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Section Foundations & History**

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SPIRITS, BRAIN CELLS, OR FRAUD?

a potentially open-ended discussion on a
parapsychological sitting in Helsinki in 1908

Abstract

In 1908 a British medium A.F. Peters paid a visit to a Finnish parapsychological society in Helsinki. The research committee of the society did not reach a consensus, and its members explained the same phenomena in three different ways: in terms of telepathy, physiological processes and fraud. This paper demonstrates how the early parapsychological experiments were highly dependent upon background assumptions, which made the whole tradition meaningless in the long run.

1. Introduction:

Mr. Peters visits a Finnish parapsychological society

In May 1908 a British medium Alfred Voyt Peters (b. 1869) gave a series of public spiritual sittings in Helsinki.¹ He focused his efforts on "psychometry", i.e. token object reading. He also described the spirits he "saw" during the sittings and conveyed their messages to the audience.² The séances were organized by the Finnish parapsychological society, *Sällskapet för Psykisk Forskning i Helsingfors* (hereafter "SPF"), founded a year earlier for the critical study of parapsychological phenomena on the model of the famous Society for Psychical Research in London.³ SPF has traditionally been an educated laymen's society but few people with an academical background also participated in its activities in the first decades of its existence. The most outstanding and most

¹ On Peters: Price H. *Fifty Years of Psychical Research*. London, New York, Toronto: Longmans, Green and Co. 1939: 149, 308, 324.

² *Mr. Peters' seancer i Helsingfors 1908. Experiment i psykometri och clairvoyance*. Helsingfors: Sällskapet för Psykisk Forskning i Helsingfors. 1910: 5-7, 13-15 et passim; see also Grotenfelt A. 1926. 'Miten on okkultismia ja "parapsykologiaa" arvosteltava tieteilisen sieluntutkimuksen kannalta?' [How should the occultism and "parapsychology" be assessed from the viewpoint of the scientific psychology?]. Pp. 43-96 in *Ajatus*, Vol. 1: 65.

Token object reading is "the use of an object associated with a target person as a focus for efforts at ESP": Edge H.L. et al. *Foundations of Parapsychology, Exploring the Boundaries of Human Capability*. London and New York: Routledge & Kegan Paul. 1987: 44.

³ The archives of the Sällskapet för Psykisk Forskning i Helsingfors (at the moment in the author's possession, hereafter "Archives"), the file *Mötesprotokoll 1907-1931*, the founding meeting 10.3.1907; see also 22.3.1908 with the enclosed annual report, and 21.3.1917 with the enclosed decennial report.

active of these was Arvi Grotenfelt (1863-1941), Professor of Philosophy, who was president of the society from the beginning until 1932.⁴ A divergence of opinions was typical of the early years of the SPF. Both spiritualistic and materialistic ideas were put forward in the meetings, and there were both believers and sceptics among the active members.⁵ This can be seen in the case of Peters' séances in 1908.

The society tried to arrange Peters' sittings in the form of scientific experiments. It kept minutes and appointed a research board to study the results. The idea was that the board would assess mediums results, present them statistically, give an objective statement on the whole series of sittings, and finally publish its findings. The basic unanimity required turned out to be an illusion, however, and the board could not reach a consensus even on the most fundamental issues: e.g. the tables on successful and failed cases.⁶ The chairman of the board, Arvi Grotenfelt, was obliged to report this fact to the annual meeting of the SPF in March 1909,⁷ and some months later the executive committee decided to publish the book on Peters' sittings in this contradictory form.⁸ When it came out in 1910, *Mr. Peters' seancer i Helsing-*

⁴ Archives, the files *Mötesprotokoll* 1907-1931, 1932, 1933-1934, and 1935-1946; see especially the annual meetings.

⁵ Archives, the files *Mötesprotokoll*.

⁶ See, for instance, meeting of the executive committee 21.1.1909: Archives, the file *Mötesprotokoll* 1907-1931.

⁷ Archives, the file *Mötesprotokoll* 1907-1931: 28.3.1909; and Grotenfelt's report in the file *Protokollen från seanser, Peters seanser 1908 och 1911*.

⁸ Archives, the file *Mötesprotokoll* 1907-1931, the

fors 1908 included three mutually conflicting views on the case, described in separate chapters.⁹

2. Interpretations of the séances

2.1. The majority of the board:

Telepathy

The chairman of the research board, Arvi Grotenfelt, was methodologically skeptical about parapsychological phenomena and always preferred natural explanations. He had explained this attitude in two lectures at meetings of the society during the year prior to the sittings.¹⁰ He believed in the existence of telepathy, however, and did not put forward any categorical opinion against the spiritual hypothesis, i.e. the idea that some parapsychological phenomena are due to spirits.¹¹

The statement of the majority of the board in the book is in congruence with Grotenfelt's basic attitude towards parapsychology, and claims that Peters had succeeded in psychometry several times and in "free spiritual vision",¹² i.e. describing the spirits he "saw". On the other hand, he had also failed many times.¹³

The majority of the board interpreted Peters' achievements as implying telepathy between him and the

⁹ Mr. Peters'.

¹⁰ Archives, the file *Mötesprotokoll* 1907-1931: 24.3.1907 and 8.12.1907.

¹¹ Archives, the file *Mötesprotokoll* 1907-1931: 19.3.1911 and 20.2.1921; the file *Föredrag*: Grotenfelt's lecture 20.2.1921; Grotenfelt, 1926.

¹² Swedish: "Fri andeskådning".

¹³ Mr. Peters'.

audience. It doubted the medium's psychometric view that there were traces of some kind in the objects which he could "read". Some of the members saw evidence of spiritual reality in the sittings, but officially the board left it to reader to decide whether or not spirits played any role in the case.¹⁴ Three board members disagreed with the statement to the extend that they contributed chapters of their own to the book.

2.2. Board member Erikson:

Unknown neurophysiological processes

Emil Erikson (b. 1861) had discussed precognition with the Emeritus Professor of Philosophy Thiodolf Rein (1838-1919) at two meetings of the SPF in January and February 1908 - just before Peters' séances. In Rein's opinion precognition is due to the human soul's spiritual activity during sleep. Erikson did not deny the existence of precognition, but he could not accept Rein's explanation. Instead he constructed a neuroanatomical hypothesis, which was a natural point of view for him as a medical doctor. There is nothing spiritual or mystical in this human capacity, it is due to neurophysiological processes which are still unknown to us.¹⁵

Erikson interpreted the results of Peters' sittings in a similar neurophysiological frame. It is possible to achieve exceptional sensitivity of the nervous system

¹⁴ Ibid.

¹⁵ Archives, the file *Mötesprotokoll*, 1907-1931: 16.2.

when in a hypnotic state, and Peters had obviously developed a skill of autosuggestion for reaching a trance in which he was able to pay attention to the minimal stimuli from the audience and the objects he was "reading".¹⁶

2.3. Board members Lillja and Törnros:

Fraud

Alfred Törnros (b. 1876) was the most eager of the SPF members to reveal fraud in the actions of a medium during the early days of the society. In January 1909 this professional businessman had explained the tricks of two Russian mediums, who had visited Helsinki,¹⁷ two years later he reconstructed the séances of a Danish psychic, again in order to demonstrate how people had been deceived.¹⁸ There is no evidence that the engineer John Lillja (b. 1879) had been as active a fraud-hunter, and at least in the Danish case he had more sympathy for the medium than did Törnros.¹⁹

In the eyes of Törnros and Lillja, Peters had not been successful. Their tables showed him to have failed in many more cases than the statistics constructed by the majority of the members.²⁰ They did not see any evidence of telepathy or any other parapsychic phenomena whatso-

¹⁶ Mr. Peters': 9-20.

¹⁷ Archives, the file *Mötesprotokoll* 1907-1931: 21.1. 1909.

¹⁸ Archives, the file *Mötesprotokoll* 1907-1931: 13.12. 1910 with enclosed Törnros' statement, 19.3.1911, 19. 3.1911 with enclosed annual report.

¹⁹ Archives, the file *Mötesprotokoll* 1907-1931: 13.12. 1910.

²⁰ Compare the enclosed tables.

ever. For them Peters was a swindler who exploited the weakness of the "scientific" control that they had maintained over the event, or at least a Sherlock Holmes who interpreted minute details of the objects, gestures and answers of the audience. In their opinion the sittings were not scientific experiments but popular shows, in which medium fixed the arrangements in his own favour.²¹

3. Conclusion:

A potentially open-ended discussion

Peters' results were interpretatively quite flexible, and thus the discussion about them potentially open-ended.²² Peters described fairly much the feelings that he had when holding an object in his hands and the personalities of the spirits. Let us look at how he "read" a closed letter in the first sitting as an example of his manner of speech:

"There is a feeling or condition of movement and hurry; - someone who thinks and acts precisely, but a queer feeling agitation, of excitement, of nervousness when I hold the letter in my hand, - as if the person who wrote it was of an excitable disposition; someone who thinks & acts quickly, is me-

²¹ Mr. Peters': 42-83.

²² In this respect this case resembles later parapsychological experiments studied by Collins and Pinch: Collins H.M. 1985. *Changing Order, Replication and Induction in Scientific Practice*. London, Beverly Hills and New Delhi: Sage, chap. 5; Collins H.M. and Pinch T.J. 1982. *Frames of Meaning. The social construction of extraordinary science*. London, Boston and Henley:

thodical in [his, J.A.] way of working & has an idea of taking up a subject & is full of decision if he wishes for it."²³

"Facts" of this kinds are difficult and often impossible, to verify or falsify. And even if there were a consensus on their correctness, there still would be crucial questions to answer: Did the medium somehow read the letter in the closed envelope or did he derive the facts from the answers or gestures of the person who gave him the object? Did he read the thoughts of the person in a truly telepathical way? Perhaps he already knew the content beforehand, or was it some spirit from the dead that gave the message?

This dependence upon background assumptions was in my opinion one cause of the decline suffered by the early tradition of parapsychological research. Work with a medium was unable to generate any hard facts about the survival of death or other perennial problems. Little by little, academic interest in the tradition seems to have vanished, at least in Finland.

²³ Archives, the file *Protokollen från seanser: Peters seanser 1908 och 1911*, the first page of English minutes. According to Grotenfelt the words in the minutes

Appendix 1: Tables on token object reading

The majority of the board¹

1. a completely successful case 2. a largely successful case
 3. a partially succesful case 4. deccribed in too vague a way
 5. a case where an especially noteworthy fact holds true
 6. a combination or guess 7. a completely failure

	1	2	3	4	5	6	7	
Seancens nummer.	Fullt lyckade fall.	Till stor del lyckade.	Till ringa del lyckade.	Allt för allmänt hållna.	Fall, där särskildt remarkabla omständ. stämmer.	Kombin. ell. gissn.	Totalt misslyckade.	Summa.
I	1 (rem.)	4 (1 r.)	4 (1 r.)	3 (+2)	(3)	(2)	4	16
II	3 (2 r.)	—	1	—	(2)	(1)	4	8
III	5 (1 r.)	5 (2 r.)	2	1 (+1)	(3)	(1)	6	19
IV	6 (1 r.)	2 (1 r.)	—	2	(2)	—	1	11
V	4 (1 r.)	5 (1 r.)	3	—	(2)	—	2	14
VI	1	5 (1 r.)	1	—	(1)	—	2	9
VII	2 (2 r.)	7 (2 r.)	3	1	(4)	(2)	—	13
VIII	2 (1 r.)	2 (1 r.)	2 (1 r.)	4	(3)	—	—	10
Summa	24 (9 r.)	30 (9 r.)	16 (2.)	11 (+3)	(20)	(6)	19	100

Törnros and Lillja²

1. very good 2. good
 3. fairly good 4. remarkable
 5. worthless 6. obviously a guess
 7. unverified

	1	2	3	4	5	6	7	
Seance.	Mycket bra.	Bra.	Ganska bra.	Anmärkn.-värda.	Värde-lösa.	Tydlig gissning.	Overraskande.	Summa.
I	—	—	2	1	7	—	1	11
II	1	—	1	1	3	1	2	9
III	1	—	5	—	3	1	—	10
IV	1	2	2	1	2	—	—	8
V	—	1	3	—	5	—	2	11
VI	—	—	1	1	7	—	—	9
VII	—	—	4	1	6	—	—	11
VIII	—	2	1	1	3	—	—	7
Summa	3	5	19	6	36	2	5	76

¹ Mr. Peters, 1910: 23.

Appendix 2: Tables on "free spiritual vision"

The majority of the board³

1. a completely succesful case
2. a largely succesful case
3. a partially succesful case
4. decribed in too vague a way
5. a case where an an especially noteworthy fact holds true
6. a combination or a guess
7. a complete failure

	1	2	3	4	5	6	7	
Seancens nummer.	Fullt lyckade fall.	Till stor del lyckade.	Till ringa del lyckade.	Alltör allmänt bällna.	Fall, där särskildt remarkabla omständ. stämma.	Kombin. ell. gissn.	Totalt misslyckade.	Summa.
III		1				(1)		1
IV	2 (1 r.)	1	1	(1)	(1)			4
V		1 (r.)			(1)			1
VI	5 (5 r.)	2 (1 r.)			(6)			7
VII	1 (r.)						1	2
VIII	1 (r.)	1 (r.)					1	3
Summa	9 (8 r.)	6 (3 r.)	1	(1)	(11)	(1)	2	18

Törnros and Lillja⁴

1. very good
2. good
3. fairly good
4. remarkable
5. worthless
6. obviously a guess
7. unverified

	1	2	3	4	5	6	7	
Seance.	Mycket bra.	Bra.	Ganska bra.	Anmärkn. värda.	Värde-lösa.	Tydlig gissning.	Overfl-cerade.	Summa.
I			2	1	7	—	1	11
II			1	—	5	—	—	6
III	—		—	2	9	1	—	12
IV	—	1	2	—	7	—	1	11
V	—	2	—	1	3	—	1	7
VI	2	1	—	2	4	—	1	10
VII	—	—	4	—	4	—	—	8
VIII	—	1	—	—	5	—	—	6
Summa	2	5	9	6	44	1	4	71

³ Mr. Peters, 1910: 23.

⁴ Mr. Peters, 1910: 59.

Jastrow, Houdini and Doyle: The Struggle
with Spiritualism in Early 20th Century Psychology

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Three individuals found themselves thrust into the debate over the paranormal in early 20th century America. Joseph Jastrow, the psychologist, had a long-standing interest in parapsychology; Harry Houdini, the great illusionist, often exposed fraud and fakery in purported psychical phenomena; and Sir Arthur Conan Doyle, the eminent author, was also a fervent proponent of spiritualism. The debate surrounding psychical phenomena intensified during Doyle's first tour of the United States in the spring of 1922. Jastrow covered one of Doyle's lectures at Carnegie Hall for the New York Herald, and Houdini, a close personal friend of Doyle, repudiated Doyle's evidence. These events occurred at a time when psychology was intent upon establishing itself as a scientific discipline. Problems with these efforts can be traced to the legitimization of psychical research and the popular movement to wed science and religion through spiritualism.

Jastrow, Houdini and Doyle: The Struggle with
Spiritualism in Early 20th Century Psychology

Three individuals whose careers are familiar in their chosen professions have the spotlight in this paper: Joseph Jastrow, the psychologist, Harry Houdini, the illusionist and Sir Arthur Conan Doyle, the author. Paradoxically, however, their contributions to the topic at hand are derived from their avocations. We will see how their lives were brought together for a time in the controversial climate of New York City in the spring of 1922, and how this encounter related to the struggle for the emergence of psychology as a scientific discipline.

Joseph Jastrow

Jastrow was born in Warsaw, Poland in 1863, the son of the prominent rabbi, Marcus Jastrow. The family immigrated to Philadelphia, Pennsylvania in 1866, and Joseph and his brother Morris were steered toward professional careers by their loving, if not autocratic, father. Joseph graduated the University of Pennsylvania in 1882. He entered Johns Hopkins University in Baltimore that September to train as an instructor. He became the first Ph.D. in psychology under G. Stanley Hall in 1886, and held the position of Professor of Comparative and Experimental Psychology at the University of Wisconsin for 40 years (1888-1927).

Although Jastrow's academic career centered on teaching, directing the laboratory and publishing experimental reports, the popularization of psychology developed as his

avocation (Jastrow, 1900). Jastrow and other psychologists were persuaded to undertake research on psychic phenomena by virtue of the definition of psychology as the science of mental life (Coon, 1992). Jastrow's record was well-established, with publications on psychic research (Jastrow, 1889a), deception (Jastrow, 1888), and mediums (Jastrow, 1910). Thus, by the second decade of the 20th century he was considered a credible spokesperson to confront a variety of suspect psychic phenomena.

Harry Houdini

Born Ehrich Weiss in Hungary in 1874, he took the name of Jean Eugene Robert Houdin (1805-1871), the French illusionist, who took pride in exposing fake performers of religious wonders (Shepard, 1985). Houdini is less well known for his frequent attempts to rebuke spiritualistic phenomena and expose fraud. In his own words:

To my knowledge I have never been baffled in the least by what I have seen in seances. Everything I have seen has been merely a form of mystification. The secret of all such performances is to catch the mind off guard.... (Houdini, 1927, p. 362)

That Houdini was viewed as a credible resource is evidenced by his appointment to a committee of Scientific American in 1923. Houdini served with William McDougall from Harvard, Walter Prince from the Society for Psychical Research and others to investigate mediumships. In spite of this Houdini had maintained a strong friendship with Sir Arthur Conan Doyle, even though Doyle had developed an un-

shakable adherence to spiritualism.

Sir Arthur Conan Doyle

Best known for his popular Sherlock Holmes series, Doyle was trained as a physician and served with distinction in the Boer War (1882-1890). He was knighted in 1902. But given his preoccupations with life, love and death it was inevitable he should be drawn to spiritualism (Higham, 1976). He attended seances, about which his fascination grew, and about 1916 he embraced spiritualism which, he said, gave "strength and comfort to others" (Doyle, 1927, p. 15).

But Doyle was actually among many notable figures who attempted to validate psychic phenomena and advance spiritualism. This may be seen as a reaction against materialism, positivism and the declining influence of orthodox religion. American, British and continental scientists could be counted among those associated with spiritualism and psychic research.

The New York Lectures

Conan Doyle and his party set sail for New York on 1 April 1922 unpretentiously to champion the cause of spiritualism in America (Doyle, 1924). A focus of Doyle's lectures was evidence for the afterlife provided by spirit photography. Since there was no question that photographs existed, the only issue was their authenticity. Here entered Jastrow, Houdini and Walter Prince. Jastrow's previous research had convinced him that the photographs were

faked; Prince recognized one photograph as a fake made by a Chicago photographer (Prince, 1925); and Houdini later published an account of the techniques used to produce spirit photographs in the laboratory (Houdini, 1927).

Scientific Psychology and Spiritualism

Jastrow's reasoned and persistent perspective on psychological research and spiritualism suggests that psychic phenomena presented a problem for the development of psychology for quite some time (Coon, 1992; Jastrow, 1900). The line between legitimate and spurious research was indeed fine, and it was not always easy to discern the bounds of acceptable evidence. Fundamentally, Jastrow said, scientific psychology should be based on the "laws of nature", and to this extent phenomena could be defined and studied in the context of evolutionary biology and physiology (Jastrow, 1889a). What psychologists and others could not anticipate in this era, however, was the tenacity of belief in psychic phenomena which gripped the American and European public in spite of repeated naturalistic explanations. For his part Jastrow published no less than four books on "proper" psychology related to beliefs (Jastrow, 1918; 1935) and thinking (Jastrow, 1930; 1931). And although Doyle's vision was to see in spiritualism the eventual confluence of science and religion, history did not cooperate. As Houdini remarked:

If there is anything to Spiritualism then the world should know it. If there is nothing to it, if it is, as it appears, built on a

flimsy framework of misdirection, then too the universe must be told. There is too much at stake for a flighty passing, for unsubstantiated truths. (Houdini, 1927, p. 365)

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THOUGHT & ACTION

SOME HISTORIC AMBIGUITIES

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ABSTRACT

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THOUGHT & ACTION
SOME HISTORIC AMBIGUITIES

An abstract

In the history of the philosophy of mind the relation between thought and action has often been problematic. The vicissitudes of the relation had repercussions on the history of theoretical psychology. For the behaviorists there was no relation to worry about because they stressed one relatum at the cost of the other. Up till recently the cognitivist, on the rebound, were only interested in the mental side, thinking that behavior somehow followed our mental representations without extra problems. By going back in the history of mind, I want briefly to deal with some background views on the life and times of the dualism of thought and action and its troublesome relation. Secondly I want to throw some light on important historical efforts to overcome the dualism. And finally I shall indicate the direction I think we should head for.

1. As I see it there are roughly two ways in which the relation became problematic. The first is the one we encountered already: to concentrate on one of the relata and deliberately excluding or shutting one's eyes to the other; either to negate that we can do things with intentions, or to

identify acting intentionally with thinking one's line of action good. The latter variant we find not only among cognitivists but also among the ancient Greek. The Greek philosophers conceived the agent's wish as a kind of knowing.

The second way is posing between thought and action other faculties like the will or passions which are able to steer our actions against one's better judgement. This is the line of thought of the Judaeo-Christian tradition. It is the conception that has the moral and religious dimension we identify with this tradition. The standard form of wrong-doing is doing what you know to be bad. The will directs our actions even against clear-headed cognition. It is the most powerful faculty, the gift from God to be able to obey or disobey, the choice is ours.

So, the will has a good and a bad side, it has strength and weakness, as Descartes assures us. Sometimes in history the weak side of the will is identified with passions, which are brewing in us. Passions constitute our dark side, our irrational *Doppelgänger*. This, of course, is the conception of romanticism and of Freud's psychoanalytic theory.

But often, like in Hume's theory, the passions are not held to be that obscure but are conceived as having motivational power, directing our actions rightly or wrongly. For Hume 'propensities' and 'aversions' are the causes of all actions; they are passions and cognition is impotent in these matters. Hume is very explicite in this.

'[R]eason is perfectly inert, and can never either prevent or produce any action or affection' (Hume,

1739/1969: 509)

'Reason is, and ought only be the slave of the passions, and can never pretend to any other office than to serve and obey them (ib.: 462).

In accordance with this Hume contended that moral actions and 'thus' moral expressions could not be true or false. Kant made the same sharp distinction between knowledge and moral actions. Metaphysical 'Ideas' about God, the mind and the world do not lead to what he understands by 'knowledge', but they nevertheless regulate our (moral) actions.

In all this we recognize also the (neo)positivistic gap between 'is' and 'ought', between the world of facts and our world of values and moral commitment. This distinction reserves rationality for our cognitive abilities and, again, drives apart thought and action.

This cleavage between reason and action, or between reason and will/passions/emotions/motives, has strong connections with the dualism between mind and body.

In the Cartesian conception the mind, the thinking soul or consciousness is the all important human principle: in the end humans could live without a body. This same idea lies at the back of the modern cognitive theory of functionalism (Putnam, Fodor). Mind is a representational system and the material realization, the stuff in which this system is implemented, whether brain or body, is of no real importance for the mind-system as such .

2. In the history of the philosophy of mind there have been

important suggestions to overcome these various ways of dualism. In my full paper I shall briefly commend on the very important ideas about 'embodied mind' and 'activity' in the philosophy of Leibniz, Spinoza and German idealism (Hegel and in particular Fichte). Unfortunately these ideas could not preclude a tendency towards idealism in which again an overstressing of the activity of mind came to the fore.

There appears to be some evidence to suggest that 'pure' philosophy has a predilection for overstressing mind as a thinking faculty. Part of the problem is that philosophers time and again refuse to take enough psychology into account. Husserl is notorious for his striving for a 'pure' philosophy without 'psychologism'. However, only a philosophy, or a philosophy of mind for that matter, that is naturalized and that takes into account that the body and bodily activity matters can really overcome dualism and become a well-balanced philosophy of mind that is of importance from a psychological point of view.

One of the view exceptions in history where we find a happy mix of philosophy and psychology is the theory of functionalism, the psychological counterpart of pragmatism, around the turn of the century (James, Dewey). Conception from evolution where of importance here: the function of mind is adaptation, i.e. action.

3. In elaborating the essential ideas of functionalism, and of concepts from other positions the sensitivity for the embodied mind can be augmented. Mind is a form of acting, to perceive

is to act and we act by thinking, though we are not always conscious of all our actions or their details. The concept of cognition needs extension. There is no moment where the mind stops and the body takes over. There is no system inside our body that thinks; neither a propositional network, nor a neural network does the trick. We as complete human beings think, perceive, understand, communicate, act.

In this respect 'intentionality' is a most useful concept. It can be conceived as the proto-type of motivation that makes the actions of the embodied mind understandable.

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SUMMARY-; LIMITS AND CONSTRAINTS OF A DISCOURSE ANALYTIC METHOD
FOR THE STUDY OF CONTEMPORARY PSYCHOLOGICAL PHENOMENON.

This paper discusses the use of a discourse analytic approach in the form of an archaeological analysis (Foucault, 1973), to address a contemporary question concerning the psychological phenomenon known as hallucinations. The question addressed within this study will focus on 'how' it is possible for hallucinations to be made sense of as fundamentally pathological phenomenon? Recent Anglo-American approaches to the study of discourse will be outlined, with an exposition of their implications for studying this conceptual question. It will be argued that the 'turn to language' (Parker, 1991) which circumscribes the 'new post-modern psychology', merely conceptualizes *history* as a *backdrop*, or facilitating influence on the various ways of 'making sense', or contemporary discursive explanations surrounding hallucinations. It will be argued and evidenced with reference to a recent analysis of three autobiographical accounts of hallucinations, that a notion of history as *conditions of possibility* must be utilized in order to place the study of discourse within its 'wider social and historical context'.

ABSTRACT-; LIMITS AND CONSTRAINTS OF A DISCOURSE ANALYTIC METHOD
FOR THE STUDY OF CONTEMPORARY PSYCHOLOGICAL PHENOMENON.

Since the launch of 'Discourse and Social Psychology' (Potter and Wetherell, 1987), some psychologists have utilized the discourse analytic framework, asking different questions about the role that language plays in the construction of social reality. ie. recent work carried out on representations of race. (Potter and Wetherell, 1991; Van Dijk, 1988). Whilst there has been much conceptual theorizing surrounding the role that language plays in constructing psychological phenomenon, (Richards, 1990; Coulter, 1989; Wittgenstein, 1953; Ryle, 1949), there has been little empirical work. Central to this emerging debate is a recognition of the critical role played by 'linguistic constructions' in social life. These 'linguistic constructions' are viewed as historically contingent (in other words they are mutable), and formative of social reality, and the ways we come to understand ourselves as human subjects.

This 'turn to language' (Parker, 1991), has been appropriated within critical psychological inquiry, to answer questions about contemporary psychological phenomenon. Questions within the psychological domain which traditionally have been seen as existing within the psyche, ie. attitudes, memory, cognition

etc., are viewed as existing within language. Thus the focus of inquiry is directed to the linguistic or textual resources variously producing the possibilities for individual experience.

This paper will explore the implications and consequences of adopting a 'post-modern' linguistic approach, when attempting to answer questions about our contemporary present, and its social and historical constitution. A critical analysis of these recent 'constructionist' perspectives will be outlined in relation to a particular research question-; 'how' is it possible for hallucinations to be made sense of as fundamentally pathological phenomenon? It will be argued that although these approaches act as a 'politics of interruption', de-stabilizing current, taken for granted concepts; they merely treat *history* as a backdrop where the battle of discourses are played out.

In contradistinction, it will be argued with reference to a recent study of three biographical accounts of hallucinations, from the 17th, 19th and 20th centuries respectively, that although it was possible to identify modes of explanations or 'themes' which sufferers drew on when accounting for their experiences, it was not possible to compare the themes across historical periods for continuities and discontinuities. These difficulties conflated two issues-;

1. The relation of modes of explanation to the wider discursive field at any time.

This will be discussed in relation to the importance of adopting a 'history of the present' in order to understand the historical constitution of contemporary objects of psychological discourse.

2. The connection between 'language' and the 'material'.

This relates to how one defines 'discourse' and 'how' it is possible for certain discourses to exist; the *conditions of possibility*.

This paper attempts to address these issues by drawing upon Foucauldian notions of discourse (Foucault, 1973), and discussing the relevance and implications for historical study of utilizing an archaeological analysis of our contemporary present.

HISTORICAL ORIGINS OF THE CONCEPT OF EMOTION

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HISTORICAL ORIGINS OF THE CONCEPT OF EMOTION

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Nothing seems more natural to the modern westerner than the emotions. The standard western view is to regard the emotions as involuntary responses which are rooted in the basic physiology of the nervous system. It will therefore come as a surprise to many people to learn that the concept itself is of relatively recent origin. Prior to the nineteenth-century, the terms "passions" and "affections" were commonly used to describe what we would now call the emotions. Two of the most important treatments of this subject in the eighteenth-century are Francis Hutcheson's *Essay on the Nature and Conduct of the Passions and Affections* and the second book of David Hume's *Treatise on Human Nature* which is entitled, *Of the Passions*. By the middle of the nineteenth-century, the word "emotion" had virtually replaced these older terms. Two of the most important works of this time are Alexander Bain's *Emotions and the Will* and Charles Darwin's *Expression of the Emotions in Man and Animals*. How did this change come about and why was it necessary? The present work will attempt to answer these questions by examining some of the most important writers of the time.

HISTORICAL ORIGINS OF THE CONCEPT OF EMOTION

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The use of the word emotion in English is comparatively modern. It is found in Hume, but even he speaks generally rather of the passions or affections. When the word emotion did become current its application was very wide, covering all possible varieties of feeling, except those which are purely sensational in their origin.

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For much of the eighteenth century, British moral philosophy was concerned with the nature of the passions and affections. One influential writer was Francis Hutcheson who postulated that human beings possess a "moral sense" which enables them to reflect on the morality of their actions. According to Hutcheson, what moves

people to action are the "affections". These are sometimes linked to violent body movements in which case they are called "passions". Hutcheson believed that the wise Creator had implanted within us passions and affections which lead to virtuous action. These were contrasted with the "selfish passions" such as lust and revenge. The moral sense enables us to encourage the former and restrain the latter. Many of Hutcheson's ideas were radicalized by David Hume who argued that moral action was not the result of reason but of the passions. This led Hume to his famous statement:

Reason is, and ought only to be, the slave of the passions and can pretend to no other office than to serve and obey them.

Hume's work contains an early use of the term "emotion", though he uses it in a very different way from modern writers. Emotions are seen as "disturbances of the mind". The more violent passions - such as love and hatred - are said to *produce* an emotion. These are contrasted with the affections which "produce little emotion in the mind".

Towards the end of the eighteenth century, the view that human beings are motivated by passions and affections came increasingly under attack as associationism began to gain ground. This can be clearly seen in the work of Jeremy Bentham. For Bentham, the only source of motivation is the desire to seek pleasure and to avoid pain. The problem with the names of the various passions and affections is that they imply moral approval or disapproval. Bentham sought to avoid value-judgements in describing human action. Thus "lust" was to be replaced by "sexual desire" and "avarice" by "pecuniary interest". He writes:

It may be observed that the same words which are mentioned as the names of motives are also many of them passions, appetites and affections: fictitious entities which are framed from a particular point of view.

This tendency to redescribe human motivation in non-evaluative terms may explain much of the attraction of the word "emotion". The passions and affections were too

strongly linked to moralistic discourse. The term "emotions" did not possess these connotations and came to be increasingly used. The first writer who refers to the emotions, rather than the passions and affections, is Thomas Brown. Brown's work contains an extensive discussion of "emotions which do not necessarily involve any moral feeling". These included "cheerfulness" and "melancholy" which were seen as pleasant or painful sensations. Subsequent writers used the term in this broad sense.

From defining the emotions in terms of pleasure and pain, it was only a short step to the speculative physiology of the mid-nineteenth century. One important writer was Alexander Bain who explained the emotions in terms of accumulated energy within the nervous system. This energy was diffused along certain channels giving each emotion its own characteristic form of expression. These views were shared by Herbert Spencer whose *Principles of Psychology* contains a chapter entitled, "The Language of Emotions". This "natural language" could be observed in all races and "throughout the whole animal kingdom". The influence of Bain and Spencer can be seen in the work of Charles Darwin. Darwin attacked the view that a wise Creator had implanted moral feelings within us. According to Darwin, moral feelings are not true emotions but learned associations. These are contrasted with emotional expressions which are "actions due to the constitution of the nervous system". Darwin's work was highly influential and provided the basis for future discussions of the subject.

In recent years, the view that there is a moral basis to the emotions has once again come to the fore. Such ideas, however, tend to come up against the standard view that the emotions are natural expressions which have their basis in the physiology of the nervous system. This may be no accident. It seems that the concept of emotion itself was a product of the increasing tendency to consider the passions and affections from a naturalistic point of view.

Gestalt Psychology and *Kunstwissenschaft* aesthetic influences on the Gestalt theory of perception

Crétien van Campen

According to most historians, Gestalt psychology emerged from late nineteenth century psychological philosophy. Although these historians explain the emergence of Gestalt theory in general, they fail to grasp some particular core features of Gestalt psychology. It began as a theory of perception in the second decade of the twentieth century. The distinctive shape of the Gestalt theory (of perception), compared to other *Ganzheits* theories, was formed in its revolt against associationistic and act-psychological theories.

In my opinion, the origins of the core assumptions and empirical laws of Gestalt theory, which distinguished it from other *Ganzheits* theories at that time, are not explained sufficiently by existing historical studies.

In addition to the traditional historical accounts of Gestalt psychology, I would like to demonstrate that these core assumptions and empirical laws originate from art-theoretical and aesthetical research in the nineteenth century (not normally considered in historical studies of psychology).

It is concluded that the history of the development of psychological aesthetics and *Kunstwissenschaft*, its theoretical notions and its empirical findings, will contribute to the historiography of Gestalt psychology. Including psychological aesthetics into the historical approach of perception research will explain more precisely how the Gestalt theory was formed.

Gestalt Psychology and *Kunstwissenschaft*

aesthetic influences on the Gestalt theory of perception

Crétien van Campen*

According to most historians, Gestalt psychology emerged from the psychological philosophy of the late nineteenth century. More specifically, the origins of the Gestaltpsychological approach are located in: Ehrenfels' article on Gestalt qualities; the phenomenological education of Wertheimer, Köhler and Koffka at the Stumpf's Institute in Berlin; and their experimental education with Schumann (e.g. Asch, 1982, 1985; Leahey, 1987).

