove itself

worthwhile in all cases which have not yet been observed" (1971a, S.277). On the other hand, the unconscious inference, in comparison to logical inference, isn't derived normatively but is based on varied experiences related to the everyday. Our memory adapts itself to the "similarities of cases which were observed earlier" (Helmholtz 1867, S.448).

Characteristic of these unconscious inferences are the lack of reflection, of rapidity, of surety and of precision of the process. Inferences are carried out without self-consciousness", they aren't subjected to the "control of self-conscious intelligence" (Helmholtz 1971b, S.71). With inferences carried out consciously "if they don't base themselves on precepts but on empirical knowledge we, in fact, do nothing else other than that we repeat, with consideration and careful examination, those steps of inductive generalisation of our experiences which were already carried out in a quicker way with conscious reflection" (1867, S.449).

Helmholtz's thesis of a far-reaching analogy of unconscious and logical processes of inference, under no circumstances, finds total acknowledgement since the majority of psychologists and humanities scholars, at the time, separated inference completely as the highest level of activity of the conscious mental life from smaller sensory activity. However, presently, there is little doubt that all model formations for invariant achievements of perception are subjected to decision-making processes which can be described precisely according to greatly upheld pre-experience by dint of the principle of maximum determined probablities (Klix 1973). Whether Helmholtz's chosen expression "unconscious inference" gives cause to misrepresentations seems to be irrelevant in this context.

Sechenov (1956a,b,c, 1968) follows the same path as Helmholtz in deriving all thought processes from sensory experience, however he gives his analyses new creative impulses.

- In comparison to Helmholtz, who had characterised the thinking process psychologically and avoided the classification of thought processes rashly bound to the material basis, Sechenov (1956c) followed Spencer's concept that under the influence of external conditions of existence in the phylogenesis and ontogenesis, structures of the organism, physiological functions as well as mental activities in continuous interaction with each other are increasingly complicated and form themselves more diversely. The influence of external factors on a changing „neuro-psychic organisation" is, for Sechenov, a basic determinant for the human ability to gain knowledge which develops throughout the lifetime

- In keeping with other traditional concepts, Sechenov (1956b) differentiates between the concrete-objective thinking of the child and the abstract thinking of the adult, for which after numerous transformations mental activity with supersensory, ideal phenomena is increasingly typical. The inevitable consequence for him, out of the methodical postulate in research in the natural sciences, to progress from the simple to the complicated, is to make the sensory and thought activity of the child the starting point of his analyses.

- Whereas Helmholtz represents the physical school of psychology, Sechenov's research is in comparison markedly biologically-orientated. The specific characteristic of living systems to adapt their own organism to a changing environment takes on a concrete form in the reflective principle, which he postulated (Sechenov 1956a; Smirnow 1980; Jarosevski 1980).

- Analogous to reflective mechanisms, mental processes possess an initial, middle and final link. Therefore sensory activity, central and mental processes and individual acts are to be analysed as components of an integral process in their mutual dependencies. The isolation of higher mental processes from correspondingly physiological phenomena and lower sensory activity is a grave mistake of traditional psychology Sechenov (1956b) emphasises repeatedly.

According to Sechenov (1956c), the starting point of thinking is active contact with the objects and phenonmena of the external world. As a result of lively sensory, activity the diffuse perceptions of the newborn begin to differentiate gradually. Preferred forms stand out from the complex, integral sensory-motor impressions, which leave fixed traces behind in the memory through frequent repetition as complete, constant and clearly categorised forms.

External, as well as, internal factors play a part in the categorising of sensory impressions. Numerous physiological analyses give evidence of the high ability to differentiate, which the senses

of sight, hearing and muscle possess. At the same time the motor functions appear as a link for different sensory modalities. The repetition of impressions under as diversely subjective and objective conditions of perception as possible and, with that, the corresponding formation of traces in the "neuro-psychic" organisation of the cerebrum is the basis of the more or less rich mental world of the human.

In his opinion thought activity is founded on three elementary mental acts: the divisibility of objects, their comparison and the capacity to group objects in different directions, whereby the associations of similarity and of the spatial-temporal vicinity form the basic criteria for classification. First of all, concrete objects are combined into groups of related objects. Gradually the breaking up of the concrete whole into parts, features and states follows, whereby the separate parts often gain an independent existence, and trigger renewed processes of integration. In the process of mental isolation of groups and series of ideas and characteristics, as well as, their following integration, concepts and symbols form which distance themselves increasingly from their sensorial starting basis dependent on their degree of generality. The abstract thinking of the adult, which in traditional psychology is attributed to a supersensory ideal world, is subjected essentially to the same structural laws of development, which apply for sensory-concrete thinking.