Origins of Gestalt theory

Although these historians explain the emergence of Gestalt theory in general, they fail to grasp some particular core features of Gestalt psychology, which actually started as a theory of perception in the second decade of the twentieth century. The distinctive shape of the Gestalt theory (of perception), as compared to other *Ganzheits* theories, was formed in its revolt against associationistic and act-psychological theories. In my opinion, the origins of the following core assumptions and empirical laws of the Gestalt theory, which distinguished it from other *Ganzheits* theories at that time, are not explained sufficiently by existing historical studies.

Theoretical assumptions:

- *Gestalten* are directly given and not mentally constructed.
- The visual field is a two-dimensional *Gesamteindruck*.
- Perception has its own intrinsic (Gestalt) laws and is not governed by external laws.

Empirical laws:

- The Gestalt factors of proximity, similarity, symmetry and good Gestalt.
- The rules of the figure-ground phenomenon.

In addition to the traditional historiography of Gestalt psychology, I would like to demonstrate that its core assumptions and empirical laws originate from art-theoretical and aesthetical research in the nineteenth century (not normally considered in historical accounts of psychology).

Psychological aesthetics

Psychological aesthetics started in the eighteenth century by philosophers such as Baumgarten and Kant, and matured in the second half of the nineteenth century. The main difference with classical aesthetics, in which objective qualities of art were studied, was the assumption of beauty as being an experiential category. This stimulated philosophical and art-theoretical research of the mechanisms of the mind that were thought to account for aesthetic experiences.

In the second half of the nineteenth century, psychological aesthetics took an empirical turn. Art historians and psychological philosophers became interested in constructing an empirical foundation of aesthetics, which resulted in the so-called *Kunstwissenschaft*. This became a framework for different philosophical, art-historical and experimental studies of the formal features of art. In general, these studies showed an interest in the perception of *Gestalten*. I will discuss specifically those studies that express the Gestalt-theoretical assumptions and empirical laws mentioned above.

The art-theorist Fiedler claimed the existence of fundamental aesthetic forms in perception, which he called "pure visibility" (*Sichtbarkeit*). He assumed that artists were capable of perceiving aesthetic forms directly, on condition that they would not let associationistic thinking influence their perception. These basic forms were the foundation of the *Kunstwissenschaft*, in which the laws of direct aesthetic experience were studied according to Fiedler's ideas. His theory of direct formal perception shows similarities with the later Gestalt psychology, which aimed at a more general study of the laws of all direct formal experiences.

The sculptor Von Hildebrand applied Fiedler's theory to the visual perception of art and developed a distinction between artistic vision, which is a phenomenal *Gesamteindruck*, and empirical vision, i.e., the associationistic perception of an object. The concept of artistic vision is very similar to the Gestaltpsychological concept of "visual field".

The art-historian Riegl has formulated some perceptual laws of Von Hildebrand's artistic vision. In his study of ornamental art he describes some of the rules of the "figure-ground phenomenon", which were later on tested experimentally by the Danish psychologist Rubin.

In his study of classical architectural forms, the art-historian Wölfflin formulated the principles of symmetry, regularity and harmony in direct aesthetic experiences which correspond to Wertheimer's Gestalt factors of proximity, similarity, symmetry and "good Gestalt".

Historical connection

Around the turn of the century, psychological aesthetics and psychological perception research were more intertwined than historical accounts of both aesthetics and psychology nowadays suggest. Psychological aesthetics was at the same time part of experimental psychology. It is also a fact that the main founders of experimental psychology have written books on aesthetics. Cross-references between experimental psychologists and *Kunstwissenschaftler* were more common than today. The psychologist Bühler, e.g., explicitly based his program for the study of Gestalt phenomena on Von Hildebrand's study of artistic vision. The philosopher Cornelius wrote books on Gestalt psychology as well as on *Kunstwissenschaft*. Rubin even explicitly refers to Riegl in the introduction of his study of the figure-ground phenomenon. According to Bühler, Gestalt psychology used the results of the *Kunstwissenschaft* and studied them experimentally without bothering about the aesthetic questions, because the aim of Gestalt psychology was to discover general laws of perception.

Conclusion

The origins of the discussed theoretical assumptions and empirical laws of the Gestalt theory of perception can be found in studies concerning psychological aesthetics in the nineteenth century. There is historical evidence for connections between these studies and the later Gestaltpsychological ones. Historical research into the development of psychological aesthetics and *Kunstwissenschaft*, its theoretical notions and its empirical findings, will contribute to the historiography of Gestalt psychology. Including psychological aesthetics into the historical approach of perception research will explain more precisely how the Gestalt theory was formed.

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THE BEGINNINGS OF PSYCHOLOGICAL PROFESSION IN SPAIN

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Rather than creating a scientific psychology of its own, Spain seems to have been a receptive country, importing psychological ideas and methods from abroad, the prevailing criteria being their usefulness and relevance for solving the various social problems affecting the country. This is the reason why theoretical and experimental psychology may be said to have been preceded and introduced in Spain by applied psychology.

During the last decades of the nineteenth century an increasing attention was paid to the new psychological science. As a result, two psychological centers were developed, one in Madrid and the other in Barcelona.

The school of Madrid stemmed from a group of intellectuals deeply committed with the task of modernizing the country and promoting new ways in education. They established a center, the Free Institution for Education (1876), where neither the political nor the religious constraints heavily weighing on governmental schools and centers at the time were present. Its founder was Francisco Giner de los Ríos, Professor of Law and Morals at the University. He and Luis Simarro, a psychiatrist and first professor of psychology at the University of Madrid (1902), were able to gather around them a group of teachers deeply interested in psychological affairs: Martín Navarro, Francisco Santamaría, Fermín Herrero, Domingo Barnés, J. Vicente Viqueira were among them, and they succeeded in enhancing the connections between psychology and education. They not only introduced psychological matters in high school curricula, but also stressed the significance of psychological knowledge for teacher training, thus allowing teachers to profit from the scientific basis of their practical work.

Psychological contributions to educational practice

From the early decades of the twentieth century, psychological approaches to school problems were very much enhanced as schooling of mentally handicapped children was shown to experience significant improvement resulting from the work carried out by some pioneer groups. The influence of some French and Belgian psychologists, like A. Binet, O. Decroly and A. Descoedres, were partly responsible for such a state of affairs.

A physician and a student of Cajal's (the wellknown Spanish neurohistologist and Nobel prize winner), Dr. G. R. Lafora, himself a neuropathologist, greatly contributed to the establishment of a governmental Patronato Nacional de Anormales (National Council for the Mentally Disabled) which was hindered by many operating problems, but succeeded in stimulating the development of other private institutions.

As a result, some tentative efforts were made to provide the required materials for the diagnosis and treatment of such special people. Lafora's book on Mentally abnormal children (Los niños mentalmente anormales, 1917) provided teachers and professionals with an adequate synthesis of modern ideas, techniques and therapeutic procedures for the treatment of such children. Other relevant contributions were Diagnóstico de niños

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anormales (1914) (Diagnosis for abnormal children), by A. Anselmo González; La psicología experimental en la pedagogía francesa (1921) (Experimental Psychology in French Pedagogy), by D. Barnés; and Pruebas de Inteligencia (1930) (Intelligence Testing), an adaptation of the Terman test to the Spanish population, by J. Germain and M. Rodrigo.

In addition to these, several other steps were taken in the same direction. Well acquainted with the Decroly System, J. Orellana was put in charge of a newly founded national school for the deaf-mute. A psychotechnical laboratory was also established in the National School for the blind. By then, Lafora had already founded a private center in Madrid (Instituto Médico-Pedagógico, 1925) where adequate training was provided for some of the people mainly responsible for the development of Spanish scientific psychology in the following years (J. Germain, M. Rodrigo). Such a formal and varied cooperation between educators and clinicians may be taken as a characteristic feature of what we may call here "the group of Madrid".

At the same time, pedology as a holistic approach to child study received much support from educators. D. Barnés, secretary of a newly created Museo Pedagógico Nacional in Madrid, gathered information from all over the world and developed an eclectic view by combining functionalism, Gestalt psychology and comprehensive approaches in a well balanced system. His ideas were spread over several generations of Spanish schoolteachers and were greatly influential on teaching practices in elementary schools.

The intellectual models of these practical approaches may be found in Decroly's Ecole de l'Ermitage, in Belgium, and Claparède and Piaget's Institut J.J. Rousseau, in Genève: both centers were frequently visited by many Spanish technicians working in psychological issues. Claparède's Child psychology and experimental pedagogy was well known among teachers and greatly influenced their views. Piaget's influence was also significant, some of his books being first published in their Spanish version. Also, some attempts were made to apply Freudian ideas to school settings, as shown in the works of J. Jaen and J. Peinado; their influence, however, was limited.

Psychology and the working world

Although by no means in an exclusive way, the group of Barcelona was rather oriented towards industry and its working problems.

Along with the Basque country, Catalonia had been the one region to create and develop Spanish industry in the nineteenth century. As a result, a rising middle class claimed for a higher degree of political autonomy and became increasingly acquainted with social problems in demand of a solution. Technical and cultural approaches to such social demands were greatly promoted by E. Prat de la Riba, President of the Mancomunitat de Catalunya - a sort of local government newly established in the 1910's.

It is against this background that early psychotechnical developments in Catalonia ought to be considered. A Museo Social (Social Museum), including a Secretariat d'Aprenentatge (Secretary for Education) where professional and vocational guidance was carried out, was established in Barcelona (1914). This center was to become a larger Institut d'Orientació

Professional (1918) including a psychometric section soon headed by Emilio Mira, a physician who may be accounted for as the first professional psychologist in Spain.

Connections were soon established between this and other centers from abroad (mainly in France and Germany) also devoted to work psychology and psychotechnics. As a result, two international meetings on the specialty took place in Barcelona (in 1921 and 1930), and wider programs for personnel selection and aptitude diagnosis were designed.

Mira's ideas on psychotechnics were largely drawn from a holistic conception of the human person, partly inspired in the ideas of two Catalan biologists: Ramon Turró, head of a Catalan school of biology; and a follower of his, A. Pi Sunyer, a physiologist who developed the view of the organism as a functional unity in a very appealing manner (Miralles, 1980). Mira had a very creative mind: new instruments and testing techniques (perceptotachimeter, axiesterometer...) were devised by him; he also developed a new personality test on the basis of the analysis of some expressive traits of motor functions evidenced while making a number of geometrical designs with no eye-control from the subject (Myokinetic Psychodiagnosis Test, 1939). Mira also worked on forensic psychology (Manual de psicología jurídica, 1932). He was appointed Professor of psychiatry to the first chair ever established at the University of Barcelona; he also lectured on child psychopathology for education students. Mira's world-wide acknowledgement was clearly shown in his appointment as President of the Eleventh International Congress of Psychology, to be held in Madrid in 1936. The outburst of the Civil War, however, prevented this Congress from taking place; it had to be moved to France and was postponed to the following year (Paris, 1937).

Early developments in psychotechnics

The interactions between these two groups, the group of Barcelona and the group of Madrid, provided the ground for the development of applied psychology in Spain.

Under the direction of the German psychophysiological Theodor Ziehen, a seminar on psychological issues took place in Madrid in 1922. It may have been this seminar what led to the foundation of an Institute for the reeducation of handicapped workers (Instituto de Reeducación Profesional de Inválidos del Trabajo, 1922), which was put in charge of an engineer, C. de Madariaga, and a physician, A. Oller. It aimed at organizing a therapeutically oriented training that might help handicapped workers to recover their abilities for normal life and job performance. In addition to physical training, a psychological study of aptitudes was also established, thereby providing "a rational guidance for their reeducation process". The center embodied a psychotechnical laboratory, as well as a physiological section and an information service. Some of the technical procedures there employed were implemented according to the model provided by the J.J. Rousseau Institute, including a Rossolimo-Claparède profile and a number of other proofs. 14

A few years later, in 1928, plans for a new Formación Profesional (Preworking Special Training) for young people were laid, including vocational and professional guidance for students. The already existing centers in Madrid and Barcelona,

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now organized as Institutes for Professional Guidance and Selection (Institutos de Orientación y Selección Profesional) were charged with those tasks. Under their lead, several other laboratories were founded in different towns of the country. J. Germain and E. Mira were respectively in charge of the Madrid and Barcelona centers. The assessment and guidance of the young and/or the handicapped were among the tasks assigned to them. They were also responsible for testing future drivers, as a part of a traffic safety plan established in Spain in 1933 (Soler & Tortosa, 1987). Tests were also devised for pilots and military drivers.

Some features of early Spanish psychotechnics

The early Spanish psychotechnical movement clearly tended to inject the psychologist's role into other preexisting and well defined professional tasks (those of educators, physicians and engineers). These roles had to be thus partially modified in order to leave room for some of the activities usually assigned to psychologists -that is, the study of mental processes and the analysis of tasks and skills.

Lafora's point of view may serve to illustrate this. He thought, for instance, that tests could be successfully applied by teachers, with no assistance whatsoever from a psychologist. In his opinion, tests were so objective and well defined that no special technician was required for their application (Lafora, 1930). On the other hand, within the world of labour, certain "psychologists- physicians" were referred to by Madariaga as carrying out several reeducative and rehabilitation tasks.

This shows, in short, that the need for psychology was more deeply felt than the need for psychologists, as it was naïvely believed that professional psychologists could be easily replaced by other skillfull technicians, provided that objective procedures for the knowledge of individuals were available.

Limitations of early Spanish psychotechnics

Spanish psychotechnics in this early period suffered from a severe limitation. What was missing was a psychotechnical study of the normal young and adult human person. Neither was there an important industrial nor a military psychotechnics. While the former lack may be connected with the contry's low industrialization level during the first third of the 20th century, the latter ought to be associated rather to the non-intervention role played by Spain during World War I.

The lack of a military psychotechnics, so relevant in other countries for promoting an extensive knowdlege of tests and creating possitive attitudes towards the social usefulness of psychology, is a very significant fact. Driver and pilot selections were carried out in 1932 and 1933, but cannot be compared to the vast testing program implemented in the United states during World War I, when the Army Alpha and Beta tests were applied in a large scale.

In 1932 a military officer published a study on the usefulness of psychological testing for the Army ("La psicología experimental ¿es útil al Ejército?...Veamos" -"Is experimental psychology useful for the Army?...Let's see"). To him, psychology was a "science bounding in many aspects with

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Metaphysics and Philosophy, in others with Religion and, in a modern sense, with Physiology...". He warned that psychology, "as a developing science... has not yet said, and might never succeed in saying the last word... to solve the psycho-philosophical enigma introduced by life itself and put forward by Christian religion, subject to faith, by saying 'God'" (García Navarro, 1932, 6). Such an interpretation of psychology favoured a use of psychological aptitudes and profiles along the same lines as those prevailing in educational psychotechnics. Thus, far from the extended use of tests occurring in other countries, only a few punctual interventions for diagnosis and selection were here to be allowed.

A general characterization

Slowly developing along the first three decades of this century, early Spanish psychotechnics previous to the Civil War (1936-1939) succeeded in shaping a role for the psychologist on the basis of diagnosis and assessment tasks.

It was a psychotechnics dealing with the differential study of certain groups (children, workers suffering from some deficiencies) or of certain special tasks. As already mentioned, however, a study of subjects in military age as well as an extensive industrial operation was lacking. This hindered the collection of a large data base with the characteristics of adult, normal subjects of Spanish population.

The psychologist's role was thus defined within an interdisciplinary team with either mental and physical rehabilitation or school guidance and support purposes.

This early psychotechnics was carried out, in general, by professionals with a different and specialized first role (physicians, engineers, teachers) receiving a limited, applied-oriented training in psychology. Such a training was usually limited to brief occasional courses not giving rise to new specialized centers.

The ultimate inspiration of this first professional applied psychology may be said to stem from European sources, specially French, Swiss and German. Nevertheless, early Spanish psychotechnics was, indeed, very creative and, in some fields (as in the application of psychology to vehicle security), it ought to be acknowledged as pioneering later developments accomplished in other countries.

When the Civil War broke out, this growing psychotechnics was severely disrupted, and considerable time and effort were spent in reinstating its activity after such a tremendous social breach.

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The History of Basic Psychological Concepts:

A Brief Introduction

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Modern Psychology employs certain intuitively meaningful categories to define its subject matter, categories like emotion, intelligence, personality and cognition. The question is whether the meaning of these categories is due to the fact that they represent "natural kinds", or whether it is due to discourse practices that have developed historically in a particular social context. In recent years three lines of evidence have converged to support the second alternative. First, ethnographic evidence from non-Western cultures has indicated that our network of psychological categories is only one among many possible ways of organizing the description of human action and experience. Second, post-empiricist philosophy of science has demonstrated the dependence of empirical observation on a certain pre-understanding about the nature of the objects being observed. The categories of this pre-understanding, however, are not themselves empirically derived but depend on social agreement about language use. Third, current work in the history and sociology of science has shown how the symbolic order that science uses to represent aspects of the natural world is embedded in the historically contingent life of the scientific community. Undoubtedly, this also applies to the order of basic psychological categories.

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In introducing this symposium I would like to refer briefly to three modern developments in fields outside psychology that have contributed to the work to be reported here.

The first of these developments took place in cultural anthropology and took the form of a renewed interest in the conceptual systems of non-Westerners. Undoubtedly stimulated by the decline of colonialist attitudes, non-Western analogues to Western science began to be taken more seriously and to be studied as cognitive constructions in their own right. The field of "ethnoscience" was interested, for example, in the principles of classification to be found in non-Western botany. But non-Western cultures not only have their own coherent ideas about the natural world, they also have complex concepts that are used in the description of human action and experience where we would use western psychological concepts. In other words, these latter do not constitute the only conceivable categories for the representation of what we regard as psychological facts.

Although one can sometimes detect a certain overlap between this or that non-Western concept and one that forms

part of our own heritage, there is often an astounding lack of correspondence between the two sets of concepts. This is particularly evident at the level of what are to us basic distinctions, such as that between cognition and emotion, between body and mind, and between individual agent and social environment.

Such observations, surely, give us reason to pause and reflect about the categories in terms of which we organize our psychological knowledge. Do the categories that are currently popular among us, categories like cognition, emotion, learning, motivation, memory, personality, attitude, intelligence etc., represent natural kinds? Are we the people who happen to have hit on a nomological net that genuinely reflects the natural, the objective, divisions among classes of psychological events? Perhaps. But if we are, it is not because of our superior methods of empirical investigation. For the categories in question were not invented as a consequence of empirical investigation - they were there before anyone used them to identify the objects of empirical studies. Psychologists did not invent the concept of "emotion," for example, to account for certain empirical findings; they obtained certain empirical findings because of their desire to investigate a set of events which their culture had taught them to distinguish as "emotional."

That brings me to another set of developments outside psychology that are relevant to the subject matter of the

present symposium. This time the impetus comes from the philosophy of science. Although the news has yet to reach some of the darker corners of psychological research, discussion of the nature of scientific knowledge has been dominated for several decades by the critique of empiricism. An important part of that critique has led to a recognition of the crucial role played by preconception (or pre-understanding) in all empirical research. We can only communicate (and probably only make) empirical observations by applying a network of pre-existing categories. Every empirical description is an account that has been organized in terms of certain general categories. These categories define what it is that is being observed. For an observation to be psychologically relevant and interesting it has to be couched in terms of psychological categories. The report that the pencil in someone's hand made contact with a piece of paper at a certain distance from the top of the page does not count as an empirical observation in personality psychology. The report that someone received a certain score on the Taylor Manifest Anxiety Scale does. It is not enough to make any kind of observation in science - one has to make relevant observations. And you cannot make psychologically relevant observations without the use of psychological categories - "anxiety" in my example.

Such really rather obvious reflections have led in recent years to a much greater appreciation of the importance

of basic preconceptions in scientific work. We have to have some agreed upon notions of what it is we are investigating before we can make empirical additions to the sum total of our shared knowledge. This is not to say that our preconceptions are necessarily incorrigible, but the more we take them for granted, the less aware we are of their very existence, the less corrigible they are likely to prove in practice.

And that kind of incorrigibility could well rob us of the fruits of our empirical research. Our empiricist tradition has accustomed us to constantly correct our explicit theories about classes of psychological events in the light of empirical evidence. But post-empiricist philosophy alerts us to another kind of theory, i.e. the presuppositions about our subject matter that are implied in the categories we use to define the objects of our research and to express our empirical findings. If we render these presuppositions practically incorrigible because we never examine them we set very narrow limits to the progress of our science.

For psychology the problem is particularly serious because even after a century of specialized usage most of its terms remain heavily dependent on shared understandings in the general culture. Psychology may have developed certain theories about motivation, about personality, about attitudes, and so on, but the network of categories that

assigns a distinct reality to motivation, to personality, to attitudes etc. has been taken over from a much broader language community of which psychologists are a part. If we consider it advisable to become clearer about the ontology we may have imported with these categories one of the most helpful steps we can take is to look more closely at the historical vicissitudes of the relevant concepts. This will also make us aware of the specific role of modern psychology in modifying traditional understanding in its turn.

Such questions have taken an increasingly prominent place on the agenda of historical scholarship in recent years. And that takes us to the third and last of the developments I want to refer to briefly in introducing this symposium. These are developments in the sociology and historiography of science that have rather transformed these fields during the last few decades. A generation ago it could still be said with some justification that the history and sociology of science consisted largely of the history and sociology of scientists. That is no longer true to-day, though the history of psychology has tended to form the rearguard of this advance.

The change I am referring to involves a different way of organizing historical accounts. Such accounts of course originated with hymns of praise for individual rulers and chronicles of dynastic succession of such rulers. Later on, other heroic or at least prominent individuals, eventually

including scientists, became the focus around which historical narratives were organized. We still find this kind of organization in Boring's classical History, for example. But two kinds of development have tended to make historians much more aware of the limitations of this approach. In the first place, the work of social historians and of sociologists of science has brought to the fore the role of broad social changes, unattributable to any individual, and also the role of communities of scientists and professionals in shaping the course of history.

Secondly, - and this is more directly relevant in the present context - there has been a powerful move in the direction of analysing the symbolic products of human culture, and that includes science, as forms of discourse rather than as reflections of nature or expressions of individual character. In other words, a major part of what the historian of a field like psychology has to deal with consists of cultural products that are in the public domain and that constitute systems of shared meaning among many individual participants. Here we find common preconceptions, common ways of organizing knowledge, common assumptions about what can be taken for granted and common definitions of matters experienced as problematic. However, the sharing of certain understandings does not mean unanimity. Like natural language this is an area in constant flux and in the process of long term historical change. It is a discourse to which

many contribute, and, like a conversation, it has a structure and a historical development of its own. Such perspectives have become popularly linked with the name of Michel Foucault, but I must emphasize that his is not the only version of this general approach to be found in the relevant literature.

In the light of all these developments then the historical analysis of basic psychological concepts assumes a new significance. These are the concepts that psychology has borrowed from the culture that gave rise to it and in which it remains embedded. They are the concepts that often constitute the theoretical pre-understanding of the subject matter that psychologists investigate, and at the same time, it is these concepts that organize the discourse which is modern psychology. The hope is of course that an improved historical understanding of the evolution of these concepts will promote their more insightful deployment in everyday practice.

Paper submitted for the session: Contemporary questions (to be) answered via historical research.

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METHODOLOGY AS LEGITIMATION AND OUTPUT

The neglected role of methodology in the historical development of the social sciences

SUMMARY

Traditionally, philosophy of science pictures scientific development as the outcome of methodology-governed theory-choice. In the field of science studies, this image has been denied "historical adequacy", though. Here, methodology is exposed as rather a fifth wheel under the wagon than the motor of scientific progress. In line with that picture, the remarkable methodological rigorousness of the *social* sciences appears to be not only a zealous, but moreover a *misdirected* attempt to equal the natural sciences. Sciences like psychology and sociology seem to mimic some admired qualities of a big brother, which this brother in fact does not have at all.

As will be argued in this paper, though, regarding the social sciences the general statements on the role of methodology presented in science studies seem to possess not much more historical adequacy than those presented by philosophy of science. Historical research into these sciences themselves is required to answer the question of how they became that meticulous in the methodological domain and to gain an adequate image of methodology's contribution to their development. A tentative answer will be elucidated: of old methodology not only legitimates the social sciences' place in society, but moreover it has gradually become part of social scientific *output* itself, and as such finds a ready market.

fr: methodologie vervult eerder een praktische dan een
wetenschappelijke rol.

METHODOLOGY AS LEGITIMATION AND OUTPUT

The neglected role of methodology in the development of the social sciences.

Trudy Dehue

Introduction

In the last few decades, the empirical science studies pictured quite another image of the development of science than that of philosophers and of most scientists themselves. While the latter generally ascribe theory choice, that is scientific progress, to the application of the right methodological criteria, in science studies it is generally agreed upon that from a *historical* point of view such an account does have little "adequacy". In different ways historians/sociologists like Kuhn, Feyerabend, Latour, Woolgar and Knorr argued and illustrated that the development of science is not as rule governed as philosophers tend to picture it and that it in fact is a social process.

As a consequence, such historical/sociological studies of scientific development mostly investigated methodology only in the *negative* sense that its actual impotence was shown. Once methodology was labeled as the fifth wheel under the wagon rather than the motor of scientific progress, it did not seem to be a very important subject of investigation anymore.¹

Not surprisingly, in this field of science studies, little or no *special* attention has been paid to methodology's contribution to the development of *the social and behavioral* sciences (henceforth for short "the social sciences"). In line with the conviction that methodology's contribution to the growth of knowledge is negligible, the wellknown methodological

scrupulousness exhibited by contemporary social sciences,² appears as not only a zealous but moreover a *misdirected* attempt to imitate the natural sciences. The social sciences desperately seem to mimic some admired qualities of a big brother, which this brother in fact does not possess. Woolgars point of view is typical: "The debate about the SCIENTIFIC character of social sciences recurs from time to time (...) (the capitals denote the mythic, idealist connotations of the use of this term) (...). Perhaps the most significant achievement of the social study of science is the finding that the natural sciences themselves only rarely live up to the ideals of SCIENCE (...). By recognizing the un-SCIENTIFIC character of both social and natural sciences, social scientists can stop worrying about how SCIENTIFIC they are".³

Elsewhere I argued that this critique is justified in so far as it concerns the inclination of some social scientists to ascribe universal and timeless validity to one particular image of science.⁴ Nevertheless, apart from that, it should be pointed out that with this view the pot calls the kettle black: not less than the criticized, this critique proclaims the habits of the natural sciences to be the standard. For, it argues that if methodology actually does not have much impact in the natural sciences, this must hold true for the social sciences as well. In spite of science studies' characteristic aversion of ethnocentrism⁵ and of groundless claims concerning the unity of science, it is taken for granted that what holds true for the natural sciences, holds true ^{also} for the social sciences in general.

Regarding the social sciences, the --natural sciences-based-- accounts of methodology presented in science studies, might possess not much more historical adequacy than the --natural sciences-based-- accounts of philosophy of science. It just seems too *easy* to dismiss the methodological rigorousness of the social sciences as an expression of an alleged inferiority complex. Historical research into these sciences themselves is required to answer the question of why they are that meticulous in the methodological domain and to gain an adequate image of methodology's contribution to their development.

The case of psychology

Such research of course needs a provisional answer to start off with. As Karl Popper must be granted, without conjectures one cannot know what to investigate. In an ocean of potentially available data, also historical research needs the direction that a reasoned guess can provide. Taking psychology --the most methodology minded discipline of all social sciences-- as the main example, I will tentatively present some interrelated hypotheses, which taken together render the social sciences' methodological sensitivity intelligible.

In the last decades of the 19th century, when psychology was established as an academic discipline, this did not occur because some academics calling themselves psychologists began to ask questions that never were posed before. On the contrary, questions on the relation between stimulus and sensation, on human emotions, the differences between men and women or the education of children, all had been discussed and answered before in circles as broad as those of philosophers, novelists, clergy man, physicians, teachers, and in fact whoever could read and write or just speak up. The rise of academic psychology was connected with the application of *methods* that were not used for such questions before. Declared pioneers of the discipline like Wilhelm Wundt, James McKeen Cattell, Francis Galton, Alfred Binet and Gerard Heymans demarcated the new discipline mainly by calling forth certain methods.⁵

In this respect there is of course no *absolute* difference between the social and the natural sciences. As the well known cases of Galilei and Boyle clearly illustrate, the natural sciences neither did start off with questions no one else had claimed to answer before. In the natural sciences, methods were an important argument as well. However, there weren't nearly as many competitors as on the fields academic psychology tried to occupy, and moreover, in due time the natural sciences produced theories and research programs that generated their own new subjects for further research. Soon, in most natural sciences questions were posed and topics were discussed that were incomprehensible to the non-initiated, let alone even slightly answerable by them.

Psychology developed in another direction. The larger part of the discipline began to fulfill a social role which differed from that of a theory directed science. As has been described very often, psychology did not retreat from society but on the contrary made its way into general social worries (or, in Foucault-inspired terminology, became a human technology for the calculated social organization of individual forces and capacities).⁶ The discipline began to deal with matters concerning labor relations and the assessment of personnel, educational measures, the mental health of adults, measurement and influence of peoples' attitudes, diagnoses and therapy of mental illness etc.

Therefore, *increasingly* academic psychology began to make products analogous to those of many other sections of society. While there is nowadays nearly no competition anymore between the natural sciences and the churches or philosophy, competition to academic psychology not exactly diminished. Journalists, novelists, philosophers, governmental officials, clergymen, physicians and social workers, as well produced and still produce thoughts on social development, individual identity, psychological therapy, child-rearing, conflict management, group processes or mnemonic functions. The specific task assigned to or claimed by psychology became to supply knowledge of higher or other quality on these sorts of topics. However, this special quality mostly did nor does show directly in these knowledge products themselves. Many of the insights of psychology were (and remained) put in an idiom identical or very close to that of non-scientific sectors. The *difference* mainly has to be indicated by reference to the way in which these insights were obtained, that is by reference to the methodology that was used.

In short, throughout its history psychology demarcated itself by referring to the "how" of the discipline from its many "non-scientific" competitors and thus legitimated its place in society.⁷ It was not the alleged *wrong* assessment of its self-interest that motivated psychologists to concentrate on methodological procedures, but a quite right one. In the course of its history the discipline developed an elaborate array of methodological means, for the effective treatment of scattered practical problems and empirical questions.⁸ At

best, these problems and questions were only superficially theoretically embedded. Methodological rules in the social sciences mostly are not *meant* to steer rational theory-choice.

However, there emerged a second and maybe even more important consequence of the fact that questions on people and their mutual relations are also generated in many other social sections. Methodology gradually began to constitute not just the *means* of knowledge production for social scientists themselves, but almost unnoticed it became a form of social scientific *output* on its own. Methods and techniques of psychological research, initially developed for intra disciplinary purposes, began to be disseminated among other parties to handle their own questions, plans, findings and choices. Thus the means of research, no less than the research results, have become a kind of deliverable knowledge, and as such have found a ready market.

The career of the American psychologist/methodologist Donald T. Campbell offers a clear case. Initially, Campbell's methodological interest was excited in the context of matters that already had been studied by academic psychology for some decades: that of assessment of personality and personality change, of relevance for instance for personnel selection or for measuring the effects of psychotherapy. Soon though, Campbell began to generalize his methodological insights: first to other parts of psychology, next to other social sciences and finally to extra academic matters.

In 1957 he published an article that became very influential in international psychology.⁹ In this article he extended the meaning of the concept of "validity", well known in the methodology of psychological test-research, to the methodology of *experimental* research. Campbell's new article began with a distinction between the so called "internal validity" of an experimental design and its "external validity." An experiment meets the criterion of internal validity when different results in a pre-test and a post-test in fact were due to the experimental stimulus, instead of to one or the other disturbing "extraneous variable". It meets the criterion of external validity when the results are not confined to the

experimental situation but hold true for relevant other populations and settings as well. Campbell then discussed a list of "threats to validity". One of those for instance was labelled "Maturation". The term referred to processes within the subjects operating as a function of the passing of time per se. Children may grow older or get hungry between a pre-test and a post-test that is intended to measure the effect of a teaching method, and the effect in fact might be due to these extraneous variables. Another "threat" was named "statistical regression." This one operates when groups have been selected on the basis of their extreme scores. Just by the laws of chance, extreme scores can be expected to "regress to the mean." After psychiatric treatment, a group of patients might rate lower on a test of neuroticism than before, due to this effect rather than to the therapy. After having listed more of these validity problems, Campbell continued by assessing several complicated experimental designs --called e.g. the "Pretest-Posttest Control Group Design" or the "Posttest-Only Control Group design"-- on their resistance to these extraneous variables.

In 1969, the first step was taken in marketing his methodological analyses and experimental designs outdoors. Together with J.C. Stanley, Campbell had written a chapter in a 1963 *Handbook of Research on Teaching*.¹⁰ In this chapter first some threats to validity were listed again. Next, an important new notion had been introduced: that of "quasi-experimentation". It was reasoned that in educational research, the researcher quite often lacks control over the randomized scheduling of experimental stimuli to particular subjects at a particular moment. "True" (laboratory) experiments can not be conducted. Therefore, especially in this kind of research, internal and external validity are jeopardized. A range of so called "quasi-experimental designs," had been presented with names like "the non-equivalent control group design" or "the separate-sample pretest-posttest control group design".

Though, soon after publication of their chapter, Campbell and Stanley had come to the conclusion that in many more situations, a researcher has less control than when working in a laboratory. They decided to advocate their ideas for the social sciences in

general. They slightly edited the text of their 1963 chapter and published it again. In 1966, the chapter originally entitled "Experimental and Quasi-Experimental Designs for Research on Teaching," re-appeared as a *book* under the shorter and wider title "Experimental and Quasi-Experimental Designs for Research."¹¹

Now, quasi-experimental methodology clearly had become a kind of output of psychology, recommended for all social sciences. Some years later, the final step was taken. Campbell advocated his quasi-experimental designs in circles *outside* academia. He directed himself to social policy makers with a plea to "extend the logic of the laboratory into the 'field'." In a 1969 article, entitled "Reforms as Experiments"¹² he announced the relevance of his methodology for social "program evaluation". "The United States and other modern nations should be ready for an experimental approach to social reform," the first sentence of his article exclaimed. Once more Campbell presented his list of "threats to validity", as well as some methodological remedies when only quasi-experimentation is possible. He for instance amply discussed the example of a measure taken by a Governor Abraham Ribicoff of Connecticut, who instituted a severe crackdown on trespassing speed limits. When after one year there had been only 284 traffic deaths as compared to 324 the year before, this Ribicoff claimed to have considerably reduced the motor vehicle death toll in one year. Campbell criticized Ribicoff's "experimental design" in the light of his list of threats. "Maturation" might be a rival hypothesis, he argued: relative to miles driven or population of automobiles, death rates might have gone down due to other changes in the drivers. Or "Statistical Regression" might have caused the reduction: the sample of 324 deaths might have been such an exceptional one, that in any case this number would have regressed to the statistical mean.¹³

Psychological methodology now functioned as a form of output itself. In a article with the expressive title "The Social Scientist as Methodological Servant of the Experimenting Society," Campbell even proclaimed methodology to be the *most important* product of the social sciences: "The aspect of the social sciences that is to be applied, is

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primarily its research methodology rather than its descriptive theory (...)."¹⁴.