Recognition, the simplest of all the thinking processes, which exists in the comparison of a reproductive idea with the new impression, becomes "reason", if that what is recognised controls the action appropriately. Actions gain consciousness if the stored structure of ideas integrates subjective elements - wishes, intentions and ideals - firmly into itself. The conscious, active components of the inference process appear more and more clearly dependent on the expanse of the appropriated wealth of experience. Causal thinking, which comments on the relations between the objects and phenomena, falls back upon the experience of individual behaviour and thinking.

As a result of Helmholtz's and Sechenov's theoretical analyses of thinking outlined above, the question is inevitably posed as to whether their scientifically-orientated conceptions correspond to a forward-looking scientific programmatic or whether, as critical contemporaries maintain, a renewed attempt has gradually become available to reduce psychic processes mechanically to material phenonmena and to governing laws, what unequivocally means considerable devaluation of the humanistic ideal of the freedom to decide and of the individual's personal responsibility to take responsibility for their actions and misdeeds.

Whereas traditional psychology (Wundt, Kawelin) at the turn of the 20th century remained to a large extent anchored to Descartes' schema of existence of two worlds, that of res extensa and res cogitans, and the mental world was considered as a world of consciousness, isolated in itself, which is isolated from the external world, Helmholtz and Sechenov support the concept of a close "relation" (Sechenov 1956a) between the nerve and mental processes. Theories which split the natural and mental world into two worlds of experience are completly unacceptable to Sechenov, who feels himself tied to the uncompromising materialistic monism of Tschernyschewski (1953, Budilova 1978). On the other hand Helmholtz, in his postdoctoral paper (1852) "On the nature of human sensations", shows himself to be a convinced follower of Kantian transcendental philosophy: "Light and colour sensations are only symbols for relations of reality. We learn nothing about the real nature of external relations, through which the symbols are decribed, not anymore than from the names of people" (quoted from Königsberger 1902, S.176). However many interpreters (Conrat, 1904; Erdmann 1921; Heyfelder, 1897; Schlick; 1921; Schwertschlager, 1883) leave us in no doubt as to his increasing distance to Kantian agnosticism. A paper on "Goethe's presentiments on coming scientific ideas" gives evidence of this change, in which Helmholtz demands that "the theory of knowledge, based on the psychology of the senses, has to instruct humans to take action in order to become sure of reality" (1971c, S.362). Ideas have the function to regulate and to control actions: "We call our idea of the external world real" Helmholtz emphasises "when it gives us enough instruction about the results of our actions with regard to the external world and when it allows us to draw conclusions concerning changes to be expected of the same" (1896, S.590).

Empiricism, the theory of sensory perception established by Helmholtz, characterises the correspondance between sensory perception and the external world as „adaptation gained individually, as a product of experience and practice" (1867). Without exception, all thought processes are bound in this externally determined learning process. Kant's theory of the apriorism of

spatial perception or of originally given categories of reason is refuted. The geometric axioms are no synthetic propositions a priori but arise from experiences which are limited to our space, the Euclidean space. Beltrami, who had investigated pseudospherical areas, proves that through his analyses for which the parallel axiom is not valid. Lobaschewski in sacrificing the parallel axiom, also succeeded in creating a constant, geometrical system free of contradiction in itself. In addition the Euclidean system is also a product of our experience because a purely practised physical geometry, according to experience, could prove geometric axioms very quickly without having to go back to a purely deductive process of knowledge (Helmholtz 1971d,e).

Helmholtz emphatically opposes nativism, which is explained causally from the perception process through primarily innate factors independent of experience. Nativist-orientated physilogists would lose themselves in unfruitful speculations instead of explaining the perception process. He acknowledges then with reference to his own empirical view that it would be better "to possibly take the opposing view to extremes instead of remaining in the previous rut" (Letter to Donders, quoted in Königsberger 1902, S.88). In addition, the nativist has to grant experience the function of constantly correcting and overcoming the originally existing spatial perceptions. This hypothesis of changing one's ideas contradicts the scientific method which gives precedence to the simple principles of explanation.

Helmholtz's empirical position received a lot of recognition as well as opposition. Even du Bois-Reymond communicates his doubt in a letter to his friend Helmholtz: "It seems to me that one can always speak against the strictly empirical view since it has to be consistently feasible which you admit yourself isn't the case, because if it is innate for the calf to look for the udder through smell what else can be innate? It seems to me that so much nativism is left over which cannot be got rid off that it doesn't matter how much whether its a handful or not" (quoted from Königsberger 1902, S.84).

For Helmholtz there is only the alternative "innate" or "acquired through experience". Sechenov (1956), in no way, doubts that the mutual adaptation of the organism and environment succeeds in the phylogenesis and ontogenesis. The organism is for him a whole and goal-orientated system that adapts itself actively and independently to its external living conditions. Reflective mechanisms of the nervous system are prominently involved in this process of self regulation. Sechenov extends the reflective principal onto all mental phenomena in his theory of the reflective nature of the psychic without submerging himself in vulgar materialism or of reducing the mental to the nerve processes.

Sechenov refers with that to the close affinity of reflexive acts, instinctive processes, voluntary motions and processes of the consciousness which are amply verified through the position of knowledge of contemporary physiology. Common to all organic processes is a method "similiar to reflexes". In this sense Setschenow's reflexes with "psychic complication" (c. Rubinstein 1964, 1969) are integral acts which exist out of an initial, central and final link. He is decidedly against traditional concepts, from different psychological schools, of isolating the middle link from the integral happening and of investigating it as a separate sphere of consciousness. The assumption that the psyche begins and ends in the consciousness is in his opinion a fallacy. Sensory, reflectional and motory activities are components of a unified mental process (1956a,b).

Convinced of Locke's empirical position Sechenov emphasises that the original cause of each action always lies in the external sensory stimulus without which no thought takes place. As a departure from the traditional way of looking at things, Sechenov transfers the function of a signal to the external stimulus that is foresightly informed about different conditions of action and therefore enables goal-orientated actions in accordance with the necessities of the life.

First, in the course of ontogenesis internal mediator stimuli are added to the external stimuli which possibly appear as a direct cause of goal-orientated actions (1956b). Thought activity and voluntary activity is bound into this process, although the reflexive process comes to an end more so in processes of inhibition and seldom in processes of stimulus. In adulthood the real determinant of each concrete action is not an abstract ego in the end, but the prevailing mental and moral profile of the personality, which contains stored experiences of life as a result of the analytical-synthetical sensory and thought activity (Sechenov 1956b). In the submitted outline of Helmholtz's and Sechenov's analyses of mental phenomena, several general tendencies are revealed.

1. A concrete investigation of mental phenomena is being dealt with, speculative ways of contemplation and thinking are fundamentally refuted. Seen methodically psychophysiology experiments are not only regarded as obvious. Daily experiences, child psychology data, historical material for example concerning the development of culture and language are likewise profitable methodical inventory.
2. Dualistic concepts of the separate existence of a physical world and a world of consciousness are Wheatstone in 1838 scientific investigation of mental phenomena is hardly questioned, introspection loses its privileged position in psychology. Modern experimental psychology proves the narrow affinity of mental and nerve processes. That concerns analytical-synthetic activity as well as the specifics of the learning process. Under these circumstances the nalogy formation is an appropriate method to check the findings of lower sensory areas in view of their usefulnesss in higher sensory areas.
3. Sechenov and Helmholtz represent an interactive approach to the interaction of the human and the world. Mental phenomena aim to regulate the organism-environment- relations. The demands of life prove themselves as basic determinants of the neuro-psychic process of development out of which, on the one hand, a primary external determination of the psyche follows for all organisms, on the other hand, an increasingly complex neuro-psychic capability enables a greater autonomy as regards the material world.
4. In the causal analysis the effect of supernatural factors are awarded no place, indeterminable approaches of explanation are regarded as paradox. The original source of the whole psychic activity ncluding abstract thinking processes and voluntary processes are without exception external influences about which sensory activity is informed. Explanations are sought in the mutual dependencies between the nerve, sensory and consciousness processes. Helmholtz and Sechenov's trust in natural law-doesn't stop at mental processes.

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References

Budilova, E.A.: N.G. Cernisevski i materialistiskie psichologija. In: voprosy psichologii 5, 1978.

Conrat,F.: Hermann von Helmholtz' psychologische Auffassungen (Abhandlungen zur Philosophie und ihrer Geschichte), hrsg. v. B.Erdmann, Bd. XVIII, Halle/Saale 1904.

Du Bois-Reymond, E.: Vorträge über Philosophie und Gesellschaft, Berlin 1974.