Campbell's ideas surely did catch on. As the contemporary American evaluation specialist Lee Cronbach wrote in 1983 in the preface of his book *Designing Evaluations of Education and Social Programs*,¹⁵ the little book of Campbell and Stanley has had a "profound influence" on his field. "Evaluation research" nowadays is an independent methodological branch of the social sciences. It has its own handbooks, chairs and departments. Specialists in evaluation methodology offer their service to governmental committees, publish their methodological comments in magazines and newspapers and are employed outside the universities in diverse "research institutions." Particularly in the United States evaluation methodology has gained a huge market, but in other countries it flourishes as well.

Recently, I set up a first small "pilot" study (as it is called in social scientific methodology) into the role of psychologists in "policy research institutions" like *The Institute for Applied Social Research Nijmegen*, *The Foundation Center for Educational Research Amsterdam*, and *The Dutch Institution for Preventive Health Care Leyden*.¹⁷ On the average 25% of the academic employees of these institutions appeared to be psychologists. The question "What is their most striking expertise?" in *each* case was not answered by pointing to empirical or theoretical knowledge in a relevant field but by mentioning "methodology". The question "What academic degree do the methodologists in your institution mostly have?" was answered by mentioning psychologists and sociologists.

But other examples of methodology as output can also be pointed out. Although the method of statistical comparison of groups subjected to different treatments was well known in medicine before it was in psychology,¹⁸ in due course psychologists were hired as methodological advisors for medical researchers. Methodological handbooks written for the social sciences in fact are studied in a wide range of disciplines including medicine, biology, history and liberal arts.

More examples of methodology operating *outside* academia can also be given. In the

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Netherlands, a recent public debate discussed the ways to collect evidence of incest. Psychologists joined in this discussion. Here as well, they mainly offered *methodological* standards for collecting acceptable evidence, rather than *empirical* knowledge on for instance children's minds and memories. The same is true for psychological expertise in court, where a psychologist not only may act as an expert on the memory of witnesses, but more often is a commentator on methodological ways to gather evidence.

Conclusions

As said, my claim is just to present some interrelated *hypotheses* on the social sciences' methodological sensitivity. In short, my guess is that this methodological rigorousness is due to the fact that in the course of their history the social sciences began to fulfill a far more practical social role than that of a theory directed science, which implied that they treated questions answered in many other sections of society as well. In consequence, an elaborate array of methodological rules and techniques were developed for the lowest levels of empirical research, that not only serve to demarcate the scientific answers from the non-scientific ones, but moreover began to function as an important part of the social scientific output themselves.

Many questions remain to be answered. First, more profound historical research should be done to check the validity of my ideas. Different social sciences in divergent countries should be compared. Moreover, new questions emerge. A very interesting topic seems to me, what might be called, the *policy making* role of social scientific methodology. It can be doubted that Campbell was right when he named himself and his companions just "methodological *servants*" of society, and when he maintained that "the job of the methodologist for the experimenting society is not to say *what is to be done*, but rather to say *what has been done*." (italics by Campbell)¹⁹

As has been argued in recent technology studies, the relationship between techniques and politics is not that simple. Technologists do not just provide means, they also create

aims; they do not just create the possibility to do things, their artifacts also define *what* to do. In a way, this can be shown to hold true for social scientific methodology as a kind of "social technology" as well. As a matter of fact, in the very same articles Campbell himself already provided striking examples. In his call for an "experimental approach to social reform," he pleaded for congressional decisions to establish "social indicators" and "socially relevant data banks." He also pleaded for the "education of administrators," to guarantee that, at the outset, administrative decisions "make allowance" for experimental or quasi-experimental designs. This for instance would imply that administrators use "staged innovations" and "allocate scarce resources by lottery" (to make sure that treatment and control groups can be randomly assigned).²⁰

Also the before mentioned Dutch discussion about the means to get evidence of incest, provides a clear example of the policy making role of social scientific methodology. In this debate psychologists rejected the unreliable method of interpreting children's play with so called "anatomically correct" dolls. In newspapers and committees they stressed their own severe methodological criteria, derived from quantitative techniques of mental testing. These criteria are designed to submit proofs to the severest demands. While some participants to the incest-debate argued that so much damage can be caused to children that not much risk of a false negative can be taken, these psychologists stressed --along with their methodological arguments--- the general judicial principle to let prevail the risk to acquit a guilty person over the risk to condemn somebody who is innocent. Pleading for higher methodological standards *implied* pleading for a lower risk to punish innocent parents at the cost of a higher risk to victimize the children again.

As in Campbell's case, I do not want to say that the psychologists were wrong, nor that they were right for that matter. Here, I only want to argue that with these methodological standpoints a definition or redefinition of the problem is at stake. Methodology is not just a neutral instrument; a methodological stand includes a stand on the right interpretation of the problem. Inavoidably, methodology defines what is to be

done.

As to the subject of methodology's contribution to the development of the social sciences, new perspectives are opened up. Let me end with just some brief remarks. First, recognition of the fact that the larger part of social scientific methodology is not *intended* to accomplish something like "rational theory choice," negates accounts arguing that methodology in fact does not contribute to theory choice or scientific progress at all. In this respect, natural sciences-based analyses just are not applicable to the social sciences. A re-assessment of methodology's contribution to the development of these sciences is needed. Perhaps, in the social sciences "growth" must not in the first place be looked for in the domain of empirical and theoretical knowledge, but in the ongoing refinement of their methods and techniques. While these are not more than conjectures, one thing does seem for sure. To maintain that the social sciences might as well drop their "methodological hang up" without any loss, testifies of little knowledge of these sciences. They make their *living* out of it.

1. Some analyses of its hidden purposes and social implications have been presented, though. For example: Shapin, S. & Schaffer, S. (1985) *Leviathan and the Air-pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press). Schuster J.A. & R.R. Yeo (eds.) (1986) *The Politics and Rhetoric of Scientific Method: Historical Studies* (Dordrecht: D.Reidel Publishing Company). K. Danziger, *Constructing the Subject: Historical origins of psychological research* (Cambridge: Cambridge University Press, 1990). T. Dehue, *De regels van het vak: Nederlandse psychologen en hun methodologie* [The rules of the discipline: Dutch psychologists and their methodology] (Amsterdam: Van Gennep, 1990: to be re-issued in English with Cambridge U.P.) investigates the question why methodology changes in the course of time.

2. There even are indications that the social sciences display a considerable larger degree of explicitly formulated methodological standards than the natural sciences. This phenomenon is, for example, confirmed by Winstons examination of 60 university level introductory texts in each of four disciplines: psychology, sociology, biology and physics. Between 1930 and 1980, the percentage of introductory texts with research methods

discussions in psychology increased from 50% to 90% and in sociology from 25% to 70%, while in biology from 20% to 45% and in physics as little as from 16% to 30%. In the 1970s, therefore psychology was leading with 90% of its introductory texts including sections on research methods, followed by sociology with 70%, while biology "lagged behind" with 45% and physics even with 30%. Neither in biology and physics *laboratory manuals*, Winston found discussions on experimental designs (A.S.Winston, *The Construction of Experimental Method in Introductory Texts: An Interdisciplinary Analysis*. Paper presented at the 13th annual meeting of the American Society for the History of the Behavioral and Social Sciences, Slippery Rock, 20-23 June 1991). Besides, autonomous handbooks on methodological matters usually are to be found in much greater numbers on the library shelves of social scientific departments than on those of the natural sciences. In the social sciences, there also are chairs and whole departments of methodology, where students can graduate as a "methodologist" and where researchers from other departments can obtain methodological advice (for which aim social scientific departments in the Netherlands even set up so called "methodology shops"). Recently a cd rom has become available with 30.000 references to modern literature on social scientific methods and techniques (SRM Documentation Center, Erasmus University Rotterdam, 1992).

3. Woolgar, S. (1988). *Science the Very Idea*. Sussex: Ellis Horwood & London: Tavistock Publications, p.107.
4. Cf. Dehue, T., *De regels van het vak*; Dehue, T. (1991) "Why Methodology Changes: Transforming psychology in The Netherlands", *History of the Human Sciences*, 4, 3, p.335-349.
5. See K. Danziger, *Constructing the Subject; Historical origins of psychological research*. On Gerard Heymans, see P.J. van Strien and J. Verster, "The response to Fechner in the Netherlands: Heymans' and Fechner's monism", in J.Brozek and H.Gundlach, eds., *G.T. Fechner and Psychology* (Passau: Passavia Universitätsverlag, 1988): pp. 169-78.
6. N. Rose, *Governing the Soul: The shaping of the private self* (New York and London: Routledge, 1989).
7. In one way or another, this phenomenon has been pointed at by other historians of psychology as well. K. Danziger, G. Hornstein, T. Dehue, P.J. van Strien, Ben David, Dale Stout.
8. Effective according to standards of the culture or subculture they form part of.
9. D.T. Campbell, "Factors relevant to the validity of experiments in social settings," *Psychological Bulletin*, 54, 4, 1957, pp.297-311.

10. D.T. Campbell and J.C. Stanley, "Experimental and Quasi Experimental Designs for Research on Teaching." In: N.L. Gage (ed.), *Handbook of Research on Teaching* (Chicago: Rand McNally, 1963).
11. D.T. Campbell and J.C. Stanley, *Experimental and Quasi Experimental Designs for Research*. (Chicago: Rand McNally, 1963).
12. D.T. Campbell, "Reforms as Experiments", *American Psychologist*, 1969, 24, p.409-429 (p.409).
13. This case was also published in D.T. Campbell & H.L. Ross, "The Connecticut Crackdown on Speeding: Time series data in quasi-experimental analysis." *Law and Society Review*, 1968, 3, 33-53.
14. D.T. Campbell, "The Social Scientist as Methodological Servant of Experimenting Society". In: S.S. Nagel, *Policy Studies and the Social Sciences*. Lexington: Lexington Books, 1975) p.27-31 (p.27).
15. L.J. Cronbach, *Designing Evaluations of Education and Social Programs* (San Francisco: Jossey Bass Publ., 1983).
16. Recently, I set up a first small pilot study (as it is called in social scientific methodology) into the role of psychologists in "policy research institutions" like *The Institute for Applied Social Research Nijmegen*, *The Foundation Center for Educational Research Amsterdam*, and *The Dutch Institution for Preventive Health Care Leyden*. On the average 25% of the academic employees of these institutions appeared to be psychologists. The question "what is their most striking expertise?" in each case was not answered by pointing at empirical or theoretical knowledge in a relevant field but by mentioning "methodology". The question "what academic degree do the methodologists in your institution mostly have?" was answered by mentioning psychologists and sociologists.
17. 12 forms were sent off, 8 were sent back.
18. This information (and the remark that there is no evidence that this kind of medical research had an influence on early psychological practice) stems from K. Danziger, *Constructing the Subject*. Danziger refers to A.M. Lilienfeld, "Ceteris Paribus: The evolution of the clinical trial," *Bulletin of the History of Medicine* 56 (1982):1-18.
19. D.T. Campbell, "The social scientist as methodological servant of the experimenting society", p. 27.
20. D.T. Campbell, "Reforms as Experiments", p.428.

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Groningen, The Netherlands, august 26-30 1992.

Education of deprived and retarded children: the struggle for influence between schooldoctors, experts on juvenile law and the behavioral scientists in the Netherlands in the second half of the nineteenth century.

Summary

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Three examples are given, on pedagogical pathology, on residential reeducation, and on special education.

In two cases, medicalization of education and pedagogics, in other words strong influence of the doctors, could be established. First of all, pedagogical pathology was strongly influenced by medical terminology. Secondly, (school) doctors dominated the development of special education. On the other hand, residential reeducation was build up without great medical influence. Experts on juvenile law were more important in that case, together with pedagogues.

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Education of deprived and retarded children: the struggle for influence between schooldoctors, experts on juvenile law and the behavioural scientists in the Netherlands in the second half of the nineteenth century (summary)

Jeroen J.H. Dekker, University of Groningen

In this contribution, we focus on the struggle for power - but at the same time forms of cooperation between rivals - on the domain of the education of deprived and retarded children in the Netherlands in the second half of the nineteenth century. Especially schooldoctors, experts on juvenile law and pedagogues, in other words representatives of the medical sciences, of the legal sciences and of pedagogics are playing the leading part. Three examples are given, on pedagogical pathology, on residential reeducation, and on special education at schools. Together, these three examples form a complex of new strategies of intervention concerning the education of deprived and retarded children in the Netherlands. The first example, on the genesis of special pedagogics - then called "pedagogical pathology" - as a scientific discipline with methods of diagnostics and therapy, belongs to the level of changing mentalities. The other two examples are on a new practice of education, of pedagogical intervention, on behalf of the deprived and retarded children. Thereby, we shall see that theory and practice influenced each other, especially in the development of dutch pedagogical pathology at the end of the 19th. century.

Our main question is about the influence of the doctors on theory and practice of (re-)education of the deprived and retarded children.

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INTRODUCTION

Medicalization of human behaviour seemed to become dominant in the second half of the 19th. century (cf. Gélis, Houwaart, De Swaan). De Swaan, however, goes not so far as to characterize all doctors as aggressive fighters, ready to gain total victory on their opponents. On the contrary, the greater part of the responsibility in this process of medicalization is given to other professional groups, like pedagogues, psychologists, etc., who prepared the way for the doctors, or, in any case, didn't make a successful stand against their advance. For the representatives of the medical professional group, therefore, it became almost inevitably to accept a victory they didn't pursue in such an aggressive way. In the words of De Swaan, restrained imperialism was really characterizing the doctor's strategies. Perhaps, that's a good description of what really happened. Other professional groups were playing the same game, and in confrontation with each other, the domain of human behaviour could be divided between the different professional groups and their scientific disciplines.

In this contribution, we focus on the struggle for power - but at the same time forms of cooperation between rivals - on the domain of the education of deprived and retarded children in the Netherlands in the second half of the nineteenth century. Especially schooldoctors, experts on juvenile law and pedagogues, in other words representatives of the medical sciences, of the legal sciences and of pedagogics are playing the leading part. Three examples are given, on pedagogical pathology, on residential reeducation, and on special education at schools. Together, these three examples form a complex of new strategies of intervention concerning the education of deprived and retarded children in the Netherlands. The first example, on the genesis of special pedagogics - then called "pedagogical pathology" - as a scientific discipline with methods of diagnostics and therapy, belongs to the level of changing mentalities. The other two examples are on a new practice of education, of pedagogical intervention, on behalf of the deprived and retarded children. Thereby, we shall see that theory and practice influenced each

other, especially in the development of dutch pedagogical pathology at the end of the 19th. century.

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THE INFLUENCE OF THE MEDICAL NOMENCLATURE

In two cases, medicalization of education and pedagogics, in other words strong influence of the doctors, could be established.

First of all, pedagogical pathology was strongly influenced by medical terminology. That was not surprising. Pedagogical theory on educational problems did not exist at all in the first half of the 19th. century (pedagogical theory on normal education and development was, of course, developing, but that's another story). At the same time, casuistry and nomenclature was developing within medical pathology. As we know, nineteenth century pedagogics was, like all social sciences, developing as a branch of philosophy. At the end of the century, medical nomenclature was put into pedagogical theories, becoming at the same time focused on empirical questions too, instead of being merely a moral science. While making use of medical nomenclature, the differences between medical and pedagogical pathology were stressed, that's to say by the pedagogues. The Leipzig professor Adolf Heinrich Ludwig von Strümpell (1812-1899) was of great importance for the development of dutch pedagogical pathology, and that's the reason to give him at this place some attention. Von Strümpell could combine the more philosophically tinged branch of pedagogics (in 1843 he published *Die Pädagogik der Philosophen Kant, Fichte, Herbart*) with the modern medical pathology in his work *Die Pädagogische Pathologie oder die Lehre von den Fehlern der Kinder. Versuch einer Grundlegung für gebildete Altern, Studirende der Pädagogik, Lehrer sowie für Schulbehörden und Kinderärzte*. The title alone gives us already an idea about his intention to write a book for pedagogues and doctors at the same time. Pedagogical science had to be more empirically, less philosophically, he argued (see Közle, on which he could support; see Depaepe on the development of empirical pedagogics more in general). Pedagogical pathology was, in the words of Von Strümpell "[d]ie Lehre von allen denjenigen Zuständen und Vorgängen, welche erfahrungsmässig während der Entwicklung des geistigen Lebens im Kindheitsalter von solcher Beschaffenheit sind, dass sie der Abschätzung und Werthbestimmung, nach denen der Pädagoge sie im Hinblick auf sie von ihm gedacht und erstrebte Jugendbildung auffasst und beurtheilt, sie entweder nicht als genügend oder als bedenklich oder schädlich, überhaupt als irgendwelcher hinsicht der Besserung bedürftige Fehler darstellen" (Von Strümpell, p. 16-17).

In nineteenth century Holland, systematic inventarisation of children's deficiencies, especially deficiencies of the moral kind, was realized by schoolteachers in particular. Children's deficiencies were considered fundamentally different from adult deficiencies, in so far as children's character and behaviour could be changed, while changing adult's behaviour and character was considered far more difficult, not to say nearly impossible. Children's deficiencies were formulated in normal, non-scientific language. Hundreds of deficiencies were formulated, such

as stubbornness, a propensity to steal, disobedience, etc. Jan Geluk, a schoolmaster, published a real inventory on that subject within his *Dictionary of Education* (1882), in which he summed up a tradition beginning with Father Cats, the famous moralist and Landsadvocaat of the Golden Age.

Within a few years, however, everything changed. Paedagogical pathology, with its own nomenclature, different from the ordinary language of Geluk and his tradition, developed. Its fathers were Jan Klootsema and K. Andriessse. They took over the existing medical nomenclature and put the new terms enthusiastically in their pedagogical theories. They wrote texts like "Introduction to pedagogical pathology and therapy", the first handbook on pedagogical pathology written in dutch, in which they tried to establish an independent position and a domain of its own for the pedagogical pathology. They found it necessary to overstress the differences with the medical pathology, a normal phenomenon in the history of the genesis of new scientific disciplines. A naive sort of arrogance was not strange to these representatives of the new discipline, of whom Von Strümpell considered his pedagogical pathology as even more difficult than the existing mother discipline, the medical pathology. Were not only a hundred childhood diseases known, while more than threehundred moral and pedagogical deficiencies could be mentioned?

In short, in developing special education as a discipline, pedagogues would like to make use of the nomenclature of their medical brothers, a nomenclature with already a high status in the academic world. At the same time, however, they stressed their autonomy and sometimes even their superiority as scientific discipline.

PEDAGOGICAL PRACTICE AND THE INFLUENCE OF THE DOCTORS

In our second example, educating the retarded children, pedagogical practice is under discussion. Around 1900, we can see the beginnings of the rapid expansion of special schools for the retarded. Earlier in the nineteenth century, experiments were developed. They were, however, not succesful in causing a real movement for the foundation of a series of schools. Experiments like that of Fokke Yntes Kingma in Amersfoort, Utrecht, and Amsterdam, the efforts of Van Koetsveld and his school for idiots in the Hague (see Dekker 1990a and Jak 1988) were certainly not unimportant. The break-through of the special school movement took place, however, around 1900, caused by the introduction of compulsory education in 1901. On the one hand, the retarded became visible - in fact, the category was constructed in function of the so called normal development of the other children and became filled with retarded children. On the other hand, retarded children became a problem, a burden for the normal children.

In short, for the sake of the "normal" as well as for the sake of the "non-normal", i.e. retarded, children, activities in the direction of special education could be justified. Pedagogues as well as doctors were willing to develop this new enterprise and to play the leading part in it. As in the development of pedagogical pathology, Jan Klootsema was the leading representative of the pedagogues, together with people like D. Köhler, head of the school for retarded children in Rotterdam,

and A.J. Schreuder, director of the *Heilpaedagogies Instituut Klein-Warpsborn*. On the side of the doctor's, A. van Voorthuysen and Van Herderscheê played the leading part.

The influence of these doctors was strong, the opposition, from schoolmasters and pedagogues, was strong too (see Dekker 1990a, p.118). Therefore, competition between doctors and pedagogues along with cooperation was possible at the same time.

There was, however, another development of intervention and treatment of children in care, namely the residential reeducation, our third and last example. The beginnings of this phenomenon can be seen in the thirties of the nineteenth century, in France (the agrarian colony of Mettray for criminal boys, in the surroundings of Tours) and Germany (the agrarian colony or "Rettungshaus" the Rauhe Haus, for deprived children in the neighbourhood of Hamburg). Within a few years, countries like Belgium and the Netherlands followed. We have, in short, to do with a European phenomenon. Dutch developments were part of this international movement. Dutch reeducation homes like "Nederlandsch Mettray" for protestant boys and the "Heldringstichtingen" for protestant girls were known. After 1848, a period of rapid expansion came, which continued until the beginnings of the First World War.

The realizing of this system of thousands reeducation homes, in which ten thousands of boys and girls were treated, was possible without influence of the medical sciences or the medical professional group. In the beginnings, it was first of all a philanthropical enterprise, with famous philanthropists like Charles Lucas in France and Willem Suringar in Holland. At the end of the nineteenth century, experts on juvenile law became important in developing child laws, realized in all European countries around 1900. Experience in pedagogical pathology was build up there over more than half a century, in about hundred reeducation houses, treating a great number of children (around 1900, residential capacity in Holland was about 12.000). This practical pedagogical pathology, consisting of diagnostics and therapy, was not founded on anything like scientific theory or scientific methods. On the other hand, the introduction and further development of pedagogical pathology in the Netherlands around 1900 was only possible in a climate, dominated by great experience with practical reeducation.

A person who combines our three examples, the pedagogical pathology, special schools for the retarded, and reeducation houses for deprived and criminal children, was Jan Klootsema. He was the founder of pedagogical pathology in the Netherlands. His publications and his book, however, were not based on theoretical interest. On the contrary, his practical experiences with retarded and criminal children brought him on the way of pedagogical pathology. As founder of the first Amsterdam school for retarded children and as director of the Rijksoopvoedkundig Gesticht (Reformatory School) for criminal boys in Alkmaar he laid the foundations for his idea of pedagogical pathology.

CONCLUSION

The basis of the genesis of special pedagogics as a scientific discipline has to be found in the educational practice, in the schools for retarded children, but first of all in the reeducational houses. Influence of the medical science was important in supplying, at the end of the century, the appropriate nomenclature, a nomenclature which the pedagogues needed so much. In setting up a system of schools for the retarded children, doctors tried to play the leading part. Partly, they succeeded in that ambition. The world of residential reeducation, however, was practically closed for them. In the beginning, philanthropists dominated that world. At the end of the century pedagogues and experts on juvenile law became more influential.

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Eugenische Bestrebungen im Umgang mit geistig behinderten
Menschen am Fallbeispiel einer konfessionell geleiteten
Einrichtung für behinderte Menschen in der Zeit von 1880
bis 1920

Eugenic Concepts in the Rehabilitation of Mentally Retarded
People in Germany 1880 - 1920: A Historical Case Study Exa-
mining a Confessional Institution

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Eugenic Concepts in the Rehabilitation of Mentally Retarded People
in Germany 1880 - 1920 : A Historical Case Study Examining a
Confessional Institution

Summary : In this survey it is examined if resp. in how far eugenic resp. socio-darwinistic concepts influenced the work with mentally handicaped people.

From 1880 when eugenic thoughts were spread enough until the time of 1920 the question is examined if this can be proved through the work of a confessional orientated institution for mentally handicaped people.

After the explanation of the terms "eugenic" an "socio-darwinistic" general informations about the institutional history are given.

Following the methodological approach is shown by exspecially refering to the printed sources.

Finally first results resp. "trends" are given from which indications to a growing influence of the socalled medical section and a support of eugenic thoughts are derivable.

Eugenische Bestrebungen im Umgang mit geistig behinderten Menschen am Fallbeispiel einer konfessionell geleiteten Einrichtung für behinderte Menschen in der Zeit von 1880 - 1920

(Ronald Doran und Paul Probst)

Zusammenfassung: Es wird der Vermutung nachgegangen, daß eugenische bzw. sozialdarwinistische Konzepte bereits früh Einfluß auf die Arbeit mit geistig behinderten Menschen genommen haben.

Für den Zeitraum ab etwa 1880, d. h. einem Zeitpunkt, zu dem eugenische Gedanken verbreitet genug waren, bis etwa 1920 soll der Frage nachgegangen werden, ob bzw. inwieweit sich in einer konfessionell geleiteten GroBeinrichtung für Behinderte Auswirkungen eugenischer bzw. sozialdarwinistischer Konzepte aufzeigen lassen.

Nach einer Erläuterung der Begriffe "Eugenik" und "Sozialdarwinismus" folgen allgemeine Informationen zur Institutionsgeschichte.

Anschließend wird der methodische Zugang erläutert, insbesondere zu gedruckten Quellen.

Abschließend werden erste Ergebnisse bzw. Trends vorgestellt, aus denen sich Hinweise auf eine zunehmende **Medikalisierung** bzw. eine Befürwortung eugenischer Konzepte ableiten lassen.

1. Einleitung

Anstoß zur vorliegenden Arbeit ist die aktuelle Diskussion in der Bundesrepublik Deutschland zu Themenbereichen wie Sterilisation und eugenischen Maßnahmen bei behinderten Menschen, humagenetischen Beratungen mit ihren Implikatio-

nen, hierher gehört auch die sogenannte Singer-Debatte.

Ein Blick auf die Literatur bzw. die wissenschaftliche Diskussion zeigt, daß der Themenbereich der Eugenik bzw. Rassenhygiene sowie der Euthanasie in den letzten Jahren ein verstärktes Forschungsinteresse auf sich gezogen hat, auch wenden sich Einrichtungen der Behindertenhilfe verstärkt der eigenen Vergangenheit zu. Hier liegen inzwischen eine ganze Reihe von Veröffentlichungen vor, an dieser Stelle sollen nur stellvertretend Klee (1983; 1985; 1986), Weingart u. a. (1988), Bastian (1990), Schmuhl (1987), Bradl (1991), Reyer (1991), Störmer (1991), für den angelsächsischen Bereich Proctor (1988) und Weindling (1989) genannt werden.

2. Fragestellung und wichtige Begriffe

2.1 Fragestellung

Im Rahmen der vorliegenden Arbeit soll der Vermutung nachgegangen werden, daß eugenische (im deutschen Sprachraum findet sich hierfür zumeist der Begriff **rassenhygienische**) bzw. sozialdarwinistische Konzepte bereits früh Eingang in die Arbeit mit geistig behinderten Menschen gefunden haben, dieses insbesondere bereits für den Zeitraum vor dem Nationalsozialismus von 1933 - 1945.

Dieses erscheint umso bemerkenswerter, als sich die eugenischen Vorstellungen deutlich sowohl gegen die christliche Ethik als auch gegen den Gleichheitsgrundsatz der Aufklärung richten, womit ganz entscheidend gesellschaftliche Werte und Normen berührt werden.

Innerhalb dieser Betrachtungsweise wäre dann der Nationalsozialismus ein brutaler und unbarmherziger Vollstrecker eines längst verbreiteten eugenischen bzw. sozialdarwinistischen Gedankengutes (vgl. zum Euthanasieprogramm der Nationalsozialisten selbst dann Klee, 1983, 1985; Wunder u. a., 1987; Schmuhl, 1987; Aly, 1989²).

Innerhalb der beforschten Einrichtung (gegründet 1863 von H. M. Sengelmann (1821 - 1899) soll anhand vorhandener Archivunterlagen (gedruckte Veröffentlichungen) versucht werden, nachzuvollziehen, inwieweit sich für den Zeitraum von 1880 - 1920 Veränderungen etwa beim Vokabular bzw. den sprachlichen Begriffen im Hinblick auf eugenische Konzepte auffinden lassen. Gefragt werden soll, ob sich hier eine kontinuierliche Entwicklungslinie aufzeigen läßt bzw. ob sich Widersprüche bzw. Diskontinuitäten finden.

2.2. Der Begriff "Eugenik"

Eugenische Vorstellungen und Ideen haben eine weit zurückreichende Tradition (vgl. dazu Weingart u. a., 1988). Der Begriff "Eugenik" stammt aus dem griechischen und bedeutet "das gute Erbe", er wurde durch Francis Galton (1822 - 1911) eingeführt, wobei Galton die sogenannte positive Eugenik im Vordergrund sah.

Zu unterscheiden sind hier die

1. "negative Eugenik", hier soll die Beseitigung "schlechten Erbgutes" aus dem Erbgut einer Bevölkerung dadurch erreicht werden, daß den Merkmalsträgern eine niedrigere bzw. keine Fortpflanzungsmöglichkeit eingeräumt wird und die
2. "positive Eugenik", hier wird den Trägern der als positiv definierten Merkmale eine höhere Fortpflanzungsmöglichkeit eingeräumt (sh. dazu insbesondere Weingart u. a., 1988).

Eugenik kann weiterhin durch zwei Paradigmen gekennzeichnet werden, und zwar

1. das selektionstheoretische Paradigma, welches auf Veränderungen gesellschaftlicher Gegebenheiten im Hinblick auf ihre eugenische Bedeutung zielt (es wird auch als darwinistisch-polulationsgenetisches Paradigma bezeichnet, tritt in der Entwicklung stärker hervor und ist an Begriffen wie Volk, Rasse, Genpool usw. orientiert) und
2. das medizinisch-humangenetische Paradigma, welches auf individuelle Erbkrankheiten und ihre Verhütung zielt.

Grundlage hierfür ist die darwinsche Evolutionstheorie, die ein biologisch begründetes Naturgesetz annimmt, nach welchem sich tierische Arten bzw. die menschliche Art kontinuierlich "höher"- bzw. "weiter"-entwickeln, wenn die selektorisierenden Faktoren nicht außer Kraft gesetzt werden (Darwin, 1963; Weingart u. a., 1988).

Hierauf aufbauend begründet sich eine neue Ethik, die scheinbar durch die darwinistische Biologie wissenschaftlich abgesichert ist (vgl. dazu Schmuhl, 1987).

Weingart (1988, S. 87) zufolge lautet die zentrale eugenische Botschaft dahingehend, daß die medizinische Pflege und Therapie, die Sozialversicherung, die Armenfürsorge und der Wohnungsbau, d. h. die gesamten sozialen Fort-

schritte nicht zu einer Lösung der sogenannten Degenerationsproblematik (vgl. dazu auch Hauss, 1989) führten, sondern diese im Gegenteil verschlimmerten.

Bei den eugenischen Programmen und Konzepten zeigen sich im Rahmen der wissenschaftlichen, religiös-ideologischen bzw. sozialpolitischen Umsetzungsversuche ausgesprochen widersprüchliche Züge.

2.3. Der Begriff "Sozialdarwinismus"

Unter dem Begriff "Sozialdarwinismus" sind nach Schmuhl (1987, S. 381) diejenigen Lehren zu verstehen, die das in der darwinistischen Biologie aufgestellte Evolutions- und Selektionsprinzip mit in die Beschreibung sozialer Prozesse einbeziehen. Insofern wird seitens der sogenannten Sozialdarwinisten ein Ausschluß sozialer Momente gefordert, die möglicherweise in eine "natürliche Selektion" eingreifen könnten.

Sozialdarwinistische Konzepte kommen ganz wesentlich im Rahmen einer krisenhaften Entwicklung der Industrialisierung und einer Finanzkrise des öffentlichen Gesundheitswesens zum tragen, in Deutschland insbesondere während der Weimarer Republik (vgl. dazu Baader, 1980).

3. Methodische Überlegungen und zeitlicher Rahmen

Die vorgestellte Untersuchung ist naturgemäß mit all den "typischen" Fehlerquellen und "Unzulänglichkeiten" behaftet, die sich bei einer "Rekonstruktion vergangener Wirklichkeit" und ihrer Betrachtung aus dem heutigen Blickwinkel ergeben.

Hier kann im Grunde genommen Borowsky u. a. (1980) zugestimmt werden, nach denen Vergangenheit nicht reproduzierbar bzw. rekonstruierbar ist, sondern daß es sich hierbei immer um eine "Konstruktion" handelt (vgl. dazu auch Lück, 1991; Nipperdey, 1986).

Zur zeitlichen Eingrenzung wurde der Zeitraum 1880 - 1920 gewählt, da zwar eugenische bzw. rassenhygienische und sozialdarwinistische Gedanken vereinzelt vor diesem Zeitraum diskutiert wurden, jedoch kann erst ab etwa 1880 mit einer weiteren Verbreitung gerechnet werden (vgl. dazu auch Schmuhl, 1987).

Der Blick soll verstärkt auf die Jahrzehnte vor dem Nationalsozialismus gerichtet werden, um hier eine evtl. bereits laufende, unheilvolle Diskussion bzw. Entwicklungslinie aufzuzeigen, die möglicherweise die letztlich erfolgte Vernichtung "lebensunwerten Lebens" mit ermöglicht bzw. ihr zumindestens keinen entschiedenen Widerstand entgegengebracht hat.

Beim methodischen Vorgehen habe ich mich am Konzept von J. G. Droysen orientiert, welches meiner Meinung nach die wesentlichen Punkte umfaßt und insofern auch heute noch wertvoll für ein psychohistorisches Vorgehen ist:

1. die historische Frage,
2. die Materialsuche (Heuristik),
3. die Materialkritik,
4. die Interpretation des Materials und
5. die Darstellung des Materials (Topik), sh. dazu Droysen (1868).

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4. Erste Ergebnisse

Nachfolgend möchte ich erste Belege bzw. "Tendenzen" darstellen, wie sie sich aus den bislang gesichteten, gedruckten Archivmaterialien der Alsterdorfer Anstalten ergeben.

- 4.1. Zunächst wurden die Protokolle der "Konferenzen für "Idioten-Heilpflege", hier die Bände 1874 - 1907, gesichtet. Diese "Konferenz" wurde maßgeblich mit durch H. M. Sengelmann, den Begründer der Alsterdorfer Anstalten, initiiert, wobei Sengelmann auch der langjährige Präsident bzw. ein Vorstandsmitglied dieser Vereinigung war. Diese

Konferenz tagte alle drei Jahre, über die Zusammenkünfte wurden dann "Konferenzprotokolle" veröffentlicht. Da die maßgeblichen Personen bzw. Einrichtungen der zeitgenössischen Behindertenarbeit in der "Konferenz" vertreten waren, kann vermutet werden, daß die Beiträge in der Regel stellvertretend für einen weiten Bereich der zeitgenössischen Behindertenarbeit angesehen werden können.