Erdmann,B.: Die philosophischen Grundlagen von Helmholtz' Wahrnehmungstheorie kritisch erläutert, In: Abhandlungen der preußischen Akademie der Wissenschaften, Jahrgang 1921, Philosophisch-historische Klasse, Nr. 1, Berlin 1921.

Helmholtz,H.v.: Handbuch der physiologischen Optik, Hamburg und Leipzig, 1. Auflage 1867.

Helmholtz,H.v.: Handbuch der physiologischen Optik, Hamburg und Leipzig, 2. Auflage 1896.

Helmholtz,H.v.: Handbuch der physiologischen Optik, Hamburg und Leipzig, 3. Auflage 1909.

Helmholtz,H.v.: Die Tatsachen in der Wahrnehmung. In: Hörz,J.F; Wollgast,W. (Hrsg.): Hermann von Helmholtz. Philosophische Vorträge und Aufsätze. Berlin 1971a.

Helmholtz,H.v.: Über das Sehen des Menschen. In: Hörz,J.F.; Wollgast,W. (Hrsg.): Hermann von Helmholtz. Philosophische Vorträge und Aufsätze. Berlin 1971b.

Helmholtz,H.v.: Goethes Vorahnungen kommender naturwissenschaftlicher Ideen. In: Hörz,J.F.; Wollgast,W. (Hrsg.): Hermann von Helmholtz. Philosophische Vorträge und Aufsätze. Berlin 1971c.

Helmholtz,H.v.: Zählen und Messen erkenntnistheoretisch betrachtet. In: Hörz,J.F.; Wollgast,W. (Hrsg.): Hermann von Helmholtz. Philosophische Vorträge und Aufsätze. Berlin 1971d.

Helmholtz,H.v.: Über den Ursprung und die Bedeutung der geometrischen Axiome. In: Hörz,J.F.;Wollgast,W. (Hrsg.): Hermann von Helmholtz. Philosophische Vorträge und Aufsätze. Berlin 1971e.

Heyfelder,V.: Über den Begriff der Erfahrung bei Helmholtz, Berlin 1897.

Jarosevski, M.G. Secenov i razvitie naucnych znanij o povedenii. In: Kostjuk, P.G.; S. R. Mikulinski, S.R.; Jarosevski, M.G. (Ed.). Ivan Mihajlovic Secenov. Moskva 1980.
Klix,F.: Hermann von Helmholtz: Beitrag zur Theorie der Wahrnehmung - Bleibendes und Vergängliches aus einem großen Lebenswerk. Wissenschaftl. Zeitschrift der Humboldt-Universität zu Berlin, Math.-Nat. R. XXII, 3,1973.
Königsberger,L.: Hermann von Helmholtz, Bd. 1-3, Braunschweig 1902-1903.
Rubinstein,S.L.: Sein und Bewußtsein, Berlin 1964.
Rubinstein, S.L.: Prinzipien und Wege der Entwicklung der Psychologie, Berlin 1969.
Schlick,M.: Mitherausgeber von: H. von Helmholtz, Schriften zur Erkenntnistheorie. Hrsg. von P.Hertz und M.Schlick, Berlin 1921.
Schwertschlager,J.: Kant und Helmholtz erkenntnistheoretisch verglichen, Freiburg im Breisgau 1883.
Secenov, I. M.: Refleksy golovnogo mozga. In: Izbrannye proizvedenie. Moskva 1956a, S.7-127
Secenov, I. M.: Komu i kak razrabotyvat' psichologiju. In: Izbrannye proizvedenie. Moskva 1956b, S.172-267
Secenov, I. M.: Elementy mysli. In: Izbrannye proizvedenie. Moskva 1956c, S.272-426
Secenov, I. M.: Hermann v. Helmholtz kak fiziolog. In: Izbrannye proizvedenie. Moskva 1956d, S.49-509.
Sechenov, I. M.: Selected works. Amsterdam 1968.
Smirnov, A.A.:Psjchologiceskie bozrenija Secenova. In: Kostjuk, P.G.; S. R. Mikulinski, S.R.; Jarosevski, M.G. (Ed.). Ivan Mihajlovic Secenov. Moskva 1980
Tschernyschewski, N.G.: Ausgewählte Philosophische Schriften, Moskau 1953.

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Origin of Ideas of Contemporary Education

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Summary

By presenting books of two psychoanalytically oriented authors, published in the 1930s and 1940s in Hungary, I would like to show that their views on child-rearing and education involve the idea that human nature and society are opposed to each other. On the other hand, practice suggested by them as an adaptation of psychoanalytic ideas to education, was surprisingly similar to methods suggested by the behaviorist Watson, whose most important tenet was the predominance of environmental impact on children's development. Realising that two so very different schools of thought may represent similar practical methods, we may ask the question: where do ideologies of education come from?