Aus den Tagungsprotokollen der "Konferenz für Idioten-Heilpflege" wird deutlich, daß sich im Laufe der Jahre Begriffe und Inhalte ändern, es zeigen sich zunehmend häufiger Vokabeln wie Lebenskampf, Entartung, Degeneration usw.

Bereits im Protokoll der I. Konferenz in Berlin (1874) wird auf Morel verwiesen, nach dem die Idiotie als Schlußstein der erblichen Degeneration durch mehrere Generationen hindurch anzusehen ist (Bericht über die Konferenz für Idioten-Heilpflege, herausgegeben von H. M. Sengelmann, Hamburg, 1874).

Nachdem sich die IV. Konferenz für Idioten-Heilpflege in Hamburg (1883) und die V. Konferenz in Frankfurt am Main (1886) mit exogenen Verursachungsmomenten der Idiotie beschäftigten, hier werden vorrangig die "Trunksucht" sowie der Einfluß der Ernährungssituation auf die Schwangerschaft genannt werden, wird im Rahmen der X. Konferenz für die Idiotenpflege und Schulen für schwach befähigte Kinder (es hatte eine Titeländerung stattgefunden) in Elberfeld (1901) die Bedeutung der "Degeneration" als Ursache für die Idiotie betont.

Darwin wird in verschiedenen Beiträgen zitiert und zur

Entstehung der sogenannten "psychopathischen Minderwertigkeit" werden neben Vererbungstheorien Umwelteinflüsse (Syphillis, Alkoholgenuß) angeführt, weiterhin wird zwischen der ererbten und der erworbenen "Minderwertigkeit" unterschieden.

Insbesondere im Zusammenhang mit Kostenberechnungen bezüglich der Betreuung behinderter Menschen werden eugenische Vorstellungen deutlich:

"Das Verhüten von Krankheiten und Kränklichkeiten ist ohnehin wichtiger und befriedigender als das Heilen" (Seite 119) bzw.: "Die nackten statistischen Zahlen über Irrenanstalten und Nervenheilstalten, über Idiotenanstalten und Schwachsinnigen-Schulen, über Rettungshäuser und Zuchthäuser mahnen uns, daß bald überall etwas Durchgreifendes geschehen sollte, auf daß statt einer sich verbreitenden und zunehmenden Degeneration eine Regeneration eintrete" (Seite 121, X. Konferenzbericht über die Konferenz für Idiotenpflege und Schulen für schwach befähigte Kinder in Elberfeld, 1901).

Die XI. Konferenz in Stettin (1904) befaßt sich mit medizinischen Aspekten bzw. der Rolle des Arztes in Schwachsinnigenanstalten, in einem Vortrag des Dr. Kellner (Oberarzt in den Alsterdorfer Anstalten) über die "Opium-Brom-Behandlung der Epilepsie" heißt es unter anderem: "Wir wissen nur, daß Erblichkeit und Trunksucht der Eltern prädisponierende Momente sind, ..." (Konferenz für das Idioten- und Hilfsschulwesen, herausgegeben von J. Schenk, Idstein, 1904).

Auch auf der XII. Konferenz im Chemnitz (1907) wird über die Aufgabenstellung der Ärzte in den Anstalten gesprochen, weiterhin findet sich eine interessante Berichterstattung bezüglich der Konferenz in der Zeitung "Chemnitzer Tageblatt" vom 17. 09. 1907, hier wird die Gefahr von Entartung und Degeneration als Zivilisationsfolge betont, es wird eine Zunahme von "Entartungser-

scheinungen" und von "geistig Minderwertigen" konstatiert.

Zwar wird in diesem Bericht den Behinderten das Recht auf Leben und Verbesserung der Existenz zugebilligt, immer wieder tauchen jedoch Begriffe wie Lebenskampf, körperliche und geistige Minderwertigkeit usw. auf.

4.2 Weiterhin wurden die "Briefe und Bilder aus Alsterdorf" (im folgenden abgekürzt als BBA) ausgewertet, hier die Jahrgänge 1877 bis 1919, die Jahrgänge 1883 und 1884 waren nur teilweise bzw. 1889 gar nicht vorhanden. Die "Briefe und Bilder aus Alsterdorf" erschienen in der Regel vier- bis fünfmal jährlich und beschäftigten sich vorrangig bzw. ausschließlich mit Themen aus dem Alsterdorfer Bereich, wie u. a. Bautätigkeiten, Mitarbeitersituation, Bewohnern, Vorstandsaktivitäten, hinzu kamen Artikel zu verschiedenen Themenbereichen der Behindertenarbeit. Als Hinweise auf eine zunehmende Medikalisierung der Behindertenarbeit finden sich in den BBA 1895 (Heft 3, Seite 3), BBA 1896 (Heft 3, Seite 7 ff.) und auch in den Folgejahren Hinweise auf eine Ausweitung der ärztlichen Dienstleistungen, 1896 wird eine Krankenstation eingerichtet, in den "Briefen und Bildern aus Alsterdorf" finden sich mehrfach Artikel des Anstaltsarztes bezüglich der Behandlung von Epileptikern, es erscheinen regelmäßig ärztliche Berichte über das abgelaufene Jahr usw.).

Um die Jahrhundertwende kommen veränderte sprachliche Formulierungen zum Tragen, so z. B. in den BBA 1901 (Seite 39):

"Der Begriff Idiot ist ein Sammelbegriff. Ich stelle darunter alle Blöd- und Schwachsinnigen, von dem elendsten Blöden vor, der eine

leere Menschenhülle und dazu noch eine äußerst defekte Menschenhülle zu sein scheint, ohne göttlichen, geistigen Kern, bis hinauf zu dem Schwachsinnigen, der an der Grenze der Schwachbegabten steht", jedoch auch: "Auch da, wo sich keine Spuren von Verstand finden, zeigt sich Gemüt" (BBA, 1901, Seite 41 f).

Wenn sich auch deutliche Aussagen für eugenische bzw. rassenhygienische Maßnahmen finden, so wird sich doch deutlich gegen Euthanasiemaßnahmen ausgesprochen:

"Wir maßen uns kein Recht zu, diese Geschöpfe von der Erde zu vertilgen, die ebenso gut wie wir unter dem Schutz des Gebotes stehen: Du sollst nicht töten, ..." (BBA, 1901, Seite 46).

Ein christlich-caritativer Ansatz zeigt sich etwa in den BBA 1904 (Heft 2, Seite 79):

"Oft ist beim Anblick dieser Kinder, keine Hoffnung auf Fortentwicklung geboten, gefragt worden: "Warum läßt Gott diese Aermsten leben?" Wir pflegen dann zu antworten: "Damit wir sie mit der Geduld der Liebe pflegen".

Sehr pointiert tritt die im Laufe der Jahrzehnte gewachsene Auffassung, eugenische Maßnahmen zu befürworten, Euthanasie aber abzulehnen, aus dem folgenden Text hervor:

"Wenn der Tod einem solchen Leben ein Ende setzt, dann sprechen wir wohl von Erlösung. Kommt dir beim Anblick dieser Jammergestalten vielleicht auch die Frage auf die Lippen: Warum verhilft man so einem elenden Dasein nicht zu einer Auflösung? Wie oft schon habe ich diese Frage hören müssen, und zwar immer stelle ich ihr die andere entgegen: Wer will die Verantwortung für eine solche Tat auf sich nehmen? Noch kein Fragesteller hat sich dazu bereiterklärt, und wahrhaftig, welche sittlichen Werte müßte unser Volk eingebüßt haben, eher es auf einer solchen Stufe der Gefühlsverrohung angekommen sein würde! Viel praktischer wäre es, die Frage nach der Verhütung solchen Elends, wie es unsere Anstalten bergen, wenn jeder Besucher einer Bekämpfer des Alkohols würde, dann wäre ein wirklicher Fortschritt gegen den Idiotismus zu verzeichnen, denn dieser Erzfeind unseres Volkes ist in mehr als 50 % die Ursache vom Zustand unserer Pflegebefohlenen" (BBA, 1910, Heft 2, Seite 89), im gleichen Sinne äußern sich auch die BBA 1918 (Seite 9).

5. Schluß

Zusammenfassend kann als ein vorläufiges Resümee festgehalten werden, daß sich über eine Betrachtung der "Konferenzen für das Idiotenwesen" sowie die "Briefe und Bilder aus Alsterdorf" für den Zeitraum von 1880 - 1920 ein allmählicher Begriffswandel im Hinblick auf eugenische Gedanken aufzeigen läßt, es tauchen Vokabeln wie Kampf ums Dasein, Lebenskampf, Degeneration auf, dieses führt bis zu einer Charakterisierung behinderter Menschen als "leere Menschenhülle und dazu noch eine äußerst defekte Menschenhülle" (BBA, 1901, Heft 3/4, Seite 40).

Als Beleg für eine fortschreitende Medikalisierung bzw. stärkere naturwissenschaftlich-medizinische Ausrichtung zeigt sich eine gestärkte Rolle des Arztes innerhalb der Anstalten, die sich in einer Erhöhung der Anzahl der Ärzte wie auch der Ausweitung des Aufgabengebietes ausdrückt.

Für den untersuchten Zeitraum von 1880 - 1920 finden sich weiterhin verstärkt "Nützlichkeitsüberlegungen" in dem Sinne, daß im Bezug auf die Betreuung behinderter Menschen Kosten und Nutzen gegenübergestellt werden.

Während eugenische Überlegungen durchgängig und positiv bewertet angestellt werden, wird eine Euthanasie an behinderten Menschen sowohl von Theologen als auch von Medizinern abgelehnt. Hier ist geplant, noch weiteres Material zur Beurteilung heranzuziehen, dieses, gilt insbesondere für die Vorstandsprotokolle aus dem Archiv der Alsterdorfer Anstalten sowie die Bewohnerakten.

Deutlich geworden ist, daß es sich bei diesem Begriffs- bzw. Einstellungswandel um einen allmählichen Übergang gehandelt hat, der zumindestens in den bisher gesichteten

Archivunterlagen "spurenhaft" und teilweise auch widersprüchlich zu Tage tritt. Augenfällig sind Parallelen zu heutigen Diskussionen, d. h. wiederum finden sich Standpunkte und Beiträge wie Kosten-Nutzen-Denken, Nützlichkeitsabwägungen im Hinblick auf ein "Lebensrecht" behinderter Menschen, deutlich ist das Bestreben, das technisch Machbare auch auszuführen.

Auch insofern erscheint eine Beforschung dieser Problematik relevant, um evtl. über eine Aufhellung historischer Vorgänge und Entwicklungen zu einem besseren Verständnis der zeitgenössischen Eugenik- bzw. Euthanasiediskussion kommen zu können (vgl. dazu auch Lück, 1991).

Abschließend möchte ich mit Nipperdey (1986) einschränkend darauf hinweisen, daß die Geschichte uns zwar die Möglichkeit gibt, aufgrund der Kenntnis der Vergangenheit die Gegenwart besser zu verstehen, eine verbindliche "Sinnorientierung" kann Wissenschaft jedoch nicht vermitteln, d. h., wir erhalten dadurch kein Urteil über Werte bzw. die Wahrheit von Werten.

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PUT TO THE TEST

The history of psychological testing

Slides presentation

Peter van Drunen/Willy de Konink

(Fnd. Historic Materials of Psychology, Groningen)

Pictures constitute not only a valuable source of historical information, but also provide a means for a lively presentation of the history of psychology. With this in mind, Historic Materials of Psychology is developing a slide series on one of the central fields of psychological practice: psychological testing. At the CHEIRON-meeting, a provisional version of this slide series will be presented, including about 50 slides.

Psychological testing is a twentieth century phenomenon. In other forms, however, attempts to detect the particular psychological propensities and capacities of individuals have a much longer history. This early history is presented by slides showing techniques like astrology, physiognomy and phrenology.

With regard to psychological testing as it developed from the 1890's onward, emphasis is on three important traditions, that dominated the field up to the 1960's: intelligence testing, psychotechnics, and projective techniques. Apart from the tests themselves, the slides give an impression of the background of psychological testing and the context in which the tests were used.

The pictures used in the slide series are mainly derived from a travelling exhibition on the same topic, which is touring the Netherlands since spring 1991. The series is meant for use by teachers of psychology and the history of psychology, at universities as well as schools for professional education (teachers, social workers, etc.). In its definitive form, the slide series will become available in spring 1993, together with an accompanying text explaining the slides.

For further information:

Stichting Historische Materialen Psychologie

P.O. Box 1710

9701 BS GRONINGEN

**Miracles and Dreams. 18 th Century Psychological
Representations of the Other of Realism.**

(Angelika Ebrecht FU Berlin)

In the 18th century miracles and dreams were considered as private disturbances in a common world that was supposed to be governed by the laws of reason. It is being argued that they were regarded as well as the psychic raw material from which conclusions could be drawn to a reliable other world within. In contrast to an outer world which was being valued as a safe territory for the activities of understanding this inner world seemingly appeared as the other of reason which was yet to be explored. The discourse on phenomena that seemed to contradict reason led to a theory of representation which gave rise to later thoughts of a generalized inner otherness as a foundation of psychic and social reality.

Träume und Wunder. Die psychische Repräsentanz der anderen Wirklichkeit in Theorien des 18. Jahrhunderts

Angelika Ebrecht (FU Berlin)

(Vorläufige Fassung)

Im 18. Jahrhundert galten Träume und Wunder als Störungen der Vernunft und der Wahrnehmung von Realität. Sie wurden aber auch als Repräsentanten einer anderen, inneren Wirklichkeit begriffen. Im Gegensatz zur äußeren Objektwelt, die zunehmend als sicheres Terrain der Verstandestätigkeit galt, erschien diese innere Welt als das Andere der Vernunft, das erst noch erkundet werden mußte.

Ich möchte im folgenden zeigen, wie durch den Diskurs über scheinbar widervernünftige Phänomene eine Theorie der Repräsentation innerer Andersheit als psychischer und sozialer Realität entstand. Die Vorstellung von einem inneren Anderen ermöglichte den Blick auf den generalisierten Anderen in sich. Diese Entgegensetzung von sozialem Subjekt und innerer Andersheit ließ einen Riß im Erkenntnissubjekt entstehen. Aus ihm entwickelten sich nicht nur Ansätze psychischer Introspektion, sondern auch das moderne erkenntnistheoretische Problem, wie die innere, subjektive Selbstwahrnehmung von der äußeren, objektiven Tatsachenerkenntnis - oder psychoanalytisch: wie Übertragungsphänomene von realitätsgerechten Einschätzungen - zu unterscheiden seien.

"Ein Traum, sei er auch noch so kurz und unbedeutend, sollte (...) für den Psychologen immer eine der wichtigsten Erscheinungen sein" (Voß 1786, S.249) schrieb 1786 Christian Daniel Voß. Und bereits 1753 äußerte ein anonym Autor die Ansicht, man könne "durch Auslegung" von Träumen auch anderen "ersprießliche Dienste leisten" (Anonym 1753, S. 158). Er selbst träume nicht nur "von Sachen, die mir vordem zu Gesichte" gekommen sind, sondern auch von solchen, die "auf das Zukünftige" verweisen (S.159f.). Diesem Verständnis zufolge besitzen Träume also eine vorausschauende Erkenntniskraft, die dem naturwissenschaftlichen Gesetz von Ursache und Wirkung entspricht.

Es war David Hume, der 1748 in "An Enquiry Concerning Human Understanding" versuchte, das Ursache-Wirkungs-Verhält-

nis auf Erfahrung zu gründen. Im 10. Kapitel "Of Miracles" räumt er jedoch ein: "Though experience be our only guide in reasoning concerning matters of fact; it must be acknowledged, that this guide is not altogether infallible, but in some cases is apt to lead us into errors." (Hume 1956, S.113) Wunder seien solche Irrtümer; denn sie durchbrächen das Verhältnis von Ursache und Wirkung. "A miracle is a violation of the laws of nature" (S.118). Dennoch hielten die Menschen gern an ihrem Wunderglauben fest: "The passion of surprise and wonder, arising from miracles, being an agreeable emotion, gives a sensible tendency towards the belief of those events, from which it is derived." (S.121) Der Glaube erscheint als eine psychisch bindende, emotionale Kraft. Insofern sind Wunder für Hume in erkenntnistheoretischer Hinsicht keine Störfaktoren, sondern notwendig, um den Glauben an die christliche Religion zu begründen.

"Mere reason is insufficient to convince us of its veracity: and whoever is moved by faith to assent to it, is conscious of a continued miracle in his own person, which subverts all the principles of his understanding and gives him a determination to believe what is most contrary to custom and experience." (S.136)

Der Glaube durchbricht nach Hume zwar die Regeln der Vernunft, er repräsentiert aber etwas Ungewöhnliches, aller Erfahrung Widersprechendes. Er schafft das Bewußtsein von einem Wunder im Individuum als dessen je persönliches Geheimnis. Auf diese Weise wirkt er daran mit, das Bewußtsein von etwas Fremdem, Anderem im Individuum zu etablieren, das zunächst als unerkenntlich gilt.

Christian Friedrich Pockels knüpft 1785 an Humes Argumentation an. Er behauptet jedoch, der Mensch sei so an die Verbindung von Ursache und Wirkung gewöhnt, daß er im Wunderglauben "unsichtbare Wesen zu den Ursachen ihm unerklärbarer Begebenheiten" (Pockels 1785, S.252) mache. Dies setze den Geist in einen Zustand "beständiger Spannung", da er sich noch keinen genauen Begriff von der Sache machen könne. "Unbefriedigte Wißbegierde ist also vornehmlich, was unsere Seele so geneigt gegen das Wunderbare macht." (S.256) Nicht etwa Aber-

glaube und Erkenntnisfeindschaft, sondern Wißbegierde und emotionale Spannung motivieren also nach Pockels den Wunderglauben. Denn, so Pockels, durch das Wunderbare werde die "Einbildungskraft" in Bewegung gesetzt, was "einen besonders hohen Grad des Vergnügens" bedeute (Pockels 1785, S.255), aber auch "Erstaunen, Furcht und Schrecken" freisetze (S.260). Der Wunderglaube kann demnach ebenso sehr als emotionaler Antrieb von Neugierde und Erkenntnisdrang begriffen werden, wie auch als Flucht aus der Realität in innere "Leidenschaften".

Die Auffassungen von Hume und Pockels widersprechen der Behauptung von Hartmut und Gernot Böhme, die Etablierung der Vernunft Herrschaft im 18. Jahrhundert sei erkaufte worden "mit der Angst, von allem abgeschnitten zu sein, und vor der Auslieferung an die Gespinste der Innerlichkeit." (Böhme / Böhme 1985, S.240) Der von den beiden diagnostisierte "Angstspannung" zwischen der Vernunft und der von ihr beherrschten inneren wie äußeren Natur (S.18) entspricht vielmehr eine Neugierspannung. Diese stellt sich erst durch die Entdeckung des "inneren anderen" her (Todorov 1983, S.293) und wird dann jedoch durch die Trennung in Innen- und Außenwelt ein emotionaler und motivationaler Antrieb zur Erforschung beider Sphären. Während die äußere Realität sich zunächst selbst zu repräsentieren scheint, kann das innere Andere nur vermittelt über fremde und rätselhafte Repräsentanten erschlossen werden.

Wie vieles äußere Fremde galten die Repräsentanten des inneren Anderen ebenfalls oft als böse und verdorben. Auch Wunder und Träume mußten im 18. Jahrhundert gegen den Vorwurf verteidigt werden, sie seien unmoralisch. 1745 meint ein anonymes Autor, Träume seien nicht nur "leere Schatten und Gespenster", sondern besäßen "den Werth (...) moralischer Fabeln", da sie dazu dienen könnten, die "verhaßte Wahrheit" darzustellen (Anonym 1745, S.89). Johann Heinrich Jung-Stilling will 1808 sogar "zeigen, daß es unter tausenderley Träumen, Täuschungen, Dichtungen und Phantastereyen doch noch immer einige wahre und unläugbare Ahnungen, Gesichte und Geister-Erscheinungen gebe, womit der Satan und seine Engel nichts zu tun haben." (Jung 1808, S.6) Wird hier die These von der göttlichen Abkunft der Wunder gegenständlich gewendet, so

entgegnet dieses Argument dem Vorwurf, Wunder wie Träume seien lediglich Täuschungen und damit verwerfliche Produkte der Einbildungskraft.

Der Einbildungskraft, die im 18. Jahrhundert als ästhetisches Vermögen Geltung gewann, kam bei der theoretischen Etablierung innerer Andersheit eine entscheidende Rolle zu. Ihr wurde die Fähigkeit zugesprochen, nicht die wahre Realität zu erkennen, sondern eine wahrscheinliche Welt darzustellen. Dies trug ihr (in Platonischer Tradition) einerseits den Vorwurf ein, widernatürlich, lügnerisch und krankmachend zu sein. Andererseits wurde ihr eine realitätskonstituierende Fähigkeit zuerkannt. Insofern war fraglich, in welchem Verhältnis sie zu den Wahrnehmungen und Empfindungen der Realität stehe.

1762 bezeichnet ein anonym Autor die Einbildungen "als ein Echo der Empfindungen" (Anonym 1762, S.259). Würden diese "verdunkelt" und jene "erhitzt", so könnten die Einbildungen "einen so starken Grad der Lebhaftigkeit erhalten, daß sie den Empfindungen gleichen", wie es nicht nur bei "betrunkenen und verrückten Leuten", sondern mitunter auch im Schlaf der Fall sei (S.258). Träume und Wunder galten aber meist nicht als Abkömmlinge realer Empfindungen, sondern als genuine Produkte der Einbildungskraft und wurden als Störungen der Realitätswahrnehmung aufgefaßt. 1747 berichtet ein anonym Autor von "eine(r) gefährliche(n) Seuche, welche man die Einbildungs-krankheit nennete" (Anonym 1747, S.154). Poeten, Maler, Melancholiker, Traumdeuter, Wahrsager u.a. litten an dieser Unfähigkeit, sich auf das wirklich Vorhandene zu konzentrieren. Pockels gibt 1789 zu Bedenken, "daß unsre Erinnerungskraft im Traume oft ganz ausgetilgt zu seyn scheint, und wir den Faden nicht wieder finden können, wodurch der Traum mit der wirklichen Welt zusammenhängt" (Pockels 1789, S.63).

Träume wurden also als Bedrohung des Realitätskontakts und des inneren Zusammenhangs des Bewußtseins aufgefaßt. Damit stellten sie aber auch das Bekannte der wirklichen Welt infrage und drohten es, dem Geheimnis des inneren Anderen entsprechend, in ein unbekanntes Territorium zu verwandeln. Aber uneingestanden war die psychische Repräsentation der vorratio-

nen, inneren Andersheit bereits eine Voraussetzung des Glaubens an Erfahrungstatsachen und Vernunftregeln. Kant etwa setzt ja in seinen kritischen Schriften die unerkennbaren Vernunftideen als Möglichkeitsbedingungen von Erkenntnis a priori voraus, versucht allerdings nicht, sie aus besonderen Phänomenen wie dem Wunder und dem Traum indirekt zu erschließen. Aber an solchen Phänomenen brach doch ein innerer Widerspruch zwischen der Realitätsmächtigkeit des Verstandes und der Verführung zur Ergründung des Anderen in der Vernunfterkennung auf. Denn sie führten auf das Problem, nach welchen Kriterien die Realität oder Irrealität der psychischen Repräsentanz eines Phänomens zu beurteilen sei.

Zu Beginn von Kants "Kritik der reinen Vernunft" (1781) ist das Verhältnis von "Erscheinung" und "Ding an sich" ein zentrales Thema. Kant grenzt zwar das Ding an sich als unerkennbar aus der Erkenntnis aus, sieht es aber dennoch als deren notwendige inhaltliche Entsprechung an. Zugleich macht er mit der "transzendentalen Einheit des Selbstbewußtseins" als deren "Vorstellung des Ich denke", die alle anderen Vorstellungen muß "begleiten können" (Kant 1976, S.136), ein mit sich identisches und kontinuierlich seiner selbst bewußtes Ich zur Grundlage von Vernunft und Realitätsbezug. Phänomene wie Träume und Wunder durchbrechen nun sowohl die Vorstellung eines "stehenden und bleibenden" Ich wie auch der dinglich realen Erscheinungen und müssen deshalb eine Verunsicherung der Vernunft zur Folge haben.

Im "Magazin zur Erfahrungsseelenkunde" trugen Salomon Maimon und Joseph Veit einen Streit über das Verhältnis von Traum und Realität aus. Veit behauptet dort 1791, "Verrücktheit, Faselei und der Traum" sei gemeinsam, daß "in diesen Zuständen Gedankendinge für außer uns vorhandene Dinge gehalten werden." (Veit 1791, S.194) Er kommt zu dem Schluß, "die Unterbrechung der Ideenreihe" durch sinnliche Eindrücke sei "ein Kennzeichen einer äußeren Wirklichkeit, und die Nichtunterbrechung derselben ein Kennzeichen, daß die Vorstellung ihr Dasein bloß in mir hat." (S.81)

Maimon dagegen hält 1792 eine ununterbrochene Ideenreihe für realitätsgerecht und ihr Unterbrechen für das wesentliche

Merkmal von Träumen (Maimon 1792, S.64). Während im Wachen die Assoziationen zweckorientiert verliefen, herrsche im Traum die Einbildungskraft und es "durchkreutzen sich alle Assoziationsarten" (S.58). Im Wachen gebe es dagegen eine "Macht des Vorsatzes", (S.110), die es dem Ich ermögliche, "dem Zwecke treu zu bleiben und den Ausschweifungen der Einbildungskraft Einhalt zu tun" (S.111). Man gerate aber "einigermaßen außer sich", wenn die "Selbstmacht" des Selbstbewußtseins, d.h. die willkürliche Fortsetzung oder Unterbrechung einer Ideenreihe verloren gegangen sei (S.60).

Während Maimon sich in der Tradition Kants auf die Kontinuität der Vernunft beruft und Träume als Störungen und Ver-rückungen der Realitätswahrnehmung interpretiert, fragt Veit umgekehrt, wie es möglich sei, "daß wir irgend etwas als ein Ding ansehen, welches außer uns wirklich" ist, und: "warum sind wir nicht alle erklärte Egoisten?" (Veith 1793, S.87). Hier deutet sich die Auffassung an, daß, gerade durch die Unterscheidung von Innen und Außen, Ich und Anderem das Selbstbewußtsein eine soziale Dimension enthält. Wenn für Veit Träume also die Frage nach dem Anderen in und außer uns aufwerfen, behauptet Kant 1766 umgekehrt in "Träume eines Geistersehers":

"Aristoteles sagt irgendwo: wenn wir wachen, so haben wir eine gemeinschaftliche Welt, träumen wir aber, so hat jeder seine eigene. Mich dünkt, man sollte wohl den letzten Satz umkehren und sagen können: wenn von verschiedenen Menschen ein jeglicher seine eigene Welt hat, so ist zu vermuten, daß sie träumen." (Kant 1905, S.342)

Diese eigene Welt wäre eine gegen Innen und Außen isolierte. Eine solche wäre aber im Sinne des späten Kant widervernünftig und würde damit auch die Existenz des individuellen Selbstbewußtseins in Frage stellen.

Nach Veit verunsichert der Traum zwar das Selbstbewußtsein, setzt es aber ebensowenig ganz außer Kraft wie die Fähigkeit zur Realitätsprüfung. Veit behauptet, daß wir im Traum "kein sehr lebhaftes Bewußtseyn von unserm Dasein haben; denn dieses hängt von der Wahrnehmung der inneren Gedankenreihe ab; das Ich ist demnach in diesem Zustande nur schwebend." (Veit 1791, S.204) Das schwebende Ich ist ein vom inneren

Anderen fasziniertes, das sich der Sicherheiten der Vernunft begeben hat. Seine Anerkennung ermöglicht eine Entdecker- und Forscherperspektive der eigenen Psyche gegenüber, was wiederum die Voraussetzung dafür bildet, sich mit dem Dasein des äußeren Anderen zu befassen.

Die Entdeckung des inneren Anderen bot die Grundlage zur späteren Entdeckung einer psychischen und einer sozialen Dimension der Vernunft und der philosophischen Einheit des Selbstbewußtseins. Mit dieser Entdeckung des eigenen, unbekannten Selbst geht in unserem Jahrhundert die Etablierung eines "generalized other" einher, einer sozialen Identität des individuellen Selbstbewußtseins (Mead, 1980, S.319). Phänomene wie Träume und Wunder führten also zu dem Problem, wie das innere Andere im Verhältnis zum äußeren beschaffen sei und stellten die scheinbar unbezweifelbare Gewißheit der Repräsentanz von Realität durch die vernünftigen Erkenntnisvermögen infrage. Gerade angesichts der jüngsten Diskussionen über die zunehmende Mediatisierung von Erfahrung und das angebliche Verschwinden von Realität heute gewinnt dieser Problemkomplex zunehmend an Aktualität.

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SUMMARY

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CRISIS AND CONTINUITY IN THE WORK OF THE DUTCH 'MODERN' SOCIOLOGIST JACQUES VAN DOORN.

In this paper the focus is on one person, not on a research programme or discipline. In the case of the Dutch sociologist Van Doorn, it is stressed that his work was not identical to his programme 'modern sociology' that came under heavy criticism during the sixties at the outbreak of the so-called 'crisis' in sociology. This major change that occurred at the level of the programme and the discipline thus looks somewhat more complex at the personal level. In the case of Van Doorn change is not a (Gestalt) switch, but a process. It is claimed that not all the established aims and claims of a social scientist have to change at the same time, and that some don't change at all.

CRISIS AND CONTINUITY IN THE WORK OF THE DUTCH 'MODERN' SOCIOLOGIST JACQUES VAN DOORN

In recent studies of the social sciences, the object of study often is a research group, school or discipline. Thus, major changes like the so called 'crisis' in sociology at the end of the sixties, are portrayed as discipline-wide shifts in subjects and styles of research, theories and methods. Concerning the crisis in Dutch sociology, this shift from the Dutch mainstream called 'modern sociology', to new sociological variants is said to consist of:

the shift from an onworldly, timeless sociology of the social system with role-like behaviour towards a socially relevant sociology that is more historically orientated and that questions positivist fact-finding in favour of more contemplative, constructivist and reflexive lines of thought. In an opposing view of neo-positivist origin however the shift is seen as leading towards a refining of methods and theory and towards a new integration of theory and empirical data.

In this paper I would like to focus not on a group, but on one person, Jacques van Doorn, one of the most famous, and the most cited Dutch sociologists of the second half of the twentieth century, who is considered the spokesman of modern sociology that existed as a group with its own journal until the early seventies. This because I am interested in the question how a person who starts working and thinking in terms of a programme or school reacts when this very programme becomes heavily criticised and is abandoned.

I chose to look at the career of one person because I hoped to find a 'Gestalt switch' at the personal level of what was a major change at the level of the discipline. I ended up noticing that change is a process and not a switch. As I will show now, not all the established aims and claims

of a social scientist have to change at the same time. Moreover not all the claims have to change.

Why focus on a person, rather than on a group or discipline ? What will we gain by doing so ? Let me first stress that on the subject of change in Dutch twentieth century sociology, two group-studies already exist. They offer a great deal of information. By looking more closely to one of the leading figures, Jacques van Doorn, I noticed however that the story of change was more complicated than the shift mentioned above from structural-functionalism towards more reflexive lines of thought. First I found that Van Doorn was not really identical with the programme of modern sociology - to give an example, he brought in personal accents like an interest in history which is said to be absent from modern sociology. As a consequence of this I wondered whether the changes he underwent could be connected to this personal style and his attitudes, to themes and concepts he had started using since the beginning of his career and his broader outlook on life and worldview.

So my aim is to add to the group-study a more detailed personal study, which some of you might recognize as psychology of science. I'm not sure, personally, where the dividing-line between sociology and psychology lies. There are some concepts I could use from cognitive psychology or psycho-history, as 'cognitive resources', used by Giere (1988) by which he meant both exemplars and available apparatus, or 'conceptual tools' as used by Professor Van Strien from the University of Groningen. These concepts are useful if we use them in a broad sense, covering a range of factors such as the empirical-theoretical schemes used by a scientist, the world-view of a scientist, his or her view about the professional status of the discipline and the way its professionals should operate in society.

I will now give a summary of the career of Jacques van Doorn using these three factors.

Jacques van Doorn started working at the university in the fifties and ended his academic career in the eighties - though he is still very much present in today's academic and public circles. He started his career partly as an autodidact because he couldn't go to the university at first because of the war, and because he had to join the army in the Dutch East-Indies afterwards. During his self-tuition he was greatly influenced by historians like Spengler and Romein. In the colonies the rational organization and subsequent irrationality of violence and war-crimes fascinated him, and kept fascinating him ever since. His PhD thesis was a history of organizations, and the history of the army in particular. Back in Holland still as a student he foresaw a big part for sociology to play in the rebuilding of society. These ideas were common in the social-democratic climate in Amsterdam where he studied after the second world war. The work of Mannheim was known and cited in political and academic circles. The political views of Van Doorn however were not very outspoken. In fact he was doing what all the others in Amsterdam at the time were thinking. Let us look at his world view for a moment. He defined society as 'the organized society', where different sectors worked harmoniously together - there was no class struggle. Professionals did their jobs with responsibility at every level in every sector, and even workers were seen as professionals. Sociology should take a leading part in orchestrating this society. Professional sociologists should study the most important sectors of society, like the economy or the state administration. They should not hesitate to give advice and influence policy on the basis of their knowledge. Van Doorn got a chance to put his idea about a

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professional training for sociologists into practice when he became head of the new Department of Sociology at the university of Rotterdam.

Given the harmonious world-view and the idea of a socially relevant, i.e. policy relevant sociology, what were the scientific claims of Van Doorn ? As a consequence perhaps of his autodidact past, he had an eclectic body of theory, which he wrote down in the famous Dutch textbook 'Modern sociology' which became the textbook with the most copies ever sold in Holland. In this book bits of system-theory and behaviourism can be found stressing the durability of society and the roles people have, but also bits of Weberian 'Verstehende sociology'. Testing of hypotheses is seen as an adequate way to obtain facts. In the journal 'Sociologische gids' (Sociological guide) that he founded, refining of methodology became more and more important in the course of the sixties, which did not please Van Doorn very much, although he had to admit that he had propagated more sophisticated methods in the fifties. In the articles he wrote in that time, he did not use the theoretical apparatus of structural-functionalism he worked out in his textbook. He covered a range of subjects using pragmatically what piece of theory suited him best, and did this with an essayistic style. This made contemporaries wonder if there was another Van Doorn besides the author of 'modern sociology'.