In my earlier papers I tried to demonstrate that in the second part of the twentieth century a basic change has taken place in ideologies of child-rearing and education. The essentials of that change are that it is the child and not his adult caregivers and teachers, who determines the process of child-rearing and education. Many scolars would think that this tenet was inspired mostly by psychoanalysis and Freud. In fact practical methods of child-rearing, suggested by experts of different schools of thought, were surprisingly close to each other.

As it is rather well known, one of Freuds most important aims was finding biological roots and dinamisms in human nature. It is not so evident that according to Freud innate human nature is not only different, but is even incompatible with social demands. His descriptions about infants nature and behaviour clearly have moral connotations: infants are egoistic, greedy and cruel. On the other hand Freud warned parents against trying to change this original nature of the child, supposing that it may cause later neuroses. His paradoxical tenet caused an awkward situation for those of his followers who wanted to adapt his theory in child-rearing practice.

In my earlier works about Budapest School of psychoanalysis I have argued that significant part of Hungarian psychoanalists did not accept the concept of human nature being opposed to society, and their suggestions for child-rearing and education were basicly different from what is known as psychoanalitic education (e.g. Neillss Summerhill school or other forms of alternative education.) On the other hand two Hungarian psychologists who I am going to present here, are very close to the latter and their practical advices are not uncommon for Rogers and Maslows humanistic school as well.

Our first author is Bela Szekely, born in Hungary in 1891 and died in Argentina in 1955. (Harmat, 1989) Bela Szekely was not an orthodox freudist, he partly shared Adlers ideas and he was also a marxist. He published two popular books, one The Sexuality of Children", (1934) and It is Your Child" (1935). The first title refers to Freuds important view, shared by Szkely, that sexuality is the main component of mental life and development.

The second author is Lilian Rotter, who survived the war and remained in Hungary. She published her book Psychical life of Children" in 1946, one year after the war. That was a very short period when psychoanalysis was allowed to exist in postwar Hungary, and Lilian Rotter was in charge as a director of a mental hygienic centre. (She died in the seventies in Budapest).

Their all three books were written to parents, with the purpose to make them learn about recent results of scientific psychology". Attempts to adapt ideas of psychoanalysis to education were also made by educational reformers, having a strong movement in Hungary in the 20th and 30th. In building up practical methods on scientific results they saw a way for destroying authoritarianism in familiar and institutional education.

In the same time John B. Watsons books on education became extremely popular in the United States. Behaviorism and psychoanalysis have been always considered to be two contrasting school of thought in psychology. But as Christina Hardyment points out, a careful study of contemporary

books on education shows that American pediatrics and child-psychologists began to recommend mesures designed to forestall Freudian-style traumas, although they refused to recognize Freud openly" (Hardyment, 1983, p.166) Watson was not an exception. On the other hand, impact was mutual: some of his practical advices can be found in the works of Hungarian psychoanalysts, who have never even mentioned his name, or referred to behaviourism.

While Freud did not see a real possibility for conforming individual and society - this is one reason why his ideas are evaluated as pessimistic - his followers tried to find some way out. Child-rearing was a crucial point: no one of Freuds followers shared his views about hopelessness of educational efforts. One of possibilities was to assume that it is contemporary society which is incompetent in socialising children without depriving them from their original nature. A large part of thinkers who shared some ideas of Freuds theory, assumed that parents cannot find the right way how to treat their child because they are too neurotic. Both Szekely and Rotter emphasize that parental behaviour have to be radically shanged. According to Szekely parents have to forget about their own childhood: it is essential that they break off the chain of neurotising children by their own mother and father.

The idea of parental incompetency was shared by marxism too. According to marxist philosophers parents cannot bring up their children in a healthy and free way. They themselves grew up in class system and exploitation thus became mentally crippled. This was one reason why children in kibbutzim in Israel were reared in separate childrens homes" by professional educators. Yet the same view we meet in the United States among behaviorists and other psychologists. Skinner for exemple wrote an utopian novel (Walden Two, 1948) which floated the idea of bringing children up independently, without their parents, imbued with a sense of community and acceptance of authority. (Hardyment, 1983) Watson in the preface of his book Psychological Care of the Infant and Child" (1928) also wrote:

> "It is a serious question in mind whether there should be individual homes for children - or even whether children should know their own parents at all. There are undoubtedly much more scientific ways of bringing up children which will probably mean finer and happier children".