A brief characterization of the two decennia before the so-called crisis might be, that Van Doorn tried to establish a discipline by being spokesman of a group of younger sociologists that he brought together in a new journal, that he advocated scientific methods, higher standards of theory, a professional status and a possibility to work in society.

During the sixties the ideal of a harmonious society became more and

more in contradiction with reality. In the wider society and at universities people wanted more democracy. At the end of the sixties Van Doorn stuck to his professionalism. Professors form a profession and they should decide what the curricula looked like and not the students. This attitude changed at the beginning of the seventies. After the oil-crisis, Van Doorn founded a new journal where the relation between policy and society was the central theme. So policy remained a central issue for Van Doorn but it was no longer seen as a self-evident sector to work in, it was now questioned in its working. Sociology as discipline was not very important to him any more, every (sub)discipline that had something to say about the issue was welcome. Another change is the growing suspicion towards professionals in society. He analyzed the rise of the affluent society and saw how powerful professionals had become. Institutions as clinics, schools etc. had relations with the government. Individuals became too dependent and could't influence or check the services of the professionals. What we see at the same time is a political shift. He quite suddenly realized that he was not really in the social-democrat line of thought. He now thought he was liberal-conservative. Sociologists should not want to orchestrate and reshape society - patterns that had grown through the years were safer than their constructions. He now denounced Mannheim because of this. As an effect of this criticism, sociologists saw the role they had been adviced to play in society before, taken away from them - if Van Doorn got his way. During the seventies Van Doorn warned them to be more modest about their role. It took him until the eighties to give them a new role to play in society: that of critic, intellectual, and public debater. So what we have here is a sudden shift in world-view, political stance and, connected to a growing questioning of

professionalism, a gradually declining interest in the old professional role of the sociologist in society and the emerging of a new professional role: that of critic.

But what happened to his scientific claims since the sixties? The answer is: really not that much! His textbook was published again and again with no big changes. The authors admitted there was too much harmony in it but warned against the new bias to see society only as a scene of conflict. Social system, social control, roles, all these concepts were still considered worth-while, and Van Doorn still uses them today. It is as though these concepts belong to his mother tongue, though his native country has ceased to exist. The broader structural-functionalist programme of which these concepts were part, is not used any more by Van Doorn in his broader work. Although we can ask ourselves if he ever used it at all. He was, as you may recall more of an eclectic than a theorist. Van Doorn no longer believed in value-free facts any more: he now thought knowledge was tied to societal groups. But now he propagated a sociology that would be as value free as possible, the sociologists still having an advantage over the journalist. So without a very solid basis, the sociologist still had a role to play in society on the basis of his or her scientific knowledge. To come to a conclusion, let me summarize the findings. The personal style of Van Doorn remained in tact throughout his career, like his essayism - only the audience differed; before the sixties he wrote for an academic and governmental audience, afterwards he wrote for the public in general. Themes like his interest in history, organizations and policy stayed too. So does his eclecticism, which seems to make him atypical before the crisis and somehow naively unaware of the consequences of debates on foundations in sociology during and after the crisis. His world-view and

political awareness underwent dramatic changes, going from an optimistic social-democratic stance towards a pessimistic liberal-conservative one. The active role for sociologists in society he opted for changed in character, but not in its essence. His reflexivity stays within the limits of the age-old legitimacy of and respect for academic work. So Van Doorn did have a crisis, but it was limited mainly to the contents of the role of the sociologist and to his outlook on life and society.

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The Brain - Mind - Computer Trichotomy; A Historical and Logical Analysis

Abstract

A historical analysis of the evolution of three concepts, namely brain-mind problem, brain-computer analogy/disanalogy and the computational theory of mind is given in the light of new developments in brain theory and cognitive science. There is a tendency to treat the three - traditionally disjoint - dichotomies as a trichotomy.

1. Brain - mind - computer

The term "brain" is many times associated to the notions of "mind" and of "computer". The brain-mind-computer problem has been treated within the framework of three separate dichotomies. First, the brain-mind problem is related to the age old philosophical debate among monists and dualists. Second, the problem of the brain-computer analogy/disanalogy has been the product of early cybernetics, recently revived by the neurocomputer boom. Third, according to the suggestion of the computational theory of mind, the computational metaphor is considered as the final explanation of the mental processes. "Connectionism" might be qualified not only as a new paradigm of cognitive science, but as an ambitious movement. Based on the principles of "brain-style-computation" it offers a conceptual framework for a would be general brain-mind-computer theory. Hopes and disappointments of the earlier euphoric period associated to the emergence of cybernetics and information theory is analysed.

The story of the rise, fall, renaissance, and metamorphosis of the early neural network models is reconstructed.

2. Brains and mechanisms: historical reconstruction

The history of biology could be characterized by two, rather competitive approaches. While the "engineer" emphasizes the importance of the constituents of the living being, the "holist" considers the whole as somehow (?) more than the sum of its parts.

The brain has been saddled with many metaphors. Technical

devices, as telegraph circuits and holograms have been offered as model of the brain. Of course, the most important analogy arose in the time of the birth of cybernetics, suggesting that the brain could be considered, as a computer. The old question got new lightening recently. Whether our mind is "ghost in the machine"? What could be the similarities between brains and computing devices? What are the irreducible differences between them? Is there any real chance for "manufacturing" artificial intelligence?

3. Over the mechanistic paradigm

The concept of causality, one of the most important supporting pillars of the mechanistic world view is analysed. Not only linear causality, but circular and network causalities are proper ideas to understand the emergent complexity of the brain and of the mind. Circular causality relies on the suggestion that in a feedback loop there is no meaning in separating cause and effect, since they are mixed together. The term network causality has been coined to describe interactions among circles. Causal phenomena in cognitive network have to be reinterpreted by using these concepts.

Although vague in many respects, the idea of self-organizing is nevertheless a powerful concept. It is suggested that the very essence of the nervous system is its self-organizing character.

4. Change of paradigm from dynamic structuralism to hermeneutics

The research of dynamic structures, such as rhythmicity, chaos and spatial patterns, led to the weakening of the mechanistic theories. In spite of their ambitious endeavours, and undeniable success, no theory of brain can be given within a pure structuralist framework. Brain theory (and evolution theory) ought to be "poststructuralist". One important aspect of the brain (and mind), which can not be incorporated within an even (dynamic) structuralist theory, is its self-referential character. Self-reference means that it refers not only to an object but simultaneously to itself. The popularity of this notion, rather neglected earlier, has tremendously been increased (see Hofstadter's celebrated book: Goedel, Escher, Bach).

5. Poststructuralist brain theory

A poststructuralist brain (mind-computer) theory might include three ingredients, namely autopoiesis (Maturana & Varela), endophysics (Roessler), and hermeneutics. What can a brain theoretician do after realizing the crisis of the notion of universal rationality? She/he can attempt to accept that modern (reductionist) science lack self-reflexivity, therefore she/he might turn to hermeneutics emphasizing the cyclic nature of perception and learning. The concept of "preunderstanding" and hermeneutic processes of the brain could be associated to chaotic information processing.

Cheiron

Abstract of a paper on

THE INVENTION OF THE PUBLIC

(Jaap van Ginneken, University of Amsterdam)

(This is a paper on the research project *The invention of the public - The introduction of the survey-method in the Netherlands, and the involvement of psychologists* between 1925 and 1965. It was one half of a twin project subsidized by the Psychon Fund within the Netherlands Organization of Scientific Research. It was carried out at the 'Foundations and History' Section of the Psychological Institute of Groningen University and supervised by Prof. P.J. van Strien. Both projects deal with the emergence of psychological professional practice in this country. This part knowingly chose a field somewhat marginal to the discipline, in order to see how psychologists interacted with other professions in the establishment of a particular niche. By contrast, the other half will concentrate on a field which is very central to the psychological profession: i.e. testing and selection).

1 The 'invention of the public' in the Netherlands was approached at successive stages through four case-studies, each dealing with different aspects of the problem. The first case-study deals with a kind of prelude: the emergence of the **psychology of advertising** during the interwar years. A wide array of academic psychologists seems to have been involved in advertising consultancy at one point or another, although this is hardly mentioned in the existing literature. Their advice was mainly based on the results of experimental psychology, and the laws of perception, thought and memory. It was enthusiastically solicited and welcomed by the advertising world, which was itself in the process of moving from mere 'space' brokering to expert assistance. The author claims that the psychological laws outlined the 'general reactions' of the 'wider public' to persuasive messages without, however, providing very detailed knowledge of this 'dark continent'.

2 The second case-study deals with the emergence of **market- and opinion research** around the Second World War. This, it claims, truly initiated the 'mapping' of the psyche of the general public. Sampling methods helped identify the proportions in which certain subgroups were represented; scaling methods helped probe the intensity of their feelings. All this made it possible to distinguish target groups, focus communication efforts, evaluate their results. It was an interdisciplinary venture, in which psychologists only played a subsidiary role, alongside sociologists, economists and - last but not least - statisticians of various backgrounds. Most of the techniques, however, were imported directly from the United States, and various American and international bodies played an important role by commissioning overt and covert opinion research - the latter mostly related to the onset of the Cold War.

3 The third case-study deals with one particular application of this new method in company surveys. Both Marshall-aid and subsequent 'counterpart funds' put a heavy emphasis on the introduction of newly developed American management techniques. One such method was the company survey, which was to help identify leadership-and-morale problems on the factory floor, help solve them and thus increase productivity. Under this program, a number of major research projects were carried out within Dutch industry, by a combination of university professors, consultancy services, semi-government institutions and others. These 'social psychological projects', as they were called, played a key role in introducing new types of social research in this country. And also in training a new generation of social researchers - which came to fill new university chairs in social psychology, industrial psychology and their like.

4 The fourth and final case-study shows how, from the mid-fifties on, psychologists claimed a more exclusive role in certain strands of market research: more specifically in motivation research. Quantitative methods of opinion polling were held to be poor tools when it came to probing the consumer's soul. Qualitative methods of 'depth' and group interviewing were considered to be an irreplaceable supplement - which could best be realized by trained psychologists. This shift was related to the emergence of the consumer society, and was linked to the themes of subliminal perception, hidden persuaders, etcetera. Interestingly enough, these controversies seem to have had a dual effect. On the one hand they caused rather a stir and quite some public criticism of advertising psychology. On the other hand, it reminded entrepreneurs that psychologists knew some very special tricks, and could not be ignored.

Throughout these four case-studies on 'the invention of the public', the author follows a consistent scheme. Every chapter begins with a section on the new 'societal problems' which psychologists and opinion experts were trying to address. One such problem was the fact that the scale of most organizations, and the distance between planners and target groups, had become such that personal contact had become illusory, and purposefull 'mass persuasion' had become imperative. It therefore became increasingly important to anticipate the choices of the individual, to try and guide them, and to develop methods for evaluating the effectiveness of persuasion attempts. This was the prime raison d'être of the new types of opinion research. A

Each chapter then devotes a section to the institutional infrastructure within which these problems were articulated. On the one hand, the social and psychological sciences got organized: in an academic as well as a professional sense. They branched out into various applied fields. On the other hand, new functions and departments arose within industry as well. B

Efficiency consultants, personnel managers, advertising experts, marketing consultants were all looking for more scientific guidance. The two groups interacted in various ways, pondered solutions, transformed problems, and gradually came up with new ways of dealing with them.

Subsequently, another section of each chapter explores the concepts and methods which were employed to this effect. Gradually, a different vocabulary was generated in order to outline the contours of the public and its inclinations: terms like 'opinions', 'attitudes', 'belief systems' took on new and more precise meanings. Simultaneously, new techniques were developed to make these measurable and calculable. What distinguishes modern public opinion and the survey from its predecessors are first and foremost: a) a sense of the proportion of subgroups, established through sophisticated sampling techniques; and b) a sense of the intensity of their feelings, established through 'multiple choice' (fixed response categories) and scaling methods. This implied a thorough revolution. The previously vague entity of the 'general public' thus became a concrete object which could be explored in infinite detail - in order to anticipate its reactions and improve efforts at information, communication, persuasion.

This was not entirely non-controversial, though. The final section of each chapter is therefore devoted to the way in which psychologists and other experts presented and legitimated these activities to other groups. Obviously they had to address the 'forum' of their colleagues within their own subdiscipline or discipline. But also a wider academic public, in order to convince them that they contributed to the advancement of the intellectual profession. Furthermore, they had to address their prospective clients - usually through the mass media - as well as the general population, in order to convince them that they were able of reconciling the general with private interests. There was a remarkable contrast, for instance, between the degree to which industrial psychologists experienced public acceptance of their work, and that of advertising psychologists. In consequence the latter never organized within the professional organization of psychologists, but rather among themselves - together with economists, sociologists, political scientists and others working within the same marketing field.

The 'invention of the public' was of course not a Dutch affair: some prewar concepts were imported from Germany, most postwar methods were imported from the US. Survey technology has now become a worldwide phenomenon. Yet the author maintains it is primarily related to a highly industrial, rather individualistic, but also liberal-democratic society. In pre-industrial, collectivist, authoritarian and/or totalitarian societies the problem of the systematic anticipation of 'free choice' - and subtle attempts at correcting and guiding it - do not have the same reason-to-be. It is through the mapping of the spontaneous inclinations of the

public, through the communication of images, through shaping its subjectivity, that our society is governed.

Since psychology is the science of subjectivity par excellence, this field of professional practice is bound to expand even further. Even though it may take the form of the incorporation of psychological knowledge in other academic disciplines, and the spreading of psych-speak through education and media ...

1^e syst opinie - onderz. Napoleon!

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Zur Entwicklung der Würzburger Schule der Denkpsychologie unter besonderer Berücksichtigung von Külpes Persönlichkeit

Oswald Külpe (1862-1915) spielte während der Entstehung und Entwicklung der experimentellen Psychologie eine besondere Rolle. Als Schüler von Wilhelm Wundt und Georg Elias Müller war er dem Experiment in besonderer Weise verpflichtet, doch spürte er wie viele andere das Bedürfnis, nicht nur Empfindungen, Vorstellungen und Gefühle experimentell zu untersuchen, sondern sich auch "der luftigen Gedanken" anzunehmen, höhere geistige Prozesse experimentell und eben nicht mit den Methoden der Völkerpsychologie (Auswertung von Tätigkeitsergebnissen) zu untersuchen. Die Methode der Wahl war hierbei die systematische Anwendung der kontrollierten, also von einem Versuchsleiter initiierten und kontrollierten Introspektion. In der Selbstbeobachtung geübte Versuchspersonen berichteten unmittelbar nach deren Ablauf über ihre "Denkerlebnisse". Dabei entdeckte man Vorgänge, Zustände, Richtungen, Akte, die sich dem Schema der älteren Psychologie nicht einfügten.

Külpe hatte den intellektuellen Mut, Wundts experimentelle Ansätze auf die schwerer und nicht so exakt faßbaren Denk- und Willensvorgänge auszudehnen und die Selbstbeobachtung so weit als möglich zu objektivieren, da diese bei der Erlebnisforschung nicht ausgeblendet werden kann, ohne daß wesentliche Erkenntnisbereiche vernachlässigt werden müßten. Dabei unterliefen allerdings methodische Mängel, da zwischen Denk- und Wissensreproduktionsvorgängen nicht sauber geschieden wurde.

Da Külpe von der Persönlichkeit her sehr sachlich, zurückhaltend und bescheiden war, ist es schwierig, seinen Anteil an der Würzburger Schule herauszustellen. Dies ist erst durch die Auswertung des Briefwechsels etwas genauer möglich.

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Zur Entwicklung der Würzburger Schule der Denkpsychologie unter besonderer Berücksichtigung von Külpes Persönlichkeit

In seinem Nekrolog auf Oswald Külpe beklagt Clemens Baeumker den frühen Tod dieses unermüdlischen Forschers "von ebenso umfassenden Kenntnissen, wie ursprünglicher Geistesstärke und Schaffenskraft". Er sei "ein beliebter und erfolgreicher Dozent (gewesen), der, frei von allem prunkenden Pathos und aller selbstgefälligen Geistreichigkeit, allein durch die ausgereifte Gediegenheit des Inhalts sowie durch die strenge Sachlichkeit und die vollendete Klarheit seines Vortrags dauernd zu fesseln verstand, ein Lehrer von selbstloser Hingebung für seine Schüler, denen er, wie in der Wissenschaft, so auch in allen Angelegenheiten und Sorgen des Lebens mit treuem Rat und helfender Tat allzeit fördernd zur Seite stand, ein Charakter von höchster Zuverlässigkeit in allen Lebenslagen, bei dem feste Zähigkeit und Bestimmtheit des Willens mit Weichheit des Gefühls und Milde des Urteils sich paarte, ein allzeit gütiger Menschenfreund, der im stillen ungezählte Wohltaten spendete, ein liebenswürdiger und feinsinniger Kollege, mit einem Worte: ein ebenso hervorragender Gelehrter, wie wahrhaft edler Mensch".

Die aus dem Nachruf Clemens Baeumkers zitierte Passage macht deutlich, daß wir es mit einer Persönlichkeit von besonderem Format zu tun haben, einem Format, das auch für einige Besonderheiten der Würzburger Schule verantwortlich ist. Insbesondere ist es die persönliche Rolle Külpes innerhalb dieser Schule, die durch seine fast übermäßige Sachlichkeit und Bescheidenheit selbst solchen Gelehrten gegenüber, die ihn gut zu kennen meinten, teilweise im dunklen geblieben ist.

Külpe wurde am 03. August 1862 (neuen Stils) in Candau bei Tukum in Lettland geboren. Nach dem Maturitätsexamen (1879) trat er für 18 Monate eine Lehrstelle für Geschichte und andere Fächer am Knabenpensionat der Forstei Kursitten an. 1881 ging er nach Leipzig, wo das Wundt'sche Institut für Experimentalpsychologie gerade das 3. Jahr existierte, um Philosophie, vor allem aus Interesse an ethischen Fragestellungen, zu studieren. Er nahm gleichzeitig ein Studium der Geschichte, vor allem Kulturgeschichte, im Leipziger historischen Seminar unter der Leitung von Prof. Karl von Noorden auf. Durch Wilhelm Wundt wurde er für die Experimentalpsychologie interessiert. Nach einer kurzen Episode 1882/83 in Berlin, wo er Vorlesungen bei den bedeutenden Historikern Mommsen, Kirchhoff, Diels und Treitschke hörte, ging er 1883 zu Georg Elias Müller nach Göttingen, zwei Jahre, nachdem Müller Nachfolger Lotzes geworden war. Der überaus strenge G.E. Müller war für seine einzigartige methodische Ausbildung weit bekannt, so daß selbst Edmund Husserl seinen Schülern empfahl, bei Müller Vorlesungen zu hören, weil er Wert darauf legte, daß man die Methoden der positiven Wissenschaften kennenlerne.

In Göttingen bekam Külpe denn auch das Thema seiner Dissertation "Zur Theorie der sinnlichen Gefühle", die er am 10. Oktober 1887 schon in Leipzig bei Wundt verteidigte. Ein Jahr später, 1888, habilitierte sich Külpe, der doch zwei dem Experiment besonders verpflichtete Lehrer hatte, wieder mit einer theoretischen Übersichtsarbeit: "Die Lehre vom Willen in der neueren Psychologie". Zwar gibt es von ihm eine Reihe von minutiöser Genauigkeit zeugende experimentelle Publikationen, seine eigentlichen Anliegen finden sich aber auch später, gerade zur Entwicklung der Experimentalpsychologie oder Denkpsychologie, in solchen theoretischen Übersichtsarbeiten wie:

"Anfänge und Aussichten der experimentellen Psychologie" (Berlin 1893), "Erkenntnistheorie und Naturwissenschaft" (Leipzig 1910), "Über die moderne Psychologie des Denkens" (Berlin 1912), "Über die Methoden der psychologischen Forschung" (Berlin 1914).

Von 1886 bis 1895 war Külpe an der Leipziger Universität als Student, Assistent Wilhelm Wundts und seit 1888 als Privatdozent tätig. Er hielt sowohl psychologische wie philosophische Lehrveranstaltungen.

Zum 16. Oktober 1894 wurde Külpe als ordentlicher Professor für Philosophie und Ästhetik nach Würzburg berufen, wo er bis 1909 wirkte. Dieser fruchtbarsten Zeit seines experimentalpsychologischen Schaffens folgten Ordinariate in Bonn und München, wo sich sein Wirkungsfeld stark vergrößerte, doch konnte er infolge seines frühen Todes am 30. Dezember 1915 dieses Arbeitsfeld nur noch kurze Zeit bestellen.

Die "Würzburger Schule" erwarb sich durch experimentelle Untersuchungen zur Denkpsychologie internationales Ansehen, wurde aber von Seiten der Hauptvertreter der experimentellen Psychologie, so von Wundt und G.E. Müller, kritisiert, weil sie die Methode der Introspektion verwendete.

Külpe entwickelte aber den Ehrgeiz, alle psychologischen Probleme experimentell anzugehen, gerade auch die, die Wilhelm Wundt der Völkerpsychologie zugewiesen hatte. Um die Jahrhundertwende machte sich nämlich verstärkt eine gewisse Unzufriedenheit mit dem bis dahin erreichten Stand der Erforschung höherer geistiger Prozesse in der experimentellen Psychologie bemerkbar. Diese Unzufriedenheit bezog sich vor allem auf die klassische Assoziationstheorie, die auf David Hartley, John Locke und David Hume zurückgeht.

Külpe artikulierte 1912 in seinem Artikel "Über die moderne Psychologie des Denkens" diese Unzufriedenheit so:

"Es ist dem Denken in der früheren Psychologie meist nicht die genügende Beachtung geschenkt worden. Und die experimentelle Richtung hatte zunächst so viel in dem massiveren Hause der Empfindungen, Vorstellungen und Gefühle Ordnung zu schaffen, daß sie sich erst spät der luftigen Gedanken annehmen konnte."

Um diese Einseitigkeit der experimentellen Psychologie zu überwinden und das Denken nicht nur, wie in Wundts Völkerpsychologie, auf Grund von Tätigkeitsergebnissen zu

untersuchen, erweiterte ein Kreis junger Psychologen um Oswald Külpe den Forschungsbereich des Experiments auf das Denken und den Willen. Die herkömmliche Methodik des psychologischen Experiments reichte für diese Zwecke allerdings nicht aus.

Die neue Methodik wurde von Oswald Külpe und seinen Mitarbeitern entwickelt, wobei sie durch Karl Marbe und Karl Bühler in weiten Kreisen bekannt und später von Külpes Bonner Schüler Otto Selz zur höchsten Reife gebracht wurde. Külpe faßte 1912 sein zentrales methodisches Anliegen so zusammen:

"Was uns in der Psychologie zu einer anderen Theorie schließlich geführt hat, ist die systematische Anwendung der Selbstbeobachtung gewesen. Früher war es üblich, nicht nach jedem Versuch über alle Erlebnisse während desselben Bericht erstatten zu lassen. ... So trat nur das Größte ans Licht. Auch verhinderte der Anschluß an die herkömmlichen Begriffe der Empfindungen, Gefühle und Vorstellungen ein Bemerken und Benennen dessen, was weder Empfindung noch Gefühl noch Vorstellung war. Sobald man nun anfang, in der Selbstbeobachtung geübte Personen über die Erlebnisse eines Versuchs unmittelbar nach dessen Ablauf vollständige und unbefangene Mitteilung machen zu lassen, wurde die Notwendigkeit einer Erweiterung der bisherigen Begriffe und Bestimmungen offensichtlich. Man entdeckte in sich Vorgänge, Zustände, Richtungen, Akte, die sich nach dem Schema der älteren Psychologie nicht fügten. Die Versuchspersonen begannen in der Sprache des Lebens zu reden und den Vorstellungen nur eine untergeordnete Bedeutung für ihre Innenwelt beizulegen. Sie wußten und dachten, urteilten und verstanden, ergriffen den Sinn und deuteten die Zusammenhänge, ohne eine wirkliche Unterstützung durch gelegentlich auftretende Versinnlichungen dabei zu erhalten."

Träger des Denkprozesses, so arbeiteten die Würzburger heraus, sind nicht die Vorstellungen, obwohl sie notwendig sind, um den Denkfluß in Gang zu halten. Vielmehr sind dies die Verknüpfungen der 'gemeinten Objekte', der 'unanschaulichen' Denkgebilde oder Gedanken.

Im Vortrag folgen nun Details zur Entwicklung und Anwendung der Methodik der Denkpsychologie anhand von Beispielen. ✓

Külpe selbst war zwar Anreger vieler der Experimente seiner Würzburger Mitarbeiter und Schüler. Er selbst hielt sich aber mit Publikationen deutlich zurück, sodaß die Kritik der Gegner an den "Ausfrageexperimenten" an Marbe und Bühler adressiert wurde. Aus dem Briefwechsel zwischen Wundt und Külpe erhellt, wie enttäuscht Wundt von seinem einstigen Schüler Külpe war, als er erfuhr, daß dieser wirklich der "spiritus rector" der Würzburger Schule war, hatte Wundt doch die denkpsychologischen Experimente für Marbes und Bühlers Werk gehalten.

Külpes Übervorsichtigkeit, seine diplomatische Zurückhaltung gegenüber seinen Lehrern und Schülern führte dazu, daß sich originelle Talente unter seiner Leitung ungestört entwickeln konnten. Da Külpe selbst vor allem als Versuchsperson in Erscheinung trat und auf diese Weise auch höchst indirekt auf die Entwicklung seiner Mitarbeiter Einfluß nahm, ist sein persönlicher Anteil an der Arbeit seiner Schule auf den ersten Blick schwer einschätzbar. Hier muß wirklich der Briefwechsel zu Rate gezogen werden.

Das Hauptverdienst Külpes besteht darin, den intellektuellen Mut gehabt zu haben, Wundts experimentelle Ansätze auf die schwerer faßbaren Denk- und Willensvorgänge zu erweitern und die Selbstbeobachtung, indem sie durch einen Versuchsleiter kontrolliert wurde, qualifiziert und so weit als möglich objektiviert zu haben.

Gewiß haften den Versuchen der Würzburger gewisse methodische Mängel an, da u.a. Denk- und Wissensreproduktionsvorgänge anfangs nicht sauber geschieden wurden. Dies gelang erst dem Bonner Külpe-Schüler Otto Selz, der auch Gesetze der Denktätigkeit formulierte und der eigentliche Vollender der Würzburger Schule genannt werden darf, weil er über Külpe und Bühler weit hinausging.

Warum
is das
ein
Verhängnis?

Die experimentalpsychologische Arbeit Külpes führte zu einer schrittweisen, wenn auch wegen seiner Vorsichtigkeit nur andeutungsweise erkennbaren Wandlung in seiner philosophischen Haltung, die ihn mehr und mehr zu einem scharfsinnigen und profilierten Vertreter des kritischen Realismus und zum Eintreten für eine induktive, an den Ergebnissen der Einzelwissenschaften ausgerichtete Metaphysik werden ließ.

For Freud Against Freud!": Political Metaphors
and Left Psychoanalysts in the 1930s

"My dear little man, I was discovering the functional law of life while you were shouting from the rooftops, 'He's crazy!' At that time you happened to be a little psychiatrist with a past in the youth movement and, due to your impotence, a cardiac future... When you thought I was done for, you ceased to be my friend and became my enemy. You tried to give me the coup de grace because, though you knew I was right, you were unable to keep pace with me... Didn't you, in your pussyfooting organization, pass off my teachings as your own? I assure you that the honest members were aware of this. I know, because they told me. Underhanded tactics, little man, can only bring you to your grave before your time." --Wilhelm Reich, speaking of his former comrade Otto Fenichel in Listen Little Man (1946)

Based on previously unreported papers and correspondence of Otto Fenichel, Wilhelm Reich, and their close collaborators, we describe the formation of a psychoanalytic Left opposition in Europe in 1930-1935. Contrary to Russell Jacoby's The Repression of Psychoanalysis and standard histories of the field, the early careers of Fenichel and Reich were irrevocably determined by the two men's struggle for the leadership of the psychoanalytic left opposition. This opposition, centered around a Marxist-Freudian Rundbriefe which Fenichel edited, functioned as both a discussion group and a left caucus within the International Psychoanalytic Association (IPA). Ignored by historians such as Peter Gay, the Rundbriefe group shaped mainstream Freudianism by provoking a theoretical and organizational backlash.

In reintroducing the figure of Wilhelm Reich into the history of the Rundbriefe group (and correcting the portrayal by Jacoby) we do more than tell a previously untold tale. We present this brief episode as a case study of the psychopolitics

of innovation and marginality in the behavioral sciences. Further, it is a case of psychopolitical metaphors both enriching and constricting the work of young theorists.

For Otto Fenichel, the struggle against revisions of Freudian psychoanalysis (e.g., by A. Kardiner) was an intellectual-political cause to which he was willing to devote his career. In that struggle and earlier during his psychiatric training, Fenichel used his understanding of one system (politics) to navigate through another (science and medicine). As a medical student in 1919, for example, he criticized conservative sexologists as "the Social Democratic Party of the soul," referring to a SDP which had been discredited for its World War I national chauvinism. Pursuing the analogy, Fenichel challenged his colleague Siegfried Bernfeld, "let us be the Communist Party [of the psyche]," i.e., a Freudian vanguard within psychiatry.

Twenty years later, Fenichel's psychological attraction to analogies remained, as did his goal of an intellectual revolution in psychiatry. What had changed were the tactics necessary to operate in a world transformed by fascism. Where before he urged the psychiatric equivalent of a revolution (as led by Freud), Fenichel now recommended an alliance with the psychoanalytic equivalent of the bourgeoisie. Faced with the challenge of neo-Freudianism, Fenichel wrote that theorists such as Karen Horney → [HORNE] acted like Nazis by masquerading their rightist ideology in the

guise of Leftism (i.e., National "Socialism"). To oppose such psychoanalytic fascism, Fenichel explained, it was necessary for the opposition to unite with the conservative leadership of the IPA. Defending such unsavory political bedfellows, Fenichel characterized the arrangement as equivalent to the Soviet Union fighting Nazis in an alliance with Great Britain-- a capitalist country run by a Conservative Party.

Opposed to Fenichel's strategy was Wilhelm Reich, who had followed his young colleague into both socialist politics and psychoanalysis. In Reich's view, the incipient fascism of the psychoanalytic establishment was the main danger against which he, Fenichel, and their comrades (e.g., Edith Jacobson) must struggle. To that end, Reich called for Marxist analysts to denounce the IPA leadership and the increasing conservatism of Freud himself. "For Freud Against Freud!" was one of Reich's slogans, using a Leftist grammar to express the dialectical assumption that Freudianism contained political contradictions requiring resolution through struggle. In Reich's estimation, an open walkout or expulsion of left-socialist analysts would split the IPA down the middle, winning liberals and fellow travellers to the side of the revolution. In fact, the IPA leaders (!) successfully isolated the opposition and expelled Reich. Fenichel then purged Reich and one of his followers from the Rundbriefe group, turning the opposition into his own private discussion circle.

Reich → sex pol
+ popular with students

Our paper narrates the Reich versus Fenichel struggle, tracing their relationship from student days to their collaboration in Berlin and then into exile in Scandinavia. Using the papers of Fenichel, Siegfried Bernfeld, Anna Freud, Ernest Jones, and David Rapaport as well as Fenichel's Rundbriefe, we supplement and correct previous works such as autobiographical and biographical accounts of Reich and his career.

By detailing the political interests and quarrels of Reich and Fenichel we show how these men were shaped by the same constellation of forces that they attempted to study: politics, personality, institutions, and ideas. For both Fenichel and Reich, all these elements determined-- and overdetermined-- each change in tactic, each elaboration of an underlying strategy. Both men were creative intellectuals whose personal psychologies were both constraining and inspiring. Both sought out institutional and political power, and each dramatically influenced the organizational events in which they participated. True to their belief in dialectical change, each shaped the other's response through their competition for influence, through their sharing of ideas, and through the clash of their psychological styles.

For both Fenichel and Reich, the psychopolitical analogies through which they understood the world were a source of clinical insight and creativity. When applied to the psychoanalytic

movement, however, such analogies also became a self-fulfilling source of political isolation. In Reich's case, equating the IPA leadership with Nazis justified his own sectarianism and provocations when dealing with colleagues. Such an anti-social, combative style, in turn, provoked an authoritarian response that validated his perception of the leadership as fascists. It also fomented conflicts within the IPA which strengthened the hand of those who best fit this attribution.

In Fenichel's case, loyalty to Freudian institutions was coupled with the characterization of neo-Freudians as pseudo-left, psychoanalytic right-wingers. Applying this attribution to socialists and non-socialists alike, Fenichel became isolated from exactly the sort of therapeutic post-Freudians that might have become attracted to the periphery of the Rundbriefe circle. His anti-revisionist polemics, in turn, did nothing to slow the departure of figures such as Karen Horney from official Freudianism, decreasing the junior ranks in the IPA that competed for organizational power. This enhanced Fenichel's organizational status by default, and further involved him in intramural maneuvering. Pursuing such work in the company of psychoanalytic Tories --to paraphrase Fenichel's own metaphor-- increased his isolation and further accentuated his bureaucratic tendencies and rigid leadership style.

For both Fenichel and Reich, we argue, it was a resolute, individual psychopolitical vision that integrated theoretical

viewpoint, personal style, professional life, and political activism. Operating in a less contentious profession or in a more stable political era, each man would have found less support for their tendency to subordinate all spheres of life to the political-economic. But as bright, ambitious junior psychoanalysts of Jewish background in Europe between the wars, Reich and Fenichel found ample validation for their psychopolitical analogizing and unconscious paranoid fantasies. Instead of benefitting from the corrective feedback that comes from collaborative political action, they found themselves isolated as intellectuals in a socialist-communist movement in decline. In the end, that isolation helps explain how the self-reinforcing function of their individual visions came to dominate. After almost twenty years of competition, it was their divergent visions that led Reich and Fenichel down two different paths, equally heroic and equally psychopolitical.

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Tensions between Naturwissenschaftlich and
Geistwissenschaftlich perspectives on systems ecology:
history and political implications

by Paul Hoch (Nottingham) and Andrew Jamison (Lund)

Abstract: We consider the appropriation of ecology over the past century by a variety of natural sciences and engineering specialties - eventually as a mainly technicist systems ecology - and also the more humanistic alternatives.

As Eyerman and Jamison (1991) have noted, the concerns of social movements have often been translated into wider (including intellectual) problems, whose proposed solutions often led to new interdisciplinary syntheses. However, it has usually been unclear whether these would be shaped by a top-down administrative framework, or by a bottom-up response to the programmes of the social movements. New research funds have often precipitated a scramble between existing disciplines to appropriate the area in question to their previous concerns. Hohlfield (1982) has analysed the process for cancer research; Schaefer (1984) for agriculture; and Bud (1991) for recent 'bio-technology.' Heims in The Cybernetics Group (1991) records how, in the years after the Second World War, the multi-disciplinary Macy cybernetics conferences, organised by the MIT mathematician Norbert Wiener, polarised between those from the physico-technical sciences (like John von Neumann and the mathematical biologist G. Evelyn Hutchinson), and those with socio-psychological backgrounds (like Margaret Mead and Gregory Bateson). While the former group mostly advocated a top-down 'command and control' orientation (close to the thinking of the American military in the early Cold War), Bateson and his colleagues advocated a socio-cultural approach to better articulate social needs from the bottom-up. Heims also notes the Macy foundation was later shown to be a conduit for the Central Intelligence Agency - though even then the close involvement of von Neumann with the emerging military-industrial complex was anything but secret.

For centuries a very similar contest has been shaping up over the appropriation of nature by various disciplines - as 'hard'/technicist and 'soft'/social approaches have competed for space and resources. Indeed, as we show below, the new 'systems ecology' of the post-war period was subject to a polarisation very similar to that which affected cybernetics. Of course, as Jamison (1982) has noted, national/cultural factors and particular constellations of local knowledge interests have also led to particular national styles of environmental consciousness, and to 'scientific' approaches filtered through these traditions. Even in terms of physical geography (not to mention broader cultural questions), Sweden's substantial forests, mineral resources and relative distance from continental industrial centres posed a

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different set of problems for scientists than, say, Denmark's early mercantile orientation and comparative nearness to other European centres. These in turn conditioned different attitudes toward 'nature' in the two countries, as well as different guiding scientific themata (Holton, 1973). American conditions, particularly in the aftermath of the Second World War, helped shape still other approaches - the most prominent of which was the new systems ecology.

Ecology's roots go back to notions of a 'great chain of being', a natural history tradition described by the historian Arthur Lovejoy. The problematic was refined by the systematic taxonomists of the 18th century, especially the Swede Carl von Linne (or Linnaeus), and by British natural historians. The latter traditions eventually re-congealed into what Worster (1979) has called the two roads to ecology: the one technicist/systemic, the other historical/descriptive; the first drawing on the Baconian programme for conquering nature, the other on the arcadian urge for harmony with a natural world seen as a refuge from urban/industrial pollution. In the 19th century, the latter was the perspective of English romantic poets like Wordsworth, American transcendentalists like Thoreau, and also of such romantic socialists as William Morris who wrote his News from Nowhere (1890) as a refutation of dominant social democratic predilections for central planning (as exemplified in Edward Bellamy's Looking Backward). These 'two Marxisms' - the one romantic/humanistic, the other mechanical materialist - in relation to their understandings of both nature and society continue to struggle down to the present day (Gouldner, 1980; Elzinga, 1984). Throughout the 19th century, as other notions of nature contended for intellectual space, the emphases intermingled - combined by Engels, Lamarck, Emerson, Thoreau, Whitman, Darwin, Spencer, Haeckel and others into in effect competing political programmes.

In the present century, one can distinguish two broad national styles, or streams that after 1945 combined to become the new systems ecology. The first was an inductivist/empiricist approach identified with British experimental biologists like Arthur Tansley and Hutchinson, which was gradually mathematised and combined with a new 'energy economics.' By 1935 when Tansley coined the term ecosystem, the British natural history tradition had long been superceded. The new approach entered American academia most powerfully when Hutchinson migrated to Yale in 1928. There he was an active protagonist for a quantitative and eventually cybernetic orientation toward the subject. Another more socio-culturally oriented British stream flowed to America through such 'human ecologists' as Patrick Geddes, and Gregory Bateson (who, as mentioned above, was also on opposite sides from Hutchinson in the cybernetic debate). Geddes importantly influenced American social philosophy and

urban ecology, especially through Lewis Mumford. The latter, in his 'renewal of life' book series, including Technics and Civilisation (1934) and The Culture of Cities (1938), sought to tame the emerging technicist perspectives, and relate technology to overall social needs. Bateson was influential in disseminating an ecological perspective in both anthropology and psychology (where he called for of an 'ecology of mind'). He was also an important influence on the counter-cultural and environmental movements of the 1960s and '70s.

Meanwhile in the German-language milieu, ecology developed as part of a more general holistic, organismic, anti-modernist and anti-materialist movement described by Fritz Stern (1961), which - as Bramwell (1989) has emphasised - also had important links with the emerging Nazi movement. On the political Right, Spengler's Decline of the West (1918) was particularly influential; while from the Left the Marxist philosopher Max Scheler published Man's Place in Nature (1923). The activities of Rudolf Steiner as philosopher, agriculturist and prophet were also important. It was also in such cultural soil that the Austrian Ludwig von Bertalanffy developed the general systems theory that he would later take with him to America. Bertalanffy (1971) dates the beginnings of his theory to his own writings on theoretical biology of the early '20s, and to those of the gestalt psychologist and 'psycho-physicist' Wolfgang Koehler in 1924. The following year Alfred Lotka published in America a book on mathematical biology based on earlier ideas he had carried with him from Leipzig. When rediscovered by Hutchinson's student Howard Odum in the late '40s, Lotka's mathematical approach crucially influenced the then-emerging systems ecology. In particular, Lotka's reduction of nature to energy flows became a central ingredient in Odum's ecological modelling (based on a mixture of cybernetic jargon and computer simulations).

In the 1930s Soviet 'central planning' also had a significant impact on various disciplines. In America it importantly influenced the physiologist Walter Cannon, whose colleague L. J. Henderson helped Talcott Parsons (1972) to develop his semi-biologicistic, homeostatic (structural/functionalist) model for The Social System (1951) - which became the central paradigm in mid-20th century sociology. Soviet biology was given a systemic orientation by V. I. Vernadsky, whose book The Biosphere (1926) provided a basic paradigm for an emerging Soviet ecology. Thus, the intellectual sources of systems ecology were both diverse and not necessarily in a single political direction. If the idea of a natural system originated in the British natural history tradition, the more general holistic aspiration permeated German Naturphilosophie, British romanticism and American transcendentalism. Much of the experimental methodology and overall political orientations came from British and Soviet biology. Nevertheless,, it was in

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the United States that the various strands became integrated in a new scientific praxis reflecting the social context of the early Cold War period. This was a time when the pre-war 'little science' had become irrevocably transformed by war-time needs into the 'big science' of large laboratories and computers of the post-war period. Partly as a result of this organisational transformation, business and management metaphors became increasingly salient. Nature was reconceptualised as free enterprise: 'as a modernised economic system...a chain of factories, an assembly line...(so that) ecology too would come to emphasise the flow of goods and services - or of energy - in a kind of automated, robotised, pacified nature' (Worster, 1979). Of course even then there were humanists like Mumford, Mead and Bateson, and even a mathematician like Wiener, to challenge the new perspectives.

In the emergence of the new technocratic paradigm, two conceptual innovations were crucial. First was the gradual emergence at the Macy conferences (on 'Circular causal and feedback mechanisms in biological and social systems') of a top-down cybernetic language. Taylor (1988) notes that these conferences popularised 'the perspective that complex systems can be treated as self-regulating feedback systems.' He considers the third Macy meeting, in 1946 - at which Hutchinson offered a paper on the possibilities of the reduction of biological events to energetic transfers - as the starting point for the new systems ecology. Another important post-war influence was the developing concern with the biological and systemic effects of atomic radiation. Kwa (1989) traces the development of the new synthesis after 1950 through two phases, both heavily conditioned by the problem of atomic waste. In the first, Eugene Odum (Howard's brother) built up an institute of radiation ecology at the University of Georgia with the help of funding from the U.S. Atomic Energy Commission. Here he further developed the ideas of Tansley and Hutchinson, and produced his own text Fundamentals of Ecology (1953) introducing the problematic to a new generation of students. Considerable effort was devoted to calculating systemic effects of radiation. In its second phase, systems ecology grew even more mathematical. Howard Odum, further elaborating ideas that Lotka had developed in the 1920s, wrote about the circuits and functions of 'biogeochemical cycles.' (He had completed his doctorate at Yale with Hutchinson on the 'biogeochemistry of strontium' - an important radioactive product of the nuclear fuel cycle.) He went on from the late '50s to conceptualise nature as a gigantic electric switchboard, which must be socially engineered and controlled (Taylor, op.cit.) Much was actually developed in collaboration with scientists like Jerry Olson, who worked at the Oak Ridge atomic energy laboratory (Kwa, op.cit.). Within another decade the Odum brothers contended their approach was suited to 'solve' the ecology crisis.

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Of course not everyone agreed. In addition, to the 'softer' approaches of counter-culturists, feminists, human ecologists and rank-and-file environmentalists, more recent contenders for political and intellectual control of the environment have been the market economists. In Britain the latter were extremely prominent during the Thatcher years, with their congenial message that the environment should be subject to the same economic constraints of supply and demand as other economic inputs: pollution should (according to this view) be 'accounted' and paid for like any other ingredient. This derives from the dominant economic view that everything can be considered an economic transaction and evaluated in terms of the balance between means and ends (accounted in financial terms). Other social thinkers have questioned the processes by which monetary value is put on human well-being, and have also pointed to the unavoidably political struggle by which environmental dis-benefits are differentially (ie. unevenly) distributed. What all these proposed inter-disciplinary syntheses have in common is a continuing struggle between a physico-technicist (and managerialist) paradigm of one sort or another, and a variety of socio-cultural approaches aiming at more harmonious relations between society and nature. While the first has had more appeal for physical scientists (and the more quantitatively oriented social scientists), the second was advocated by humanists and more qualitatively oriented social thinkers such as Mumford, Bateson, the anarchist Murray Bookshin and the Frankfurt school social philosopher Herbert Marcuse, as well as much of the environmentalist social movements. Of course, the latter have often been themselves split into a socio-cultural and a technicist wing, often embodied in different contending environmental organisations.

It is undoubtedly the job of sociologists of knowledge to clarify the interests (in both senses of the term) embodied in these various alternative interdisciplinary/political approaches. Unfortunately, a few like Mary Douglas and Stephen Yearley have heretofore contented themselves with emphasising the different interpretations of purity and danger of differing cultures, including that represented by the environmental movement itself; and in that way relativising (and seemingly dissolving away) any objective environmental problems - which become only the diverse understandings of different cultures. It would be better, we think, to clarify more carefully who and what different academic cultures and programmes addressing environmental concerns 'represent' (in the sense of being conditioned by particular disciplinary lenses and helped by particular social sponsors). Granting that knowledge is always conditioned by particular social ends (Habermas, 1971), we would then be in a better position to aim for a new knowledge/power synthesis designed to meet the needs of the widest segments of society. This would no doubt require considerable elaboration - as well as practical political

implementation through an active environmental movement, which in the course of its own political praxis will further adapt the required perspectives.

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CZECHOSLOVAKIA'S "VELVET REVOLUTION" AND PSYCHOLOGY

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In recent years , historians came to pay increasing attention to the social context of science. In this report we shall be concerned with the impact of the political climate on psychology. Ideally, the time perspective should include the period of totalitarian, Soviet-controlled Communist domination (1948-1989), as background; the years since the "Velvet Revolution", initiated on 17 November 1989; and the visible future, darkened by the clouds of uncertainty about the very nature of the Czech and Slovak Federative Republic. The report should consider the recent (since 1989) developments in the Czech lands (Bohemia and Moravia) and in Slovakia, with reference to a whole string of topics, such as professional organizations, institutional changes, professional rehabilitation and changes of personnel, changes in the program of the university instruction and in the awarding of Ph.D. degrees, research, psychological services, and international cooperation. Due to the limitations of time, our account must be selective rather than comprehensive.

The present report supplements and broadens an early communication (Brožek & Hoskovec, 1990) which stressed two points: 1) The need to correct the ideological deformations, and 2) Correction of injustices done to individuals and the replacement of the hardline "old structures", brought to

power by the Party-controlled system of appointments (the "nomenklatura").

We shall illustrate. Under the Communist rule, a number of psychological topics and approaches (such as dynamic psychology) and of the fields of social science (such as sociology) became tabu. More specifically, the late Marxist editor of the treatise Foundations of Psychology (Linhart, 1981) rejected outright not only the "soul" as the subject of psychology--a step taken by scientific psychology a long time ago--but behavior and "patterning" as well. Behaviorism and Gestalt psychology were viewed as forms of the Western "idealistic" ideology--the proscribed counterpart of Marxist "materialism"--and were, consequently, ideologically unacceptable. The terms "unconscious" (or even "subconscious") do not occur in the Subject Index, since psychoanalysis and all the other "analytic" psychologies (as well as the humanistic and transpersonal psychology) constituted an ideologically forbidden territory.

While international endeavors were not altogether lacking, as documented by the Prague Psychology Conferences, contacts with Western, especially American, non-Marxist psychology and psychologists were disapproved and discouraged by the authorities. Personal contacts with Western "foreigners" had to be reported to the police, in writing, and such facts as receiving correspondence from abroad were duly registered in the personal files, kept as means for the surveillance of

the population. There were times when the citation of Western literature endangered one's position and employment.

For ideological reason, the important Czechoslovak Institute of Human Work was liquidated in 1951 (Brožek & Hoskovec, 1986, p.123). In 1972 Charles University's Research Institute of Psychology was abolished. Interestingly, in Slovakia the Psychological Institute of the Comenius University, in Bratislava, continued to flourish (Anonymous, 1988).

Ideological and power considerations were at the bottom of severe interference in the scientific careers of psychologists, young and not so young. Dr. Sonya Hermochová completed in 1965 her second (advanced) dissertation, dealing with interests of university students. The dissertation was submitted in pursuit of the scientific degree of C.Sc., Candidate of Science. For political reasons she was denied the opportunity to defend the thesis and to obtain the advanced degree (together with the professional "privileges pertaining thereto"). Prof. V.Tardy, director of the Psychological Institute of the Czechoslovak Academy of Sciences, became overnight a "non-person", having signed the proclamation "Two Thousand Words."

Checkered was also the history of the Psychological Laboratory of the Czechoslovak Academy of Sciences, located in Brno, the capital of Moravia. Initially, it was established as a center of research on personality, its development and

and structure, with emphasis on creativity and value orientation. In time, in the spirit of the Cold War, its task was reformulated as "critical [read: Marxist] analysis of the bourgeois theories of personality". On 1 July 1983 the laboratory ceased to exist as an independent organization and was incorporated into the newly createdd Institute for Research on Social Consciousness and Scientific Atheism, more concerned with propaganda than with solid scientific work.

The "Velvet Revolution" brought about many positive changes. At the Phiosophical Faculty of Charles University, the Psychology Research Institute was reestablished in September 1990 under the direction of V. Břicháček. When he resigned, for health reasons in 1991, his position was assumed by M. Břicháček.

Sonya Hermochová was able to defend her thesis, obtain the advanced academic degree, joined the staff of the Department of Psychology, and eventually was elected to chair the department.

Karel Balcar, associated with the Psychology Section ("cabinet") of the Institute for Continuing Education of Physicians and Pharmacists, author of Introduction to the Study of Personality (1983), had been severely criticized in 1985 for having utilized " [Western] positivist and personalist sources, without a critical [read: Marxist] reevaluation of their theoretical and methodological [read:

philosophical) foundations". Due to the courage of his superiors, Balcar did not lose his job. In the Fall of 1991 he received a university appointment at Charles University and will serve as head of the department of General Psychology, to be established in 1992/93.

Changes in the general socio-cultural climate are clearly documented in psychological publications. The changes are reflected both in the journals existing before 1989 as well as in an impressive number of new journals. Thus in the journal ČESKOSLOVENSKÁ PSYCHOLOGIE, now in its 36th year, following a profound change in the membership of the Editorial Council in 1990, articles are appearing that are written from points of view (such as humanistic and transpersonal psychology) that were unacceptable in the 1970s and 1980s.

In the Czech lands (Bohemia and Moravia) at least six new journals were established.

1) PSYCHOANALYTICKÝ SBORNÍK (Psychoanalytical Review) , the organ of the Czech Psychoanalytical Society, is being published by the Psychoanalytical Publishing House (Vinohradská 37, 120 00 Praha). The most frequent contributors are J.Kocourek, V.Mikota, and M.Šebek. Classical psychoanalytical writings are appearing in German (S.Freud, born in Northern Moravia), in English (O.Fenichel), and in Czech (B.Dosužkov). Among the newer authors ,the most frequently translated is H.Kohut.

2) BOLLINGENSKÁ VĚŽ (The Bollingen Tower) is a journal

for analytic psychology, edited by the Club of the Friends of C.G.Jung (P.O.Box 82, 611 82 Brno). Among the authors of original contributions are psychologists (such as M.Nakonečný and M.Frýba), the psychiatrist V.Borecký, even a sinologist (O.Král). Attention is given not only to the writings of C.G.Jung but also his Moravian student, H. Široký. Warmly welcome were the essayist studies in Jungian analytical psychology contained in the volume by R. Stary, published in Czech (" Problems with Depth Psychology"; Prostor:Praha, 1990).

3) KONFRONTACE (Confrontation) , a journal devoted to the psychotherapy of marital problems, is a publication of the Center for partner, personal and family relations in Hradec Králové (Dvořákova 1150, 500 02 Hradec Králové). Its dominant orientation is Adlerian. The articles are mostly brief. In addition to the works of A.Adler, the translations include also excerpts from such authors as J.L. Moreno and Frances G.Wickes.

4) SETKÁNÍ (Encounter), a journal for the sciences of man, is published by the The Association Focus in the framework of the activities of the Society for psychotherapy and family therapy in Brno (Arne Nováka, 1, 660 00 Brno). It provides information on a variety of psychotherapeutic approaches. Both Czech (e.g.,P.Příhoda,J.Šiklová, Z. Matějček) and Moravian (e.g., S.Kratochvíl, I.Plaňava, V. Smékal, and J. Švancara) authors contribute to it.

5) VV , nezávislá revue pro výchovu a vzdělání (An Independent Review for Education and Culture) is published on behalf of the Ministry of Education, Youth, and Physical Culture by the publishing house SPN (Ostrovní 30, 113 01, Praha). It places emphasis on humanistic psychology and is focused on counseling. The journal published a translation of C.Rogers' "Manifesto of Humanist Psychology". V.Mertin is one the preeminent Czech authors.

6) GEMMA , the house organ of the Society for a Harmonious Development and Man's Spiritual Dimensions (Truhlářská 11, 110 00 Praha), is oriented toward transpersonal psychology. Substantial attention is beeing given to the Czech-American psychiatrist, St.Grof. P.Tervinová is one of the principal propagators of transpersonal psychology in Czechoslovakia.

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Summary

In 1911 Freud gave an interpretation of Leonardo da Vinci, basing himself on a childhood memory of Leonardo about a vulture. Freud based his interpretation mainly on the mythological properties of the vulture. In 1923 it was shown that the childhood memory Freud quoted, contained a major mistake: it was not about a vulture, but about a kite.

For decades this mistake has not been mentioned in the psychoanalytic literature. In the fifties the prominent psychoanalysts Ernest Jones and James Strachey explained that Freud had been misled by a Russian novel and especially by the wrong translation of the childhood memory by a German Leonardo scholar, a certain Marie Herzfeld.

However, contrary to what is mentioned in all the literature, Herzfeld actually did give the correct translation. It can even be proven that Freud must have read this correct translation before he himself quoted the wrong translation which he needed for his interpretation. It also can be proven that both Jones and Strachey knew perfectly well that Herzfeld had translated the childhood memory correctly: they deliberately changed Freud's own mistake into Freud being misled by somebody else.

Wassenaar, April 1992

Han Israëls

FREUD, LEONARDO, AND THE VULTURE

A Footnote to a Famous Detail in the History of Psychoanalysis

Han Israels

NIAS, Wassenaar, april 1992

In 1911 Freud wrote a booklet in which he explained the most important character traits of Leonardo da Vinci, basing it on an early childhood memory of the great man. Leonardo once wrote how he remembered that, when he was lying in his cradle, he had been visited by a vulture. Through a complicated interpretation, mainly based on the mythological meaning of the vulture, Freud was able to explain why Leonardo had become a homosexual, why he had often neglected his works of art and why he had become such a brilliant scientist.

Freud's interpretation, based solely on this single childhood memory of a vulture, became even more spectacular when it was found out that the memory Freud quoted, contained a major mistake. Leonardo's childhood note is not about a vulture, but about a "nibbio", which means "kite". Freud's explanation would have been impossible with a kite: this bird does not have the mythological properties which Freud used for his interpretation.

Freud's mistake was exposed by Eric Maclagan in 1923 in the prominent art-historical journal The Burlington Magazine. It

was more than thirty years before this mistake was acknowledged in the psychoanalytic literature. In the fifties, Ernest Jones in his well-known Freud biography, and James Strachey in the Standard Edition of Freud's collected works mentioned two sources of Freud's mistake. Freud had been misled by the German translation of a Russian novel on Leonardo. In the Russian novel the childhood memory had been translated correctly; however, the Russian word for "kite" also means "vulture"; the German translator had chosen the wrong alternative, and so Freud's German copy of this Russian novel mentioned a vulture. According to Jones and Strachey, another source of Freud's mistake was a scholarly book on Leonardo written by a certain Marie Herzfeld, and according to them, Freud had quoted the wrong translation of the childhood memory from Herzfeld's book. These remarks of Jones and Strachey have been repeated by later authors. They have never been challenged.

It is here that I want to offer some new details. It is not true, as Jones and Strachey say, that in her book on Leonardo Herzfeld gives the wrong translation of the childhood phantasy; she gives the correct translation with a "Hühnergeier" (kite). Jones and Strachey did not just make a simple mistake here. Quoting from unpublished correspondence between Jones and Strachey, I will show that they both knew perfectly well that Herzfeld had given the correct translation. They must have known that it was not true when

they both wrote that Freud made his mistake because he used Herzfeld's wrong translation. By doing this, they gave the impression that it was not Freud who had made a mistake, but somebody else by whom Freud had been misled.

It can even be proven that Freud must have read the correct translation of the bird in Herzfeld's book. My lecture will mainly be devoted to giving two proofs for this. The relevant passage in Freud's personal copy of Herzfeld's book is marked in his own handwriting. Another proof is more complicated and makes use of a lecture Freud gave on Leonardo in 1910, a year before his booklet was published.

The relevance of this new information should not be overestimated. Although Freud must have known the correct name of the bird in Leonardo's childhood memory and although he must have consciously changed it into the name of the kind of bird which he needed for his interpretation, this does not mean that he consciously faked his data. He must have thought that a "Hühnergeier" (kite) is just some kind of a "Geier" (vulture). It is no more than one of the many indications of a certain style of research, which should be characterized as careless and sloppy.

V. Kurt Eimer
selected material

ABSTRACT

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Governing the migrant

Dutch psychology and migration 1945 - 1980

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In this paper the dominant trend in the psychological analyses of immigration and emigration is discussed. It is shown that the studies paint a particular picture of both immigrants and emigrants: The migrant is pictured as a person who migrates mainly because of personal, psychological reasons. The question is addressed how this almost exclusive focus on psychological causes is to be understood. It is argued that this psychological reductionism fits in nicely with the function psychology fulfils in a modern, Western society. It reduces a complex societal phenomenon to a psychological one, which contributes to a process of governing the migrants.

After the Second World War, the Netherlands have evolved quickly into a multicultural society. Mass immigration started by the end of the forties with the refugees from the Dutch Indies. Many would follow. Some had to flee from their home country, others hoped to find better living conditions in the Netherlands. It took Dutch academic psychology three decades to start systematic research into the psychological aspects of a multicultural society (Jansz, 1988). This lagging behind of Dutch psychology contrasts sharply with its American counterpart. From the 1920's on, research into the consequences of mass immigration was one of the major topics in American psychology and thus contributed to the construction of the discipline (Samelson, 1978).

The fact that Dutch academic psychology neglected immigration as a field of study, does not imply that we are without psychological analyses of immigration into the Netherlands. A considerable amount of psychological work has been done by scholars from other disciplines. Under the banner of social psychology, a psychological perspective was adopted by a number of sociologists who studied immigration.

Emigration from the Netherlands led to well planned, systematic research into the psychological causes and consequences of leaving the country. Dutch government pursued an active emigration policy in the fifties and early sixties. Population statistics were interpreted as showing that the country would be overpopulated in due time. As a response to this perspective, people were stimulated to leave the country. Thousands of people, individuals as well as families left for countries like Australia, Canada, and the United States. The psychological research into emigration was done as part of the government policies. The government founded a research committee of sociologists and psychologists to do the job. The psychologists dedicated their work to topics like motives for emigration and the personality of the emigrant.

In this paper, the focus is on the migrating individuals as objects of psychological theory and research. I will not go into a detailed discussion of the differences between emigration and immigration. Both immigrants and emigrants are assumed to be members of the category 'the migrant', because they share features that are derived from the process of migration. Two common features of immigration and emigration are discussed here. The first is the fact that external factors force

immigrants as well as emigrants to leave their home country, more often than not. But even in the case of a departure that resulted from one's own free choice, immigrants and emigrants have to bid farewell to their relatives, and friends. A second common feature is that immigrants as well as emigrants have to come to terms with the new situation once they have settled in their new country of residence. The process of assimilation to a new cultural context does not limit itself to practical concerns, its psychological consequences are far reaching too.

X A review of the psychological analyses of migration that were published by psychologists and sociologists between 1945 and 1980 shows a striking fact. In the theoretical essays as well as in the empirical studies, 'the migrant' is constructed as a specific kind of person. Instead of picturing migrants as ordinary citizens who leave their home country for economical or political reasons, the dominant picture is that their migration has been mainly caused by psychological factors. In the majority of the analyses, it is for example stated that migrants leave because they were badly rooted in social networks. Another recurring theme is that many migrated because they did not feel at ease in the culture of their home country. A small, but substantial number is said to be motivated by disturbances in their personalities.

The question is addressed how this almost exclusive focus on psychological factors can be understood. Is it to be interpreted as the inevitable consequence of taking a psychological perspective? In other words, when scientists make a psychological analysis of migration will they invariably find the cause of migration at the psychological level? It will be shown that this is not necessarily the case: there are psychological analyses of migration that do not result in psychological reductionism.

△ In my own answer to the question of psychological reductionism, I will argue that the construction of the migrant as a person who is mainly motivated by psychological factors fits in nicely with the function psychology fulfils in modern, Western societies. Psychology, on the one hand, individualizes complex societal phenomena, while it provides, on the other hand, professional support (or care) for the individuals under discussion. In this case, migration is individualized by situating its cause in the individual, psychological make-up of the migrating person.

This psychological reductionism constitutes an important contribution to 'governing' (Rose, 1990) the migrants. Immigrants as well as emigrants are potentially disturbing elements in Dutch society because their migration may draw attention to structural shortcomings of the Netherlands. The arrival of immigrants, e.g., puts the issue of cultural assimilation on the agenda, which may necessitate a revision of well-established Dutch customs. Emigration inevitably results in a debate about who leaves and who stays. Dutch government might have asked itself whether everybody is free to leave or whether specific groups, or professions, must be stimulated to go. These complex questions are simplified by narrowing the focus to the psychological factors. More often than not, it results in depoliticizing the issue of migration. With respect to immigration, psychology tells the public that it depends on the personality of the individual immigrant whether he or she assimilates smoothly. With respect to emigration, the policy makers do not have to account for the fact that a particular group left. Psychological research provides the explanation that each individual migrant had his or her personal reasons.

In short, the individual migrants are held personally responsible for their decision to migrate and for their success after migration, which is in perfect accordance with the individualistic principles of Western society. In other words, if something goes wrong during or after migration, the only one to be blamed is the individual migrant. The construction of the migrant as being mainly motivated by personal factors has been productive for Dutch psychology as a discipline too. It identifies implicitly that psychologists are needed when problems arise as a result of migration. The individual migrant is not helped much by changes in government policies, but will profit immediately from therapeutic intervention in his personal process of adjustment.

TOTALITY AGAINST PSYCHOLOGY IN CZECHO-SLOVAKIA:

The power's fear of psychology and psychologists' coping with power

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Summary: The greatest part of modern development of Czechoslovak psychology took place under peripetic conditions. The fifties, following the overthrow of the government by the communists in February 1948 and the seventies, after the military occupation in August 1968 were especially critical. In the beginning of the fifties nearly all gains of the previous development of psychology were liquidated; this destructive process had a definite aim, was phased as well as brutal, and it caused a diaspora of psychologists. The organizational, personal and material liquidation of Psychotechnic Institute in Bratislava is the prototypical illustration of this pogrom. In contrast to that, the seventies, declared normalization years, represented the development inhibition of psychology with the gradual effort to ideologize it. Research activities, professional - practical jobs, university education of psychologists, macro-structure of individual psychologists were affected to the greatest degree by this process. In this context the phenomenon of the totalitarian schizoidity is described. However, some ways of coping with the totalitarian scientific policy were found, illustrated by several examples in Slovakia. The manner in which the psychology survived in the totalitarian system in Czecho-Slovakia indicates a more complete understanding of the development of psychology in the entire world.

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The onset of the communist political system in Czecho-Slovakia in 1948 found psychology already established at the universities, partially in professional practice and with certain research projects, etc. However, the system very quickly grew to a totalitarian power resulting in harsh consequences for science, social sciences in particular. Psychology, denounced as a bourgeoisie pseudoscience, became one of the targets of an extensive fight. In reality, however, the conflict between the totalitarian power and psychology had deeper roots. Since psychological knowledge gives the reasons for the justification of spiritual, moral, creative and other potentials of man, the totalitarian system, with its normative stereotyping of people's needs (in this case the needs of the worker class), had to necessarily suppress it. And if psychology is, in addition, functional, the system is afraid of it, it feels endangered. On the basis of documents, completed by testimonies of the witness of certain events, the years 1950-52 can justifiably be called the liquidation years for psychology in Czecho-Slovakia. This destruction process had a certain aim, it took place in phases, often brutal ones. In addition to general characteristics, this can be illustrated by a prototype example - the organizational,

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personnel as well as literally material abolition of the Psychotechnical Institute in Bratislava in 1951. It went as far as artificially criminalizing psychologists by falsely accusing them of having opinions ideologically incompatible with marxism. That is why it is understandable, ex post of course, that the members of the then small psychological community did not have the space to rebel against this pogrom. A diaspora occurred and only a few were able to find work at medical hospitals and professional schools.

In the meantime, the "building" of new psychology began, importing the Soviet model of science. According to it, every scientific field is a subsystem of dialectic and historic materialism which is particularly true of social sciences, where psychology was included. In spite of this fact, since approximately the middle fifties, new psychology in Czecho-Slovakia began to express itself in research form in biological context, as Pavlovian psychology, since at that time in the Soviet union the study of higher nervous activity dominated in several areas of science.

In the sixties, particularly in Slovakia, we can see an acceleration in the development of psychology; a sort of small psychological boom. The post Stalinist thaw made the political course of "socialism with a human face" possible and even though psychology was not addressed directly, initiatives were not suppressed as readily. The development of psychology with the

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creation of many new fields (psychology of personality, psychodiagnostics, social psychology) expanded in the area of manpower, organization and publication. Psychological events included discussions about issues previously taboo. Immediately following, however, came the military occupation in August 1968 and with it the tightening of the totalitarian power, a period of the so called normalization. In Czecho-Slovak psychology these peripetic years were manifested by ever deepening inhibition of development, with making ideology ever more important in almost all psychological work. The normalization years affected three areas of psychology in a particularly harsh way: research specificity ("socialist social consciousness" under the directorship of philosophers was given priority), the competence of the professional and practical psychological activity (psychological educational clinics were made into institutions directed by pedagogical clerks) and the level of university education of psychologists (a four year study was introduced, later with the possibility to get a Ph.D. degree without any rigorous work). However, normalization years interfered mostly with the macrostructure of psychology in Czecho-Slovakia (abolition of psychology centers, psychologists' posts, psychological services) and, of course, with professional development of many psychologists (political prohibition of obtaining scientific degrees, habilitations, study stays abroad, etc.). Some psychologists chose to solve this conflict between power and professional activity by emigrating, others - often the ones affected the most - became part of the dissent. But the

majority of psychologists in Czecho-Slovakia - judged by publications, presentations at psychological events, popularization of knowledge, public activity, etc. - seemed to have adjusted to the harshened totalitarian system; but that is only the half-truth. In reality, surviving the totalitarian power made it possible for a specific phenomenon to occur. It is a case of schizoid personality trait which occurs massively, under given environmental conditions and under certain circumstances; it can be considered to be schizoidity from totality or, in extreme cases, paradoxically normal schizophrenia. One of the symptoms was, for example, referring to the resolutions of the communist party congresses, quoting marxism-leninism classicists, often leaning on works of Soviet authors, etc. However, the transformation of the external normalization censorship to the internal unconscious autocensorship, at least in public, is an exemplary case of totalitarian schizoidity. Of course this "illness" did not affect only psychologists.

As far as Slovakia is concerned, in the normalization years there is evidence of aimed strategy of coping with totalitarian policy in science (unlike the anxious resignation at the beginning of the fifties). Here are a few examples from personal experience of the author.

In spite of the pressure to integrate all basic psychological research in Czecho-Slovakia into the stream of studying the already mentioned diffusion problem of socialist consciousness, the Institute of Experimental Psychology of the Slovak Academy of

Sciences was able, for full 30 years, to develop its own methodologically modified psychology of cognitive functions; all this under the sponsorship of medical sciences which were less controlled by the ideological power. Beginning in 1970, the institute succeeded in making its journal *Studia Psychologica* an international one; gradually, the majority of the articles were published in English (evidently almost inaccessible to the censors). At the initiative of the institute a tradition of international conferences developed in Czecho-Slovakia, profiled as meetings of psychologists from the Danubian countries, therefore, being more than merely professional exchange of ideas between the so called eastern and western psychologists; so far there have been 9 of them, one in Austria, former West Germany and even in the former Soviet Union.

The study of this period, which began as a special project in Slovakia in 1992, will certainly bring to surface other forms of various escapes as well as active coping with the problems caused by totality.

At the same time, the totalitarian power, practically until its end, continued to express its might, where it dared. The "maneuvers" of the Slovak Ministry of Education with the fusion of the Research Institute of Child Psychology and Pathopsychology with the Pedagogical Research Institute in Bratislava in the second half of the eighties is a prototype example, similar to the abolition of the Psychotechnical Institute in Bratislava in

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1951. The cunning but false reason for this was to bring pedagogy closer to psychology. However, the real reason was to control psychology, which so irritated the official pedagogy (on good terms with the totalitarian power), by its constant pointing to the lacks and weaknesses of the educational system in Czecho-Slovakia. And the result of this confrontation, during which there were five times as many contributions in the massmedia against this intention (long kept in secret) as for it? Fusion. Thank god it only lasted a short time, the year was 1989.