Parental incompetency can be changed only if parents listen to experts and learn scientific methods. Watson dedicated his book, published in more than 100 000 exemples, To the first mother, who brings up a happy child", and he also stated: The world would be considerably better off if we were to stop having children for twenty years (except for experimental purposes) and were then to start again with enough facts to do the job with some degree of skill and accuracy". But mothers who learned the essentials of behaviorism, may become a professional, not a sentimentalist masquerading under the name of Mother" (Hardyment, 172) This common idea for psychoanalysts and behaviorists, has not disappeared from educational ideas up to now. DeMause, (1978) psychoanalitically oriented historian of childhood and Thomas Gordon,(1990) a representative of humanistic school in their works, published in the 70ies and 80ies, argue that the only way of acceptable way of child-rearing came into being in our age - and only accepting ideas of education represented by them.

Let us turn back to Szekely and have a look on his argumentation. One of his most important arguments is an oversimplified explication of Freuds tenet: instincts and society represent inevitably opposed forces", and that any kind of instinct serves only individuals" (1935). If we have two things that can never fit with each other, we have to decide which has to be supported and which has to be rejected. Szekely tried to delay this difficult decision. He shares Freud judgement about children antisocial character: according to him infants are delighted by cruelty, they are egoistic and blackmailing parents. Yet he also states that there are no stupid, naughty, defiant children - all these bad things are results of adults pressure. Children suffer a lot, thus they must be liberated and given them the happy childhood back.

If children and parents are confronting, child-rearing certainly is a fight between them - exactly how Szekely saw it. According to him the mother who takes the child in her arms, when he/she is crying, loses the first battle. Enuresis and retaining faeces are arms against society and parents"

(p. 46) Children permanently struggle against sexual domination of adults. Authority must be avoided because it strengthens the struggle.

If child-rearing is a fight, one of the partners has to win. But according to Szekely adultss society must not thrive to victory. He speaks about compromise but his practical advices suggest that the only right way if parents keep away from the battles. Sexual development of children (let us remember, this is the substance of the whole development) is a natural process, and parents can neither prevent nor promote it. What should they do at all? They may watch with full empathy but without intervening how their child copes with his breaking up sexual instincts." (55) Szekely explicitly states that the best parental behaviour is passivity. Mothers have to withdraw their emotions when they are occupied with their child. Do not take up the child, do not cradle him in your arm, forget the period of lullabies" (p. 20) he suggested. Best mothers must give up their own tenderness for the sake of healthy education. Clock and scales are suggested by both him and Rotter, as best instruments for bringing up children well.

Let us see now some advices of Watson: There are rocks ahead for the over kissed child" - he wrote. According to him mothers kiss and caress their children in order to quiet him/her, because they needed the time thus saved for gossips, shopping and card-game. Petting and carressing the children may result invalidism in adulthood. Watson assumed that marriages may break down because wives fail to carress their husband in the way their mother had done.

By Hardimentss words Watsons answer to the complications identified by Freud was to bypass them altogether - to eliminate the love factor" (174) As we could see, this was the solution for Szekely too.

Let us see our second writer, Lilian Rotter. She started her book, published about twenty years after Watsons and ten years after Szekelys work, also with the statement of incompetency of parents. According to her parents neurotic inclinations so to say infect children. She even speaks about importance of asepsis and antisepsis in child-rearing. Interesting to mention that Alice Balint, who translated psychoanalitic ideas to practice in a basicly different way, employed a similar metaphor. In her paper What is decisive in education?" she argues that it is sterility which is harmful. Growing up in a family children will overcome smaller or bigger difficulties, but they are like necessary germs which increase resistence of the person. (1990/1937)

Rotter spared the thought that children develops independently, separated from environment. She mentions masturbation between years of 3 and 5, or agressive temper tantrums in early age as general symptoms, which can be found at each child, independently of treatment. (Only one step from this to state that the lack of these or similar symptoms shows some kind of neurosis, as it was the case with adolescent crisis).

Rotter, as Szekely did, recommend parents self-discipline in love and punishment. There is no need for strong reactions, subtile changes are sufficient" - she states. She refers to the exceptionality of the mother - child relationship in a very special way: mothers feelings are so important for the child that even small quivers may have a strong effect. The best way of behaviour if we treat our child in a kind and friendly way in which we have to apply only small changings for indicating approvals and disapprovals - she claimed: less love and resentment the parents need to apply, better educators they are. For Rotter too, the good parent is a permanent observer of his/her child, with minimal interventions.

The fight between the child and the world of adults, is a basic assumption for her too. According to her children - all of them - wish their parents to die. Oedipus conflict is an elementary convulsion, superego develops by an explosion. Parents need to behave in a moderate way for avoidance of big catastrophes and convulsions.

Mothers have to let the children cry - it is their problem and not childrens. If children do not want to eat their meal, one need let them to starve. It is wrong to let children play during meals: they must learn that joy and serious things have to be separated.

What is the reason of closeness of child-rearing advices between Hungarian psychoanalitics and American behaviorist? First, we have to realise that reformers ideas of building up child-rearing and education on the base of scientific research proved to be illusions. Certainly, there are some more important assumptions which determine educational view. What are they in the twentieth century education? What is common between behavioristic and psychoanalitic approach?

Behaviorists and followers of psychoanalysis were common in the basic view that childrens nature and society are opposed to each other. True, according to Watson children nature must be changed, while psychoanalists were not so explicit. But practical advices, represented by both part sggest that growing up is the childs own business, where adults have not a lot of things to do.

Watsons ideal lurks on moreover, perniciously but tenaciously, even today - remarks C. Hardyment.(173) What we really have now, is the idea that children may and have to grow up by themselves, by their intrinsic creative powers. This is a view represented by humanistic school, most influential on the contemporary edcuation. As we can read in the book of W.Crain, Theories of Development: Thus we are not forced to take charge of children learning, to choose tasks for them, tho motivate them by praise or to criticise their mistakes - practices that force them to turn to external authorities for guidance and evaluation. Instead we can trust their maturationally based urges to perfect their own capacities in their own ways." (323)

What is the origin of assumption of human nature and society being opposed? Certainly, it is not scientific research in psychology. It might have more general roots in the 20th century thinking - but this question is beyond my work.

References

Balint, A, 1990/1937: Min mulik a neveles? Animula, Budapest
Crain, W, 1992: Theories of Development Prentice Hall International, London
DeMause, P., 1978: History of Childhood Souvenir, London
Gordon, T, 1990: P.E.T. (Parent Effectiveness Training) Gondolat, Budapest
Hardyment, C, 1983: Dream Babies Jonathan Cape, London
Rotter, L., 1947: A gyermek lelki fejlodese Cserepfalvi, Budapest
Szekely, B, 1934: A te gyereked Pantheon, Budapest
Szekely, B. 1935: A gyermekevek szexualitasa Pantheon, Budapest
Vajda, Zs. 1995: A pszichoanalizis budapesti iskolaja es a neveles Sik, Budapes
Watson, J., 1928: Psychological Care of the Infant and Child Allen and Unwin, London

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Did We Foresee its Coming?

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Summary

One of most exciting questions in the history of science concerns the role which social sciences and the intelligentsia played in communist regimes. Communist ideology was a movement of intellectuals and, in Hungary at least, the intelligentsia had a very special role in the social life of the country. In the seventies and eighties, members of the Hungarian intelligentsia had privileges and they had the opportunity to put pressure on the Party and government which realized that they needed information about people's relationships, conflicts at work, and social life. Researchers in industrial psychology and sociology collected that kind of information and thus made some conflicts and troubles open. Worthy of mention is research on poverty, the gypsy population, and sociological surveys in large state factories. After the collapse of the communist regime, intellectuals and social scienctists quickly lost their critical inclinations.

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A Social Psychological Approach to Paradigm Change in Psychology: A Contribution to the Psychology of Science

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Summary

In the history of psychology paradigm changes generally are analyzed, either on the *psychological* level of an individual psychologist who makes a *Gestalt switch* or on the *sociological* level of an entire paradigm community. In this paper I shall approach the transition from the continental-European *geisteswissenschaftliche* way of thinking to the contemporary *naturwissenschaftliche* way of thinking in post-war Dutch psychology at the *social-psychological* level of innovative small research groups. It will be clear that in choosing this chapter of the history of psychology in the Netherlands as a case, I am not addressing the genesis of radically new ideas, in the sense of Kuhn's scientific revolutions, but merely the lines along which a new "way of seeing", that was already developed elsewhere, (gradually) became accepted within a particular scientific community. There is, however, no reason to suppose that - besides crediting for priority - the *social psychology* of paradigm changes would be different in the case of radically new ideas.