As is evident from the above, parts of the development of modern psychology in Czecho-Slovakia are marked by peripetias. To get to know them in detail is most certainly very important, in order to overcome the existing inhibitions in the future development of psychology. We dare to think, however, that the conclusions can be a contribution to the better understanding not only of psychology in Czecho-Slovakia but also of the complete picture of world psychology.

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On the history of naturalism in perception: Heider, Gibson and Marr

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ABSTRACT

This paper discusses Heider's, Gibson's and Marr's work as instances of naturalist psychology, concentrating on organism-environment interactions, rather than on sensory and mental processes in themselves. They reject the sensation-based theory of perception, which implies that only the physical impact of the stimulus at the sensory periphery should be taken into account in theories of perception, and that the ecological environment the perceiving organism lives in can be no part of psychology. All three try to account for the fact that the organism is tuned in to its environment, and that assumptions about the ecological environment are built in in perceptual systems.

On the history of naturalism in perception: Heider, Gibson and

Marr

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Introduction: naturalism in perception.

This paper outlines the historical continuity between Fritz Heider's theorising on perception and Gibson's ecological psychology, and the thematic continuity between Gibson's work and Marr's computational theory of vision. The common ground between these authors is what can be called their naturalist orientation. Fodor (1980) divided theories in psychology into naturalist and rationalist: naturalist theories concentrate on organism-environment interactions, rationalist theories consider mental processes without regard for the environment the organism lives in.

Solipsism in perception?

That the psychology of perception must be naturalistic is not as obvious as it seems. The crucial question is whether the functions of the visual system can be understood without referring to the environment in which it operates.

Stillings (1987) mentions classical psychophysics as an instance of methodological solipsism, since it holds that the physical description of the stimulus is limited to the properties of the light that can be defined without reference to the world of visible objects, and thus it eschews theorising about the world. Also, Müller's doctrine of specific nerve energy (see Pastore, 1971) holds that only the

causal impact of the external world on the nervous system is the adequate level of description of the input for perception; consequently, it implies that psychology must "end at the skin".

The present paper attempts to present some material supporting the thesis that Fodor's solipsist standards are too restrictive (Kitcher, 1985), by reviewing three typically naturalist theories: Heider, Gibson, and Marr, and the commonalities between them.

Fritz Heider

Fritz Heider (1896-1988) was born in Austria and studied at the Universities of Graz and Berlin. He worked in the early years of his career at the periphery of the Gestalt movement. In 1930 Koffka invited him to Smith College where the former was to set up a research group; in 1947 he moved to the University of Kansas and made important contributions to social psychology that will however not concern us here (Heider, 1959).

Although neither of Gibson's two biographers (Lombardo, 1987, Reed, 1988) is very explicit about the connection between the development of Gibson's system and Heider's work, Heider's autobiography (Heider, 1983) indicates that they must have had much contact between 1930 and the war. He counts Gibson among his friends; both attended Koffka's seminars at Smith (Gibson, 1967/1982, Heider, 1983). Hence, it is probably no coincidence that some of the core concepts of Gibson's system can be found in two papers Heider published in German in 1926 and 1930.

In one of these Heider discusses what Brunswik later called the "ecological phase" (Brunswik, 1952; Postman and Tolman, 1959): the relation between the proximal stimulus, the light that reaches the eye, and the distal stimulus, the object that reflects the light. According to Heider, the structure of the medium (the light) can constitute a "spurious unit", which has been imposed extrinsically on the light, and thus points to its source in the distal (real world) object. Therefore we say that we perceive objects, not that we see light. The organism "gathers" these events, coded in the structure and covariances of the medium, and the perceptual apparatus transforms them back into a perceived object. The order in the medium can only be understood from its relation with the

corresponding thing in the environment, therefore the distal stimulus, not sensations or proximal stimuli, should be part of theories of perception. This distinction runs counter to the then current model of a causal unidirectional relation between physical stimulus and elementary sensation (cf. Meyering, 1989; Pastore, 1971). More specifically, it undercuts Müllers concept of specific stimulus energy holding that "The central and fundamental principle of the doctrine is that we are directly aware, not of objects, but of our nerves themselves; that is to say, the nerves are intermediates between perceived objects and thus impose their own characteristics upon the mind" (Boring, 1950. p. 82)

These two key elements of Heider's system, the structure of the light that specifies its source in the environment, and emphasis on the role that structure of the environment should play in a theory of perception, return in Brunswik's and Gibson's work. Also, Heider (1929/1959), like Gibson, emphasizes that perception is to be approached from a functional and adaptive point of view; perceptual systems are tuned in to the environment, and perception should be studied as an achievement of an organism in its environment. As Heider put it: "The relation between stimulus and perceptual image corresponds in a mirror fashion to the relation between core event and offshoot (i.e., between distal and proximal - H.L.). This correspondence is an instance of the adaptive function of the perceptual apparatus." (Heider, 1959, p. 37), and "The performance of the perceptual system is to a great extent determined by the structure of the environment" (p. 46). Thus, the notion of a perceptual system as an adaptive function, that was to make up the core of Gibson's *The senses as perceptual systems*, (1966) can be found in elementary form in Heider's work.

The distal-proximal distinction was adopted by Brunswik, who acknowledged that Heider's ideas served as the basis for much of his own theory (Heider, 1983, p. 88). This call for ecological validity was accepted by Gibson. However, whereas Brunswik supposed a probabilistic relation between proximal and distal stimulus and had to assume an (essentially Helmholtzian) inferential process reconstructing distal from proximal. Gibson assumed a direct specification the environment by the ambient light.

James J. Gibson

Gibson criticism of Heider is essentially that: "he believed that light could only *mediate*, not *convey* information he concluded that some creative process in the organism was necessary to explain perception. He anticipated a good part of the modern theory of information however, and saw very early that the concept of stimulation was inseparable from the concept of environment."

Gibson can be considered the naturalist psychologist par excellence; although his somewhat idiosyncratic language sometimes obscures the origins of his concepts, his roots in American functionalism can easily be seen (E.J. Gibson, 1982; Noble, 1981).

Gibson's core concept of direct perception implies that information is a property of the world rather than an internal cognitive construction, or put differently, that knowledge of the world alone suffices to understand what is perceived (Banks and Kraljick, 1991).

Gibson's early research (Gibson, 1950) was on what he called perceptual psychophysics. Whereas sensory psychophysics studies sensation as a function of stimulus intensity, perceptual psychophysics studies perception as a function of the structure of the retinal pattern. The most impressive result is that information for perceiving depth is a function of retinal gradients. Thus perception is a function of stimulation (Gibson, 1959); the information is in the structure of the light, and can be directly picked up without internal cognitive elaboration.

Gibson's book *The senses considered as perceptual systems* (1966), introduces the notion of perceptual systems (as noted, the term was anticipated by Heider), which are functionally defined, not anatomically: they are adaptive, information gathering systems, which cannot be identified with sensory organs. Thus, perception is an achievement, it is the hunting for information, not the having of sensations.

Gibson's third book (1979) can be interpreted as an attempt to provide a description of the input for perception at the organism-relative (ecological) level, (in terms of media, surfaces, etc.) in contrast with the level of description of classical physics (in terms of wave lengths, sound frequencies, etc.), dominant in classical psychophysics (Neisser, 1977; Wilcox and Edwards, 1982).

To sum up, Gibson, like Heider argued that the distal stimulus, the ecological environment is the relevant variable in perception, and both emphasised that the proper description of the input of perception is the world at the level of (ecological) events, rather than at the level of classical newtonian physics.

David Marr

Several authors have drawn attention to the philosophical implications of Marr's (1982) theory of vision; it is generally acknowledged that it constitutes a naturalist approach (Stillings, 1987, Kitcher, 1988, Burge, 1987) and it is contended that as such it refutes Fodor's solipsism (Burge, 1986; but see Fodor, 1987). For our purpose, the most interesting part of Marr's work is his distinction between three levels of analysis in vision: the computational level, referring to the extraction of information about objects from the structure of the light, secondly the algorithmic level referring to the computational process that executes that extraction, and finally the neurophysiological implementation. The computational level (a bit of a misnomer) entails assumptions about the structure of the world (e.g., about the rigidity of solid objects). The algorithm is underdetermined by the function: many algorithms can subserve the same function. According to Marr, the upper level of functional specification of what the system does, cannot be reduced to the specification of the algorithms and neurophysiological structures.

That assumptions about the environment, which are built in into the system, are the extra factor that disambiguate algorithmic specification (Stillings, 1987), has in a sense been Heider's and Gibson's point all along.

Marr (1982, p. 29) summarises the relation between Gibson's direct perception and the information processing approach as follows: " .. perhaps the nearest anyone came to the level of computational theory was Gibson (1966). However, although some aspects of his thinking were along the right lines, he did not properly understand what information processing was, which led him to seriously underestimate the complexity of information-processing problems involved in vision and the conse-

quent subtlety that is necessary in approaching them. Gibson's important contribution was to take the debate away from the philosophical considerations of sense-data and the affective qualities of sensation (...) Gibson correctly regarded the problem of perception as that of recovering from sensory information "valid" properties of the external world. His problem was that he had a much oversimplified view of how this should be done."

This corresponds to Heider's and Brunswik's position: apart from the ecological phase (the relation between distal and proximal stimulus), there is also an organismic phase (the extraction of the information from the proximal stimulus), or in Pribram's (1977, 1982) phrasing, an "ecology in the head", which is missing in Gibson's ecological psychology.

Conclusion

Burge (1986), Kitcher (1988), and Stillings (1987) discuss Marr's work, especially with respect to the upper functional level as a typically naturalist psychology in Fodor's (1980) sense. According to Burge (1986) and Kitcher (1988) Marr undermines Fodor's rational psychology and the principle of methodological solipsism. Marr's upper ("computational") level concerns the problem or task for an organism in its environment, and therefore implies general assumptions about the structure of the environment (Marr, 1982, p. 43; Stillings, 1987). Marr and Gibson (and of course Heider) can thus be situated in the wider tradition of functionalist or "naturalist" psychologies (see Harman, 1988), which place the relation between organism and environment at the forefront. Both imply a relational concept of mental processes (Bechtel, 1985; Looren de Jong, 1990); the content ascribed to mental representations and processes is partly determined by their reference to the distal stimulus (Burge, 1986; cf. Putnam, 1975, 1988).

What connects Heider, Gibson and Marr is that they consider perception as a task for the organism in the environment: thus they situate themselves among naturalist theories concerned with organism-environment relations, and distance themselves from the rationalist tradition.

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ABSTRACT

CONSTRUCTING THE SUBJECT:

AN EXTENSION OF DANZIGER'S ANALYSIS

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This study uses Danziger's work on the nature and evolution of investigative practices in psychology as an exemplar to examine trends in the development of research practice in South African psychology prior to World War II. It establishes remarkable similarities with Danziger's results in the patterns of change and their historical periodization. The overall explanation for these findings is to be found in the notion of "the professional project": psychologists adopted research practices which enabled them to present themselves as "scientists", while simultaneously making contributions to the debates on the social issues of the day.

1A- psychogen: ^{UK} ~~Orntst.~~

- 2.A., maar in UK/Old. orgjeid

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CONSTRUCTING THE SUBJECT:

AN EXTENSION OF DANZIGER'S ANALYSIS

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Danziger's (2) study of the nature and evolution of investigative practices in psychology during the first half of this century focused on Europe and America. We believed that something could be gained in applying Danziger's analysis to developments in psychology outside these main centres. First, his analysis is an important contribution to psychological historiography, and deserves wider application, if only to see what insights it can yield in different settings. Second, it may tell us something about the pattern of investigative practices adopted by countries outside the European-American origins of psychology. This, thirdly, may reveal the ways in which psychological ideas and methods were dispersed across the world, and how they were adapted to local conditions, if at all. Third, such an analysis would contribute to our meagre store of knowledge about the early development of the discipline in South Africa.

All empirical studies published by South African psychologists in recognized journals between 1895 and 1939 were examined, following the guidelines established by Danziger. Broadly speaking, we wished to

arrive at a characterization of psychological research practice in South Africa prior to World War II, as reflected in the 15 journals included in the search. Only 33 empirical studies were identified, the earliest of which appeared in 1925.

Table 1

List of Journals searched for empirical studies by South African psychologists

American Journal of Psychology
 Archiv fur die gesamte Psychologie
 British Journal of Psychology*
 Journal of Applied Psychology
 Journal of Educational Psychology
 Journal of Experimental Psychology
 Journal of General Psychology*
 Psychological Monographs
 Psychological Review
 Philosophische Studien
 Psychologische Forschung
 Psychotechnisch Zeitschrift*
 South African Journal of Science*
 Transactions of the Royal Society of South Africa*
 Zeitschrift fur angewandte Psychologie

The results of the present investigation showed remarkable similarities with those of Danziger, in terms of both emerging patterns and historical periodization.

- ① The first pattern to be distinguished was the progressive decline in the utilization of the Wundtian research tradition, and an increasing tendency to rely on the Galtonian model as the dominant research practice in psychology. For example, in only one of the South African studies an exchange of experimenter and subject roles occurred. Also, from 1925 to 1933 ten articles followed the practice of identifying

subjects individually (fifteen did not), and then it ceased altogether. It appeared to be displaced by studies reporting group data; in fact, from 1935 onwards only group data were reported. It would be fair to say that the Wundtian model was adopted so weakly in the first place in South Africa that one can hardly speak of it as being replaced. The third research model discussed by Danziger, the clinical experiment, was used in only one instance in the journal articles under review here. Thus evidence could be found of the presence of all three investigative practices in South African psychology prior to World War II, but the extent to which researchers utilized them differed quite substantially.

In terms of the extra-experimental identity of research subjects, Danziger found that academic psychologists featured most often as subjects in research, at least until 1910. As the Leipzig model declined and the Galtonian model grew in popularity, psychologists increasingly were supplanted by undergraduates. In the South African articles, school children were the subjects preferred by psychologists. Between 1926 and 1938 17 studies reported data obtained from school children. Academic psychologists never were the predominant source of human data in this research: only four of the 33 reports (published between 1925 and 1928) indicated that they fulfilled this role. Undergraduates too seldom acted as subjects in the period under review: only six studies reported their participation. Subjects from a non-educational background were included only in 1930, and seven such articles were published before 1939.

The manner in which the extra-experimental identity of subjects was reported gives an indication of the concerns of researchers. (For

investigators undertaking applied research, the practical applications of their research results were paramount, thus making the extra-experimental identity of their participants integral to the relevance of their findings). The large majority of South African studies identified research participants in terms of the social categories they belonged to, a reflection an early involvement with applied and practical matters. Twenty-eight articles reported the extra-experimental identity of research participants: school children, criminals, juvenile delinquents, orphans, and patients. In five reports the extra-experimental identity of subjects included reference to "Whites", "Natives", "Indians" and "Coloureds".

This conclusion is strengthened if one examines the types of group data employed in the research: "natural", "psychometric", or "experimental" groups. "Applied" studies were much more likely than "basic" experimental research to attribute their data to natural groups. If they did create artificial groups, these were more likely to be psychometrically defined groups. In our investigation only one study between 1925 and 1939 involved an experimental group, and 21 derived their results from work with natural groups. Compared to Danziger's sample of articles, South African psychologists made less use of psychometric group data (only three reports). In fact, such groups did not appear in the local literature until 1936.

In conclusion, we note again that in the early years of psychological research in this country, a mixture of all three investigative practices is present. One, the clinical experiment, had a very weak presence; another, the Wundtian model, declined rapidly from a weak base; and the third, the Galtonian model of research, soon came to

dominate. We hypothesize that this pattern would repeat itself in other parts of the world outside the European and American centres of psychology - depending, of course, on when psychology originated in a particular country. In the South African case, it appears as if psychologists were exposed to all three research models. However, they had to be aware of the trend highlighted by Danziger: that by the 1920s, the Galtonian model was beginning to dominate in America, and to a lesser extent in Germany.

Implicitly, this provides a tentative answer to the question of how these research practices spread across the world: via personal visits. This could take the form of studying in Germany, the U.K. or the U.S.A.; foreigners from a particular tradition being appointed in South Africa; or visits to important overseas centres.

Another significant pattern emerging from this analysis is that South African psychology established itself within an applied, practical framework prior to the Second World War. In this process the widespread acceptance of a Galtonian research practice was not insignificant.

Why would the Galtonian model be adopted so rapidly and completely in South Africa? The answer to this question is not much different from the answers framed by Danziger in terms the 'professional project' of psychological investigative practice. This project involved two central concerns. First, there was the need for psychology "to develop practices whose products would answer to the immediate needs of socially important markets" (2; p. 120) (prominent among these were educational institutions). Second, there was the need to emphasize

"that what psychologists practiced was indeed to be counted as a science" (ibid.). The "neo-Galtonian style" suggested itself as socially relevant in its practical application and 'scientific' in its use of statistical biometry. It allowed psychologists to formulate laws of behaviour on the basis of aggregate data, and at the same time to investigate populations and psychological characteristics which were of cultural and administrative significance. In short, psychology could claim to be a socially relevant science.

NOTES

- (1) Apart from the senior author, seven others participated in this study: Julie Binedell, Welmoet Brimmer, Pindi Mabena, Annemarie Meyer, Evan Robins, Ken Roper, and Diana van Rooyen.
- (2) Danziger, K. 1990. Constructing the subject: Historical origins of psychological research. Cambridge: Cambridge University Press.

Multifactor Studies in Early Experimental Psychology

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Abstract

Although there has been little historical coverage of experimental design in psychology or the relationship between design and analysis, standard accounts from the 1940s seemed quite clear: prior to the advent of analysis of variance, experiments in psychology were based almost invariably on single factor designs where all the variables except one were kept constant.

We examine this proposition partly from the point of view of early advocates of analysis of variance, and partly from a survey of psychology journals published between 1887 and 1940. From this latter evidence, we conjecture that at least half of the empirical studies reported during this period employed designs which simultaneously manipulate two or more factors.

We also examine the ways in which the designs and the results were represented. Finally, we explore the issue of how experimenters viewed relationships between variables, including the possibility of "interaction".

Multifactor Studies in Early Experimental Psychology

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The received historical opinion of the 1940s suggests that experimental designs which explored the effects of one independent variable (factor) at a time were the norm in empirical psychological studies. Indeed, Crutchfield & Tolman (1940) maintained with much certainty that

"Traditional principles of experimental design require that but one independent variable be studied at a time while all other conditions are held constant...

Psychological research has depended almost exclusively upon this technique, and many textbooks of psychology go so far as to describe this single-variable design as the *only* proper one for fulfilling the prerequisites of rigorous experimental procedure." (page 38, their italics).

Similar, if slightly less prescriptive, views can be found even later, when Underwood (1949, page 2) argues that the basic experimental procedure in psychology is that of the single factor design. Furthermore, Garrett & Zubin's advocacy, in 1943, of the notion of manipulating simultaneously two or more independent variables in psychological experiments carried with it the clear implication that such multifactor design structures were virtually unknown in the subject prior to about 1940 (see, for example, pages 244 and 248; also Woodworth, 1938, page 2).

The motivation for what it will be argued later is a patently under-researched position is to be found in the hidden agenda of the papers by Crutchfield & Tolman (1940), and Garrett & Zubin (1943). Specifically, both accounts were devoted to the fundamental importance of the



analysis of variance (ANOVA) to psychology and, in particular, that such a technique would free psychology from the constraints of single factor studies.

But, of course, any general admission that psychologists had been, in fact, happily using multifactor designs for many years, without benefit of sophisticated statistical methodology (or R A Fisher), might have considerably weakened the impact of analysis of variance on psychology. Thus, in order to strengthen their hand, the pro-ANOVA missionaries recast the past in a way which provided consistent support for their position, a not unfamiliar use of historical resources. It seems, therefore, that Underwood (1949) was doing little more than repeating this interpretation of the previous fifty or so years of experimental psychology (see Lovie, 1991, for more commentary and references).

However, a modest survey of a selection of mainstream psychology and education journals over the period 1887-1940 suggests that multifactor designs were commonplace. Indeed, it is not too fanciful to conjecture that at least half of all the empirically based studies which appeared in the psychological literature (pure and applied) prior to 1940 used such complex designs to structure their experiments.

What was particularly striking, coming as these studies do from an era long before statistical cookbooks, was the relative transparency of the multifactor structures. Clearly, the researchers themselves were aware of the nature of their designs in that their experimental results were invariably laid out in factorial (that is, hierarchical) form. Moreover, results would be systematically dissected along factorial lines using graphical and tabular methods. These preserved the full structure of one or more of the factors while summing or averaging across all the investigated values (levels) of the other factor (or factors).

In all the papers, the standard tactic was to discuss each factor separately. Of course, this did not prevent experimenters from attributing varying degrees of significance to the results produced by each factor depending on its centrality to the researcher's main concerns, but it

does at least mean that they were aware that each data point could be simultaneously influenced by several independent factors.

Certain of the papers indicated an awareness (albeit in a rather primitive form) of the concept of interaction in that some explicit attempt was made to explain, on the basis of the data summary devices mentioned above, how the factors worked together. This seems to suggest an informal understanding that a joint operation between the factors might lead to differential effects over combinations of levels of these factors, as well as a willingness to develop devices which ease the cognitive strain of attempting to detect this phenomenon in complex data sets.

There is evidence, in other words, of a recognition of multifactor complexity in the modern sense, balanced by an equally explicit realisation of the limits of unaided human judgment and inference, all leading to the creation of an array of statistical and graphical aids. Summary tables and graphs seem well suited to an age when the experimenter had limited computational and statistical resources, and yet appear highly familiar to today's researchers.

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RESEARCH APPRENTICESHIP AND INTERPERSONAL GATEKEEPING:

A "SOCIAL PSYCHOLOGY OF SCIENCE" ANALYSIS OF
POST-WORLD WAR II DEVELOPMENTS IN SOCIAL PSYCHOLOGY .

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ABSTRACT

Previous research on the history of (social) psychology focused upon a social psychological analysis of scientific activity emphasizing interpersonal, rather than impersonal, negotiations among scientists. These interactions involved asymmetric power relations between: a) author and journal editor; b) grant applicant and funding panel; c) undergraduate student and professor armed with textbooks, tests and grades; d) research apprentice/PhD candidate and thesis supervisor or committee; and e) research scientist seeking career stability and administrative committees (hiring, promotion, etc.); they affected the outcome of ideas proposed as additions to a discipline's cumulative knowledge base. The present paper looks at the research apprenticeship process involving undergraduate and graduate students, focussing on the inter-generational transmission of one particular methodological strategy within post-1945 social psychology-- the laboratory experimental deception experiment. After examining the constraining rhetoric of academic textbooks and manuals, a study is made of 4 groups of mentors and students in the University of Michigan's social psychology programme (1948-1952), each with differing methodological commitments. The impact of their choices is traced through 4 decades of research activity which included a disciplinary crisis.

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Previous research on the history of psychology and social psychology (Lubek, 1979; 1980; 1990a; 1990b; in press, a ; Lubek & Apfelbaum, 1987) has focused upon a social psychological analysis of scientific activity emphasizing the interpersonal, rather than impersonal, nature of science. This research examined decision-making negotiations involving asymmetric power relations which affect the fate (acceptance, rejection, modification, delay, etc.) of ideas offered for inclusion within a discipline's cumulative knowledge base. Various archival and oral history materials, and quantitative indices are used to analyze the small group interactions between a potential scientific contributor and the (s)elected representatives of a scientific community. Of the asymmetric power relations identified -- those existing between a) author and journal editor; b) grant applicant and funding panel; c) undergraduate student and professor armed with textbooks, tests and grades; d) research apprentice/PhD candidate and thesis supervisor or committee; and e) research scientist seeking career stability and administrative committees (hiring, promotion, etc.)-- most attention was focused on the first two.

The present paper looks at the research apprenticeship process involving both recruitment and training of undergraduate and graduate students (cf. Lubek, in

press, b). In particular, I focus more on the inter-generational transmission of one methodological strategy within social psychology-- the increasing use, after World War II, of the laboratory experimental deception experiment, which in part sparked a disciplinary crisis beginning in the late 1960s. I shall review the work in progress of Winston (1992) and of Stam & Lubek (1992) on the justificative rhetoric offered to researchers and their student apprentices, for the increasingly narrow definition of experimentation found both in general experimental psychology's journals and textbooks, and in the post-1945 undergraduate and graduate social psychology textbooks and manuals.

I shall also summarize work in progress by Lubek and Thoms (1992), tracing the scientific careers of the first cohort of graduates from the first interdisciplinary PhD programme in social psychology at the University of Michigan. Here, the focus is on the transfer of a supervisor's methodological orientation to the next generation of students, as the research follows the later scientific careers and productivity of mentors and the cohort of students granted their PhD's between 1948 and 1952 in this programme. Four groupings of social psychological mentors and their students are compared: a group favouring (laboratory) experimentation (e.g., Festinger); a neo-Lewinian group less committed to such experimentation (Cartwright, Zander, French, and Lippitt); the eclectic founders of the PhD programme (Katz and Newcomb), and a group of other faculty supervising social psychological research at this time (Heyns, Hutt, Blackwell, Peak).

Preliminary data indicate that the small "experimentation" group rapidly became more visible in the primary social psychology literature than the mentors and students of the other three groups. By contrasting research productivity in the

"post war expansion" period of 1948-1966 with the "crisis, self-criticism and reappraisal" period of 1967- 1991, it may be possible to see whether the initial mentoring relation involving a distinct commitment to one "investigative practice", is linked to a stable methodological strategy over several decades of research activity, even in the face of a disciplinary crisis!

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History of Psychology - an After Dinner Slide-Show

Helmut E. Lück & Rudolf Miller

You know the TAT, don't you?

Do you also know that TAT picture showing the young boy and the violin?

It's Yehudi Menuhin aged about 12.

You didn't know? I'm not surprised. He didn't know either that he served as a STIMULUS since decades.

The process of historical (re)construction is normally confined to spoken and written words. Many history books in the field of psychology and social sciences don't show any pictures. If pictures are shown, they are mostly used as *addenda*. This more or less lenient attitude toward illustrations, sketches, photographs etc. is reflected in the fact that illustrative material has been less carefully preserved and has been rarely subject of scientific interests. Our aim to provide the interested readership with an *Illustrated History of Psychology* was, therefore, a risk. The great response to our "call for papers And pictures", however, was encouraging. The book with almost 80 small chapters, written by more than 50 authors - among them many CHEIRON members - will contain several hundred pictures, a great portion of them never published before.

In an informal presentation we would like to show approx. 160 slides from various sources referring to several schools, fields and areas of psychology. Special topics will be the Wundt School, Gestalt psychology, and applied psychology. Our comments will be brief. Discussion is very welcome.

ABSTRACT:

WITTGENSTEIN ON LANGUAGE AND EXPERIENCE

The present resurgence of interest in the thought of Ludwig Wittgenstein is related to two recent developments within philosophy and the methodology of science, namely the growing concern with the role played by conceptual frameworks, models and metaphors in the mediation of our experience of the world and the postmodernist critique of the project of modernism. It is argued that the complex question of how we should understand the relation between language and experience are fundamental to these developments. Against this background the shift in Wittgenstein's understanding of the relation between language and experience is elucidated in such a way that its broader relevance for present discussions within philosophy and the methodology of science becomes apparent.

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WITTGENSTEIN ON LANGUAGE AND EXPERIENCE

1. Introduction

As our century draws to a close, it is becoming more and more evident that the philosophical legacy of Ludwig Wittgenstein had a profound and irreversible impact, and will continue to exert a radical influence on present and future developments in philosophy and the methodology of science (Grayling, 1991:61-64). Within the analytical tradition of philosophy, by far the most dominant tradition in the English speaking world, there is hardly a current philosophical inquiry of any importance which is not conceptually and methodologically indebted to the originality and penetrating analysis of Wittgenstein's thought (Von Wright, 1982:15). Although this may not always have been the case within continental philosophy (Kenny, 1984) - being largely dominated in our century by the three movements of phenomenology/ existentialism/ hermeneutics, marxism/ critical theory, and structuralism/ post-structuralism - the thought of Wittgenstein was nevertheless influential at two important junctures: firstly it contributed directly to the initial conception, as well as to the later abandonment of the methodological programme of logical positivism (Haller, 1988:27-43), and secondly the conceptual framework of his later philosophy paved the way for the reception and assimilation on the continent of the "Kuhnian revolution" in the philosophy and history of science (Kuhn, 1962).

In spite of this constant and sustained influence of his philosophy and the continuous interest and reappropriation it has evoked - mainly within philosophy - since the 1920's, the past four or five years has witnessed a remarkably energetic resurgence of interest in and a heightened (almost vogue) appreciation for and application of his thought.¹ To my mind, this phenomenon is more than the fad it may sometimes seem to be, because it relates to two important and closely linked recent developments within philosophy and the methodology of science with regard to which Wittgenstein's philosophy is again proving to be extremely relevant. The first of these developments is the growing concern in the methodology of science in general, and more specifically in the methodology of the human sciences, with the decisive role played by conceptual frameworks, models and metaphors in the mediation of our experience of reality, as well as in our theoretical

¹ This is amply evidenced by the listings of recent publications on Wittgenstein in the "Subject Index" of the *Philosopher's Index* of the past few years. Two elaborate intellectual biographies by McGuinness (1988) and M. M. (1991) also confirmed and further stimulated this present resurgence of interest in

understanding and explanation of that experience.² Consequently attention is increasingly focused on the ways in which our conceptualisation of our experience are determined by the linguistic practices, systems of discourse and rhetorical strategies to which we adhere. The second development, closely linked to the first, is the growing suspicion and critique of the basic assumptions, pretensions, claims and practices - in fact the very "rationality" - of modern science as such in the light of various a postmodernist debunking of the project of modernism on which the epistemological justification of modern science rests.³ In this regard concepts and categories central to the assumptions and methodologies of the modern sciences, especially the human sciences, for example the concept of an outonomous subject (i.e. the cogito), and the concepts of objective rationality and a-historical truth, have been contested as ideological constructions which only served to justify the imperialistic claims of a specific cultural discourse, namely that of modernity, in its endeavour to impose itself socio-politically to the exclusion and subjection of other contending cultural discourses and strategies of knowledge and understanding. Accordingly the notion of the subject has been decentered by showing it to be a contingent effect of discourse, the notion of objective rationality has been deconstructed by disclosing the delusive deferral of meaning, the inevitable aporias, with which language contaminates human thought, and the notion of truth has been historicised and relativised by showing it to be dependant on social conditions and discursive practices rather than universal apriori. The common root of these developments is an awareness of what can be called the "omnipresence of language" in our dealing with the world.

One of the philosophical questions at the basis of these developments, is therefore the complex question of how we should understand the relation between language and experience, i.e. to what extent is our experience of the world mediated, or even determined, by our language; to what extent can we claim - the assumption of all knowledge claims! - that our language refer to, or reflect, an experience of reality that is given outside or independant of our language; and to what extent are we caught within the confines of the linguisticity, or - to use the Derridean term - the "textuality" of our experience of the world which allows us no direct access to the world, but commits us to a never-ending, groundless, undecidable and therefore playfull interpretation of the

² The following selection from the vast number of material on the subject will suffice for introductory purposes: Hester (1967), Lakoff & Johnson (1980), Knights & Cottle (1960), Leatherdale (1974), MacCormac (1976), Mooij (1976), Ortony (1980), Sacks (1978) and Wheelwright (1962, 1968).

³ For a wide ranging introduction to the critique of modern philosophy and the sciences from a postmodern perspective, counter-argumentation and a critical assessment of the debate, see the contributions of Rorty, Lyotard, Foucault, Derrida, Apel, Habermas, MacIntyre, Blumenberg and Taylor in Baynes, Bohman

"realities" we construct and deconstruct by means of our discourse (Kearney, 1986:111-133)?⁴ Understanding the complex relation between our language and our experience of the world, this very same question at the basis of present discussions in philosophy and the methodology of science, was also the major concern of Wittgenstein's philosophical work. It is therefore not surprising that his philosophy is again proving to be extremely relevant. It should however be noted that Wittgenstein was very much what one may call a philosopher's philosopher (Anscombe, 1991:1), articulating his thought in a way which is not easily accessible for non-philosophers in spite of the extreme bearing it has on the assumptions and practices of other disciplines.

Against this background the aim of this paper is to focus on the shift in Wittgenstein's understanding of the relation between language and experience by elucidating and contrasting his initial and eventual views, highlighting the reasons for this shift and showing up its implications in such a way that its broader relevance for present discussions within philosophy and the methodology of science becomes apparent. Towards this end I will distinguish between his initial understanding of language as the *symbolic representation of sensory experience* and his eventual understanding of the *dialectic of language games and forms of life*.

2. Language as symbolic representation

In many ways Wittgenstein's initial conception of the relation between language and experience, can be regarded as the culmination of and an exhaustive attempt to finally justify the assumptions - by pushing those assumptions to its extreme logical consequences - which dominated philosophical thinking about language and meaning from pre-Socratic days until the early twentieth century. Up until the twentieth century different philosophers, for instance Plato, Aristotle, Augustine and Kant, proposed different theories on the ontological and epistemological status of concepts (whether they should be regarded as reflections in human consciousness of apriori "universals" in the world, or whether they should be regarded as constructions of human consciousness projected onto the world.) What all of them however assumed is that words are names which refer to the concepts which are their fixed meanings, and thereby - by means of these concepts - to the entities (including qualities, actions and events) which are given in our experience of the world. Language was thus assumed to be a collection of names and the relation between our language and our experience of the

⁴ The *invenio* is of course with reference to Derrida's (1976:158) declaration: "il n'y a pas de hors-texte."

world was accordingly conceived as a relation of fixed correspondence between words as names and the entities named by them (Brümmer, 1989:41-51).

In his initial conception of the relation between language and experience, articulated in his *Tractatus Logico-Philosophicus*,⁵ Wittgenstein attempts to justify these traditional assumptions by reducing language, through a rigorous logical analysis, to the essential structure or "logical grammar" which makes the constitution of meaning - and therefore the reference of language to our experience - possible. According to Wittgenstein this "logical grammar" of language entails:

- 1) that all meaningful expressions in language can be shown to be logically reducible to one of (or a simple combination of) a restricted number of logical propositions (T:3.24-3.326, 5-5.5571);
- 2) that these logical propositions are structured by an arrangement of names or propositional symbols (T:3.14-3.23);
- 3) that each one of these symbols names a logically distinguishable object of experience ,e.g. a quality or a relation (T:3.14-3.23); and
- 4) that the combination of symbols constituting a proposition is arranged in such a way that the arrangement as such portrays or "pictures" the way in which the objects of our experience (to which the proposition refers) combine to constitute a fact (T:2.1-3.13).