The transition that is at stake here was part of the *Americanization* process that changed the nature of the whole of Northwest-European psychology after World War II (van Strien, 1997). It is tempting - as I actually did in previous studies (e.g. van Strien, 1990) - to attribute the reorientation to a general switch from a continental-European to an Anglo-American "reference culture". A closer inspection of developments in the separate subfields of psychology in the Netherlands shows, however, that the reception of "American" ideas proceeded along disparate lines and at a different pace in these various quarters. In this paper I focus on developments within three subfields: social psychology, personnel selection, and "psychonomics".

A reconstruction of the way the "modern" approach increasingly gained prominence shows that it was in all three cases a small, newly formed research group which, in a practical context and in a for the most part novel problem situation, departed from the established approach. In social psychology it was a group of social psychologists at the *Netherlands Institute of Preventive Medicine* (NIPG), headed by Jaap Koekebakker, that acted as bridgehead for Anglo-American ideas. In personnel selection it was the newly reinstated selection centre of the *Royal Navy*, first headed by van der Giessen and later on by Langelaar, where Anglo-American psychometric selection principles were first adopted. The genesis of the psychonomic perspective within experimental psychology came about only in the late fifties. Its epicentre lay in the human factors research centre of the National Defence Organisation *RVO/TNO* at Soesterberg, where the biophysicist Bouman was the facilitating figure, and John van de Geer the first psychologist.

As such the study of developments at the micro-level of particular research groups is an assigment that belongs to the normal historiography. The *Psychology of Science* comes in, when the characteristics of the persons and/or groups involved are drawn in in a systematic way. In the full paper I shall do this by focussing respectively on the *absence of an impeding tradition*, the *role of the leader*, *characteristics of the other participants* and, finally, on the *field of relationships with the outside world*. I shall cite examples from the literature from which these types of psychological factors appear to play a role in other cases of innovation. By way of contrast, I will show that the "psychological climate" at the Dutch universities rather fostered searching for some synthesis between the old "European" and the new "American" way of thinking in the same period, and that a veritable "gestalt switch" came about there much later. My conclusion will be, that the results of a (social)-psychological analysis of developments in science are complementary to, and not less interesting than, those of current social studies of science.

References

Strien, P.J. van (1990). Die Rezeption der deutschen Psychologie in den Niederlanden. In: A. Schorr & E.G. Wehner (Eds.) *Psychologiegeschichte Heute* (pp. 261-274). Göttingen: Hogrefe.

Strien, P.J. van (1997). The American "colonization" of NorthWest European social psychology after World War II. *Journal of the History of the Behavioral Sciences, 33*, 349-363.

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Archaeology and It's Struggle to Understand Past Mind

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Summary

Archaeology emerged as an academic discipline out of the antiquarian pursuits of the mid-late nineteenth century landed gentry and other "genteel" folk. An early programmatic statement about the purposes of archaeology was made in 1875 by Augustus Pitt-Rivers. Pitt-Rivers wrote that "these words and these implements are but outward signs or symbols of particular ideas in the mind...it is the mind that we study by means of these symbols...". But since then, archaeologists have become increasingly shy of the psychological aspects of material culture: that is, until the early 1980s. In 1982 Colin Renfrew gave his inaugural lecture titled *Towards an Archaeology of Mind* thus launching what has become known as Cognitive Archaeology. In the same year, another Cambridge based archaeologist, Ian Hodder, published *Symbols in Action* in which it was argued that material culture was meaningfully constituted and that the individual needed to be a part of theories of material culture and social change. With the publication of Hodder's book, Post-Processualism was born, and material culture became a "text": Foucault and Derida were the order of the day. Both of these new perspectives were a response to the failed hopes for Processualism. Processualism, under the banner of the "New Archaeology" arose in the late fifties and sixties as a result of the adoption of positivism by a newly professionalised discipline. Archaeology was now to be a "hard" science and not just history with a spade. What is particularly interesting about both Renfrew and Hodder is their respective attitudes towards R.G. Collingwood and his doctrine of re-enactment of past thought. Renfrew - significantly - had nothing to say about Collingwood, though as one commentator at the time remarked, "Renfrew's own thoughts about the archaeology of mind would have been richer and deeper if he had shown awareness of Collingwood's philosophy". Hodder on the other hand, at the beginning showed some interest in Collingwood but later on he found a more "respectable" hermeneuticist in Hans-Georg Gadamer. Collingwood's notion of re-enactment is still taboo in archaeology. In the paper I will outline the disciplinary development of archaeology drawing attention to the tragi-comic use archaeology has made of philosophy in it's search for cogent theory and scientific method. I will conclude with some speculation on how things might have been otherwise if Collingwood had lived to worked on in to the post-war era.