The "logical grammar" of language thus necessitates that the propositional content of our expressions should be "logical pictures" of possible sensory facts in our experience of the world. And the world is, according to Wittgenstein, a "totality of facts" (T:1-2.063), i.e. the only experiences expressible or describable by means of language are actual or possible perceptions of actual or possible empirical facts. Uses of language which purport to convey any other meaning than a "logical picture", a symbolic representation, of an actual or possible empirical fact, are meaningless even though they may be correctly constructed grammatically and may seem - by their imitation of statements of fact - to have a semantic content. Although such uses of language demonstrate a tendency of the human mind to "run against the boundaries of language" (Wittgenstein, 1965:12), they are devoid of propositional content, can't be judged true or false by comparison with "what is the case" in the world, and can therefore only fulfil an emotive, but no cognitive function. They have no reference to the world of our experience. What is not given as actual or possible empirical facts within "logical space", i.e. within the confines of the world as a "totality of facts", can not be pictured by propositions and

⁵ In accordance with the common practice in Wittgenstein scholarship further references to this work will take the form of a capital T, followed by the number(s) of the relevant propositions in the *Tractatus*.

therefore not meaningfully referred to by means of language. The meaning or the value of facts cannot be represented symbolically in language, because it is not given as empirical facts within the world (T:6.4-6.522). Consequently only statements of fact can qualify as knowledge claims.

The relation between language and experience is thus a relation of *representation* due to a logical correspondence or isomorphic resemblance between the structure of our language and the structure of our factual experience of the world. The function of language is the symbolic representation of empirical facts, meanings are mental pictures of facts, and the knowledge claims we make by means of language can thus be objectively judged by comparison with the facts they picture. The implication of this understanding of the relation between language and experience for the philosophy and methodology of the human sciences was a very extreme one, namely that only descriptions of empirical fact - as exemplified in the physical sciences⁶ - have a cognitive content, i.e. refer to our experience of the world, and consequently that our scientific knowledge (in as far as it complies to this norm) is an objective, wholly neutral mirroring of an apriori and universally given structure of human experience of the world.

This initial conception of Wittgenstein was soon assimilated as the philosophical justification of the methodological programme of the logical positivism of the Vienna Circle (Monk, 1991: 282-297) which ironically - even though almost immediately discredited within the philosophy and methodology of science - still functions to this day as the ideal on which many practitioners in the human sciences model their procedures, and understand the meaning and reference of their use of language. Wittgenstein himself however discarded this initial conception of language as the symbolic representation of sensory experience, because the only plausible philosophical justification that can be given for it - namely the one he himself articulated in the *Tractatus* on the basis of an assumed isomorphic resemblance between propositions and facts - is inherently selfrefuting. If the "logical grammar" of language necessitates that the propositional content of our expressions should be "logical pictures" of possible sensory facts, and only that, for our expressions to be meaningful, then the entire articulation and justification of this understanding of language is indeed devoid of any meaning and cognitive content, because it can not itself be verified with reference to possible sensory facts. The assumption of an isomorphic resemblance between the structures of language and experience, and the definition of meaning as symbolic representation, expresses a philosophical (mis)understanding of the

⁶ As Wittgenstein (T:6.53) explicated: "The right method of philosophy would be this. To say nothing except what can be said, i.e. the propositions of natural science, i.e. something that has nothing to do with philosophy; and then always, when someone else wished to say something metaphysical, to demonstrate

relation between language and experience and not a verifiable perception of any empirical facts. Therefore the very conception of language as the symbolic representation of sensory experience fails to comply to the criteria of meaningfulness and cognitivity which it prescribes. The discovery of this insoluble contradiction can nevertheless, in spite of Wittgenstein's own initial intentions, be regarded as one of his most important contributions to our understanding of the relation between language and experience. By default it finally and effectively closed the book on a whole history of naivety with regard to the function of language in our experience of the world, by showing that it can not and should not be reduced to that of *representation*.

3. The dialectic of language games and forms of life

Fundamental to Wittgenstein's eventual conception of the relation between language and experience,⁷ is the conviction that his first abortive attempt went astray because of a common essentialistic bias which preconditioned his analysis (Pears, 1981:105-141). This bias, in itself an example of "the bewitchment of our intelligence by means of language" (PI:#109), was the assumption that language must have an essential structure, a universal and transcendental "logical grammar", on the basis of which meaningful expressions are constituted and to which they can be logically reduced. Language itself misleads us in assuming that there is such an essential structure, because it conditions us to think that the various applications of the same general term (for instance "dog") corresponds with an identical concept (the concept 'dog') which is the essential meaning of that term (BB:19, PI:#92-96). In the same way we assume that the various applications of general terms like "language" and "meaning" must correspond to some "essence" of language or "identity" of meaning (PI:#40,104).⁸ For this reason Wittgenstein disregarded the various everyday uses of language in his initial investigation for the sake of an apriori logical analysis (BB:17-19). If the archimedean point is however shifted from this essentialistic bias towards an aposteriori analysis of the ways in which we use language as a means of communication, a new understanding of meaning and a new, much more complex conception of the relation between language and experience emerges (PI:#97,103,108,114).

⁷ Articulated mainly in *The Blue and Brown Books* (BB), *Philosophical Remarks* (PR), and *Philosophical Grammar* (PG) which together evolved into Wittgenstein's other major work, namely the *Philosophical Investigations* (PI). Further references to *The Blue and Brown Books* will take the form of the abbreviated title (BB) followed by the relevant page(s), whilst further references to the *Philosophical Investigations* will take the form of the abbreviated title (PI) followed by the number(s) of the relevant paragraphs in the text.

⁸ As Wittgenstein (BB:1) put it: "What is the meaning of a word? We feel that we can't point to anything ... and yet ought to point to something. (We are up against one of the great sources of philosophical

By carrying out a vast range of de facto analyses of the ways in which language functions, the later Wittgenstein demonstrated that:

- 1) various forms of language-usage can be distinguished from one another (PI:#38,66,67,77);⁹
- 2) each one of these forms of language-usage constitute different means of meaningful communication (PI: #12,23); and
- 3) these forms of language-usage and the nature of the meaning constituted by them is not logically reducible to one common universal structure or "logical grammar" (PI:#654,655).

It is shown for instance that the same word fulfill different semantical functions within different forms of language-use depending on the purpose of that specific form of language-use. Consequently a word does not have one identical meaning, and the meaning content of linguistic expressions are therefore not to be understood as the sumtotal of the meanings of the words of which it consist. Every word and every linguistic expression can fulfil a variety of semantical functions which cannot be reduced to one essential meaning. Which one of these semantical functions is realised, is determined by the purpose with wich language is actually used in a given context. As he exclaimed (PI:#421): "Look at the word or the sentence as an instrument, and at its meaning as its employment." Thus Wittgenstein's other famous observation (PI: #43): "For a large class of cases - though not for all - in which we employ the word 'meaning' it can be defined thus: The meaning of a word is its use in the language." The different usages of linguistic expressions, and therefore the specific semantical functions they fulfil, constitute the various forms of language-usage depicted by Wittgenstein as "language games" (PI:#2,8,18-24,33,49,125-127).¹⁰

Through his analyses Wittgenstein further showed that each "language game" implies a unique "logic" or "depth grammar" (PI:#89,111,387,496,497,664) consisting of a set of publicly accepted rules or culturally determined conventions which govern the use of language within that "language game", and subsequently determines for which communicative purposes linguistic expressions can be used in that "language game" and which semantical function they will fulfil (PI:#53,54,147b,172-178,190,372,496-499). The distinctive "depth grammars" or sets of usage rules of our "language games" result, according to Wittgenstein, from the various ways and forms of our experience of the world which confronts us with disparate demands of interpretation and understanding. These multiple modes of our experience of the world which characterises our everyday

⁹ As he stated elsewhere (Zettel:#322): "Language is not defined for us as an arrangement fulfilling one definite purpose. Rather "language" is for us a name for a collection."

¹⁰ As Wittgenstein explained (BB:#23): "...[T]he term 'language game' is meant to bring into prominence the fact that the speaking of language is part of an activity, or of a form of life."

encounter with the world, the behavioral patterns, social codes and conventions necessitated and constituted by them, Wittgenstein depicted as "forms of life" (PI: #19,23,241). A "form of life" is thus a collectively shared and culturally conditioned strategy of orientation in the world on the basis of certain presuppositions about experienced aspects of the world (Hunter, 1971:273-297; Gier, 1981:19-32).

The relation between language and experience is therefore not a one dimensional relation of representation, but a multifarious complex, a dialectic of mutual mediation and interaction between "language games" and "forms of life". The semantical functions fulfilled by linguistic expressions, are determined by the usage of those expressions in accordance with the rules of a "language game", those rules being necessitated by a specific "form of life" in order to facilitate meaningful communication about the experiences which belong to that "form of life". The collectively shared experiences which constitute a specific "form of life" thus precondition the meaning and the intersubjective understanding of a "language game". Inversely, without the constitution and exchange of meaning, the interpretation and conceptualisation of our experience by means of a plurality of "language games", our experience of the world would be senseless and would not lead to the collective formation of the "forms of life" in which our common experience of the world are encapsulated. The dialectic of "language games" and "forms of life" thus entails on the one hand that our experience of the world - as structured by various "forms of life" - condition the semantical functions which our linguistic expressions can fulfill within a certain "language game", whilst on the other hand the "forms of life" by which our experience of the world are structured and thus conceptualised are constituted by the "language games" by means of which intersubjective communication and understanding becomes possible. Although our usages of language structure our experiences of the world, this structuring is never arbitrary, but always necessitated and conditioned by the "forms of life" which characterise our experiences of the world.

4. Concluding remarks:

The untenability of Wittgenstein's initial conception of the relation between language and experience, made him realise that language should not be regarded as the symbolic representation of our experience of the world per se. For him, this however didn't imply that our language can not refer to, or reflect, experiences of reality that is given outside or independent of our language, and that we should therefore accept that we are caught within the confines of a closed, self-referential system of language in which signs only refer to other signs and thereby endlessly defer our access to the world - as post-structuralist and deconstruction theory wants us to believe. According to his later analysis of the dialectic of "language games" and "forms of life" our linguistic endeavours to understand and communicate the content of our experiences of the world are not just arbitrary

interpretations of "realities" we construct and deconstruct by means of our discourse. What it does imply, is that:

- 1) the representational or descriptive usages of language is part of a specific "language game", for example the "language game" of a specific scientific practice;
- 2) the "objects" or "facts" of experience referred to by linguistic expressions in such a "language game" are experienced as part of a specific "form of life" which are mediated by, and is therefore related to that "language game" only;
- 3) the knowledge and truth claims made in such a "language game" cannot be universalised, but are relative to the "form of life" pertaining to that language game;
- 4) there are "language games" other than the representational or descriptive usages of language which are as meaningful and necessary in our endeavour to understand the phenomena of our experience, because they apply to those "forms of life" which cannot be referred to in a representational way, the content of which we can however express and communicate meaningfully by other usages of language.

Accordingly, Wittgenstein's later thought does link up with a postmodern critique of the universalistic and objectivistic assumptions and claims of modern science by showing its alleged, objective representation of our experience of the world, to be dependent on specific "language games" or strategies of discourse which refer only to what and how we experience the world within the specific "forms of life" of our scientific procedures and methodologies.

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Academic psychology, applied psychology, and professional practice in Sweden

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For the early pioneers of psychology in Sweden in the years before and immediately after the Second World War **theoretical and practical tasks** were very close connected. Psychologists made an academic career aiming at professorships with various labels and at the same time they undertook practical tasks as psychologists within various practicing fields. They all had both academic and professional competence and it is not before the 1960's that we find the two fields - psychology as a science and psychology as a profession - going in different directions.

Psychologists had to fight for their existence on two different grounds and with arguments that sometimes contradicted each other. Psychology was in the early twentieth century a new experimental science. The young psychologists had to argue for its academic recognition and in doing so they maintained that psychology had its own subject matter and a valid methodology. On the other hand they dissociated themselves from philosophical psychology; psychological problems could not be solved by philosophical arguments. **A proper education of an experimental psychologist included physiology, physics and mathematical statistics.** But on the other hand a scientific study of psychology could not be reduced to any other specific discipline; it was a full faculty rather than a single discipline.

of Jaederholm

Psychologists were not always succesful in convincing their neighbouring disciplines of their **legitimate place** in the academic curricula. Psychology (or education) was introduced in the universities 1908 within the Faculty of Arts. The psychologist's ambition to prove the scientific status of psychology was important to get independent status apart from the departments of philosophy. **Psychology was the only experimental discipline in the Faculty of Arts.** Whereas the other departments gathered at a seminar discussing texts, the psychologists belonged to a laboratory that demanded much more space and equipments. But this tendency lead colleagues in the faculty (historians and philologists) to look at psychologists with suspicion as mechanical counters of meaningless syllables that lacked humanistic *Bildung*.

But the reason why psychology became an independent discipline within the universities had nothing to do with its status

as an experimental science. If psychologists only had referred to their strict scientific methodology, they may have stayed within the philosophical departments for a longer time. The first chairs were erected in pedagogics and not in psychology to serve the needs for the proper education of teachers; the students up to the 1950's underwent examinations in pedagogics and not in psychology. Psychology, however, was regarded as a necessary foundation for educational studies and most early professors had their main research interests in differential psychology. That meant that a heavy course in experimental and physiological psychology - later psychometrical training was added - functioned as a basic requirement, a severe impediment, for the students.

The academic psychologists knew that to promote the evolution of psychology they also had to prove its application in society and they had to do this work themselves. In the first generation before the Second World War this was done mainly within the school, but also to some extent in the industry. As no educated psychologists existed the professors had to assist and direct teachers or works managers to use intelligence and aptitude tests in their work. No one in the 1930's seemed to expect a future profession of psychology. Psychology was regarded as a scientific discipline and it should be included in the educational curricula of a lot of professions, where the scientific principles could be applied by teachers, industrial managers, etc. But applied psychology did not claim an active role for the reformation of society and the education of the working class. Differential psychology had proved that the abilities and talents of men were inborn and could not be improved. Psychometrics and especially intelligence testing should instead be used to organize the society in a rational way in controlling the workers.

After the Second World War, when the Social Democrats implemented important reforms in society, we can find two different ways of handling the relation between science and profession, one conservative and one radical and reformist. The conservatives restricted the university for theoretical investigations; psychologists with a theoretical training should keep their hands clean from dirty practical problems. Applied institutions should be kept outside the university. The radicals tried to use differential psychology to identify a reserve of talent in society. A majority of intellectuals accepted the reformist ideology of the Social Democrat ideology, and the younger psychologist employed a concept of intelligence that differed from the older generation: Talents were spread equally among the classes and depended more on social environment than on nature. A considerable part of psychological research in the 1940's and 1950's were ordered from State Commissions or directed towards practical social problems.

This situation constituted a serious problem for psychology as a theoretical discipline: the applied psychology and practical work should evolve from a theoretical basis. In Stockholm in the 1950's the methodological training was strengthened in order to increase the scientific status of psychology. Application had to be a secondary matter. This aim was, however, connected to a radical ideology in a longer perspective. Scientific psychology could not be used in a practical situation. It had the critical aim of scrutinizing and demolishing the prejudices in man and society, so that a better knowledge can evolve.

At the end of the 1940's the first generation of educated psychologists began to apply for posts in the school, the industrial and military sector, and last but not least in the clinical sector. They had to compete with other occupations who performed the same tasks, but without the same theoretical legitimation as the psychologists.

In the paper different trends in this development are discussed, and exemplifications will be taken from different Swedish psychologists. Among the early psychologists I will deal with Gustav Jaederholm (1882-1936), professor of philosophy and education in Gothenburg, Rudolf Anderberg (1892-1955), professor of education in Uppsala and David Katz (1884-1953), professor of education in Stockholm. In the younger generation the most important figures are Torsten Husén (b. 1916), professor of education in Stockholm and Gösta Ekman (1920- 71), professor of psychology in Stockholm.

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ABSTRACT

The Reception of Psychoanalysis within Medicine: the case of general medicine practice in post-war Britain

This paper focuses upon the decisive impact of psychoanalytic models on the professional fortunes of general medical practice in post-war Britain. The paper shows how the deployment of psychoanalytic themes - especially that of the **analyst's counter-transference** - were re-worked within the context of medicine, resulting in a transformation in the very conception of the nature of general medical practice as a vocation. Turning to the wider theme of the comparative reception of psychoanalysis in other disciplines within the human sciences, the paper argues that there are no general conclusions that can be drawn about the reception of psychoanalysis, since these would presuppose some kind of central "core" to psychoanalysis itself. Rather, not only are radically different aspects of psychoanalysis taken up in different disciplinary contexts, but the identity of psychoanalysis itself frequently undergoes transformation in the process.

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The Reception of Psychoanalysis within Medicine: the Case of
General Medical Practice in post-war Britain

- Abstract -

Psychoanalysis has found itself disseminated in various areas - from philosophy and literary criticism to psychology and various branches of medicine. One area where the impact of psychoanalysis has undoubtedly been decisive has been within general medical practice in post-war Britain. Consideration of this field as a case-study of the dissemination and reception of psychoanalysis should help to throw some light on the nature of psychoanalysis itself as a discipline.

Following the introduction of a National Health Service in Britain (1948) general medical practice found itself faced with a crisis of identity. What was specific to general medical practice as a vocation? What particular norm of knowledge was it to espouse if it was to retain a distinctive identity separate from hospital medicine? From around the mid-1940s, as this paper will show, the answer to these questions was formulated predominantly in terms of a model of expertise centred upon the "country practitioner". Using archival material from the Royal College of Practitioners, the paper will analyze briefly the formulation and entrenchment of this model. Of special importance here was the idea that the general practitioner's special field was minor epidemic infections (this paradigm being fabricated around clinical confrontation with illnesses such as epidemic catarrhal jaundice), especially as these related to the family domain. However, by the end of the 1950s this mode of knowledge was beginning to falter. This was partly because practitioners found the mapping of

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model

infections in their practices to be a time-consuming exercise, often without any notable clinical reward, and partly because of the growth of conceptions of general practice derived from the domain of psychoanalysis.

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- ① This psychoanalytic influence had its focus in two main currents of thinking. The first concerned a more or less direct impact of psychoanalytic thought within medicine; here the key figure was undoubtedly the psychoanalyst Michael Balint and his work with general practitioners at the Tavistock Clinic in London. The second was a more diffuse influence centred upon the question of the internal dynamics of the "doctor-patient relationship". By the mid-1960s, and again taking work associated with the Royal College of General Practitioners as the primary data-source, the paper will show how this broadly psychoanalytic model became firmly entrenched as the epistemological norm of general practice. Indeed it was this psychoanalytic model which, the paper will argue, was at the source of the so-called "Renaissance" of general medical practice that occurred in Britain after the mid-1960s.

But what was it that was decisive about the psychoanalytic model? Or rather, we might ask which model of psychoanalysis was decisive? For the model adopted in general practice in Britain owed little to the more renowned - perhaps still scandalous - themes of psychoanalysis; be they the question of sexuality or the emphasis upon the unravelling of elements repressed in early childhood. What was significant in the context of medicine was less such substantive psychoanalytic "doctrines" so much as a particular "technical" principle associated with psychoanalysis; namely, as the paper will show, the theme of counter-transference in the encounter itself. In short, the decisive element in the reception of psychoanalysis within medicine in Britain was not a particular set of ideas but a

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2/ consultation
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particular model of expertise; one which emphasized especially the reflexivity of the medical practitioner.

This case-study raises several questions of interest for those considering more generally the reception of psychoanalysis within its allied disciplines. The most important of these concerns the nature of psychoanalysis itself. The research problematic of the "reception" of psychoanalysis seems to imply that we know what psychoanalysis actually is in the first place, that it possesses some kind of "core" set of components that will be subject to "reception" elsewhere.

This paper will close with a contestation of the idea that there is any such core to psychoanalysis. Rather what the comparative analysis of the epistemological mobilization of psychoanalysis will tend to show is the differentiated nature of its dissemination and reception within other disciplines.

- 1 Sometimes psychoanalysis has been utilized almost entirely as a negative problematic to be contested; for example within the philosophy of science. Sometimes, as with the reception of
- 2 psychoanalysis within much of general psychology, the utilization of psychoanalysis has taken the form more of a kind of reference point, or a (generally negative) principle of orientation, rather than something to be comprehensively embraced or denounced. Sometimes, on the other hand,
- 3 psychoanalysis has become a theoretical tool for the avant-garde; for example, within literary criticism. Lastly, in the
- 4 more "clinical" disciplines such as general practice, family therapy, or social work the role of psychoanalysis has been predominantly a "technical" one, serving to provide a liberal - often self-consciously "liberating" - principle of expertise. In this latter case, what is often of interest is less the passive "reception" of psychoanalysis than the way it has been actively re-modelled and transformed to suit the particular interests of the discipline in question.

THE "SCIENTIFIC SUPERNATURAL" IN PSYCHOLOGY.
PSYCHICAL RESEARCH AT THE TURNING POINT OF THE XIXth CENTURY.

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ABSTRACT - In France, at the end of the last century, many famous psychologists were interested, although in different ways, in psychical research. The interest of these studies lies in the network of questions they set to psychology. I will examine the origins of psychical research in France and characterize two classes of positions taken by psychologists in relation to these studies: the positivist view, which, in particular, deals with methodological questions and with relations between psychology and physiology. An alternative conception can be related to the dim perception, among psychologists, of the unconscious processes at work in so-called occult phenomena.

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**THE "SCIENTIFIC SUPERNATURAL" IN PSYCHOLOGY.
PSYCHICAL RESEARCH AT THE TURNING POINT OF THE XIXth CENTURY.**

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Nowadays, in its common representation, psychology is still binded to marvel. A mere look across popular magazines makes it obvious: parapsychology cohabits with academic psychology without any problem. But, in France, at the end of the last century, many famous psychologists were interested, although in different ways, in the occult. Examining this studies, one realizes that their interest lies in the network of questions they raise: many problems of psychology of the moment are encountered, and they are possibly still crucial questions. Consequently, my point is not to discuss the merits of psychical research, but to determine what it can teach us about French psychology in its beginning. I don't intend either to talk about all of the many publications concerning psychical research at the end of the 19th century, but only about those at which well known psychologists or scientists collaborated. I will specifically examine the papers which appeared in the *Bulletin de l'Institut Général Psychologique*.

As a matter of fact, when the 4th International Congress of Psychology was held in Paris in 1900, Julian Ochorowicz announced the creation of the *Institut Psychologique International*, which was actually born in 1901 and called "*Institut Psychique International*". In the sponsorship committee, one could find many universally known psychologists (such as Baldwin, Bergson, Bernheim, Flournoy, Janet, Lombroso, Myers, Ribot, Richet, Sully,

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Tarde, and so on) as well as famous physiologists and physicists. At the same time, a society was founded in order to collect grants for the Institute. This society became, in 1904, la **Société Française de Psychologie**. The Institute was explicitly intended to promote especially psychical research. Therefore, many papers related with this topic can be found in the **Bulletin of the Institute** (which was renamed **Institut Général Psychologique** in 1902). But they were also published in other psychological reviews, such as **L'Année Psychologique** or the **Journal de Psychologie Normale et Pathologique**. We can see that, in its beginning, French psychology did not reject studies about the occult. Phenomena which were mostly studied are telepathy and mediumnism.

This interest, in France, certainly proceeds from late 19th century works on hypnotism and suggestion: I might mention, for example, Pierre Janet's studies about "mental suggestion" and Luys' on the influence of drugs at a distance, or the studies about hypersensitivity in hypnotized hysterics. But, from an institutionnal point of view, the model is the **Society for Psychical Research** of London.

Roughly speaking, to types of positions can be distinguished among these authors.

On the one hand, a positivist position, shared by many of them. In this point of view, the questions raised concern the scientific legitimacy of such studies and bring up methodological problems: how to observe scientifically phenomema which cannot

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be experimentally produced? Another question deals with the relationships between psychology and physiology, well illustrated in the debate concerning the existence of so-called "N-rays": could not perception at a distance be explained in terms of an undetermined energy? Here, we can emphasize the role played, at the origin of this approach, by the discovery of X-rays and by the beginning of the telephone. On the opposite, other authors maintain that psychology should take the opportunity, thanks to studies about occult phenomena, to divorce from physiology in order to get an autonomous field.

A second attitude, which generally belongs to alienists consists in considering phenomena in terms of mental pathology ("spirit madness", for example). To account for the puerility of some of their manifestations, they compare them to a regression back to childhood or to crowd psychology. Most subtle analysis is due to Flournoy (whose positions are close to Freud's later ones). He thinks that the key problem in psychical research is telepathy and emphasizes the importance, in the interpretation of facts, of the latent wish of one of the protagonists.

To conclude, one could relate this studies with the problems created to French psychology by its positivist rejection of metaphysics. The term "metapsychics", due to Charles Richet can be paralleled, in its implications, with the Freudian purpose to "translate metaphysics into metapsychology".

"Social physics": its rise and fall,' Jennifer Platt

Abstract

The project of a 'social physics' in the 1940s and 1950s in the U.S.A. is discussed, with special reference to the role of the physicist John Q. Stewart. Some of its connections with sociology and sociologists are outlined, attention is drawn to the general issue of the metaphorical use of natural-science terms in sociology, and reasons for the rise and fall of 'social physics' are considered.

"Social physics": its rise and fall.

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The story of the aspiration of social scientists to be "scientific", and to seek or claim convergence with the natural sciences, is a familiar one. What they have meant by "science" has not always been clear, has changed over time, and has not necessarily involved any real knowledge of natural science. One way in which such knowledge could be brought to bear is by contact with natural scientists, and there has been a trickle of people originally trained in natural science into sociology; they are probably most often found in the sociology of science or in "mathematical sociology", both areas where their backgrounds have some direct relevance.

However, there have been a few cases where physicists - from arguably the most prestigious natural science - have played a role in sociology while retaining their identity as physicists, and there was a period in the 1940s and 1950s when the idea of a fairly literal "social physics" was being promulgated. This paper asks why that happened, what was its influence and significance, and why it has left so little trace.

John Q. Stewart, an astrophysicist at Princeton, was active over a number of years in promoting "social physics"; he organised meetings at which physical and social scientists met to discuss such ideas, he received a grant from the Rockefeller Foundation to pursue this, and he published empirical articles in social-scientific journals. His conception of the relevant methods of physics would be regarded as philosophically naive nowadays (and might also have been then). He sees the history of physics as proceeding from "the collection of quantitative observations" to "their condensation into empirical mathematical regularities" and their "theoretical interpretation of the latter", and concludes that "if there is to be a social physics, its beginnings must follow the same standard pattern", (Stewart 1947: 179). The work he does consists, then, of a search for quantitative regularities, without any initial theoretical rationale or much worry if, when found, they appear inexplicable.

His own data are mainly demographic and geographical, relating population distribution to spatial distance. One set of data he used were on the geographical origins of students at Harvard, MIT, Princeton and Yale; he shows that the numbers from a given state are roughly proportional to the state's population divided by its distance in miles (Stewart 1942: 63). He also demonstrates various patterns shown in census data for the distribution of the whole population of the U.S. across its surface. (Stewart 1948a and b). However, he goes beyond these data to suggest ways in which they might be theorized in terms analogous to those of physics. A first step is to conceptualize them in quasi-physical terms. The density of population in a given area is treated as its "potential", or influence at a distance on other points, in strict analogy with the potential of mass in physics. (e.g. Stewart 1948a: 22) The related idea of "demographic gravitation" is used, with number of people substituting for mass, and the fascinating concept of the "human gas" is suggested to account for the apparent resistance to social gravitation. ("Were it not for the expansive force of the human gas, representing the need of individuals for elbow-room, the center-seeking force of gravitation would eventually pile everyone up at one place." (Stewart 1948a: 23).) This naturally leads to positing "Demographic energy" as a force acting between separate individuals, and in his more elaborated presentations (e.g. Stewart 1948: 6) complete systems of equations from physics are suggested to be applicable to social data.

It is not always clear whether the application of physical concepts and theories is intended merely as a convenient summary of empirical uniformities, as a suggestive analogy, or as a serious proposal of a unifying theory for social and physical topics. The most obviously problematic aspect of the whole enterprise - if one accepts the claims made for empirical fit - is the extent to which the concepts used are metaphorical, and the analogies drawn questionable. Little reflection is required to realise that terms drawn from natural science but used in apparently purely metaphorical ways are much more widespread in sociology. (For instance, the well-known theorist Talcott Parsons wrote a joint paper in 1951 called "The

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dimensions of action-space" which included phrases such as "...A constant rate will constitute a stabilized flow of such energy through the system..." (Parsons et al, 1953: 95,) or many references to "dynamics".) Should these perhaps be treated as making more literal claims than most contemporary readers probably realise?

There were certainly tendencies within sociology which could relate easily to Stewart's work. Some of his papers appeared in the journal Sociometry, which at that time was the flagship of Moreno's "sociometry" as well as including other heavily quantitative papers. Its editor from 1941 was George Lundberg, a well-known proponent of "scientific method" and quantification who corresponded extensively with Stewart. Curiously, however, he was actively interested in the work on philosophy of science of the Vienna Circle, whose background was in physics and mathematics but whose conception of appropriate method was very different from Stewart's. (There are areas here to be explored about different influences from physics.) Lundberg was closely associated with Stuart Dodd, a figure prominent at the time but now almost forgotten, who proposed a general theory of society in terms of "dimensions" of time, space and population expressed in a set of mathematical formulae. (Dodd 1942). One of Stewart's papers was republished in a well-known reader on demography (Spengler and Duncan 1956), and there was an established tradition of quantitative social ecology (e.g. Hawley 1950) which used the same sorts of data. Some work on small groups, also fashionable at the time, took a similar approach in the sense of quantifying data expressed in terms of highly abstract concepts, (cf. Bales 1950). Most of these tendencies, however, are now seen as passe, perhaps slightly ridiculous, and not seriously-claimed ancestors of current interests. It is also hard to imagine contemporary physicists relating physical ideas to sociology.

Why did this convergence happen at that time, and why, despite what could be seen as its apparent promise, does it not appear to have had intellectual descendants? A possible hypothesis is that there was a happy coincidence between a time when physicists felt they could solve all problems and sociologists were happy

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to use more-or-less metaphorical borrowings to support their own claims to legitimacy. Further work is needed on Stewart's position within physics and the other people involved in his group, (1) as well as on physical ideas in sociology. Lewin's field theory also suggests areas of related interest in other disciplines.

Note

(1) Bridgman, a distinguished physicist famous in the social sciences for his introduction of "operationalism", also later wrote in areas related to the social sciences, although this is hardly ever mentioned in social science now (Bridgman 1938). Walter's recent biography shows, incidentally, how for him operationalism helped to solve some of the problems caused for a very practical experimental physicist by Einstein's ideas by saving empiricism. Bridgman was in active contact with social scientists, especially psychologists, although the way in which they have used his ideas probably has little connection with what they originally meant to him.

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**From Independent Security to Secure Attachment.
The Development of Mary Ainsworth's Attachment Theory**

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From Independent Security to Secure Attachment. The Development of Mary Ainsworth's Attachment Theory

In 1988, a seemingly innocent review paper on the effects of infant day care on the emotional development of children caused an explosive public reaction.¹ The paper, published in *Early Childhood Research Quarterly*, stated that children who spent too much time in day care facilities or were otherwise separated from their mother on a regular basis would develop psychological problems later in life. This report sent "shudders of guilt" through millions of parents—it was either quoted as one more proof of how conservatives could abuse psychology to force liberated women into old-fashioned role patterns or as a long overdue humane appeal to parents to pay due attention to their children. The same ambivalence applies to Mary Ainsworth, who developed the research method on which the review paper was based. On the one hand, her research was influential in overcoming Freud's neglect of the role of the mother in, thereby making up for a serious omissions in psychoanalytic theory. On the other hand, many people drew conservative implications from her theories: the nuclear family with an omnipresent and always available mother was the best and only guarantee for the healthy development of children. Her influence on advice literature for parents reassures some and worries others about their parental responsibilities.

In this paper I will focus on the research methods that have been used to investigate the effects of maternal deprivation on child development. The research central to the review paper relied on the **Strange Situation Test** developed in 1964 by Ainsworth in an attempt to provide experimental evidence for John Bowlby's attachment theory.² In this test, the behavior of toddlers is observed in the presence of a stranger, with the mother alternately present and absent.³ The reactions of the children differed markedly. On the basis of these observations, Ainsworth defined three distinct reaction patterns. The first group was **"securely attached"**: these babies protested upon separation from their mother and were happy to see her return. In the presence of the mother, these babies confidently explored the strange situation, while regularly checking that their mothers was still there. The two other groups were **"insecurely"** or **"anxiously"** attached. The second group was ambivalent: these babies clung to their mothers and protested upon separation. When their

mothers returned, they found themselves ignored by their children. The third group was "avoidant": the presence or absence of the mothers did not overtly affect explorative behavior. The children ignored their mothers from the beginning. The researchers combined these laboratory observations with extensive observations in the home environment. They concluded that mothers of securely attached children were warm and responsive to their babies while mothers of insecurely attached babies were unresponsive, rejecting, or inconsistent. On the one hand, this research dispelled earlier behavioristic notions by declaring that being emotionally responsive to children is more important than keeping regular schedules. On the other hand, it also led parents to feel an enormous responsibility to provide warm and tender care for their children in order for them to become securely attached.⁴

Who was Mary Ainsworth, creator of the Strange Situation Test? Educated at the University of Toronto during the 1930's, Ainsworth, née Salter, received an education in one of the Child Study Centers that were funded by the Rockefeller Foundation. The ideas on child training of this institute were behavioristic--they emphasized exploration of the world, learning, and social adjustment, rather than emotional growth. In 1950 Ainsworth started her collaboration with John Bowlby, who wanted to reform psycho-analytic theory to give more emphasis to the importance of the mother figure for the young child.⁵ The ideas that they developed during their collaboration were tested in a research project in Uganda and eventually with the Strange Situation Test. One might have the impression that between Ainsworth's early work in a behavioristic paradigm and her later, more psycho-analytically oriented work, a dramatic conversion must have taken place. This is not the case. Despite the enormous differences between these paradigms, Ainsworth was able to integrate them, maintaining a consistent interest in the emotional development of children.

In 1929, at age 16, Mary Salter started studying psychology at the University of Toronto, under William Emet Blatz, who was by then the leading child psychologist in Canada.⁶ The functionalistic psychology of the Chicago school permeated his work as a consequence of his training in Chicago under Harvey Carr from 1921 to 1924. His work also reflected a strong mental hygiene influence. In Toronto longitudinal research projects on the development of social adjustment were conducted in natural settings, not in the lab.⁷

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This required new methods of observation.⁸ Their findings could teach parents how to raise happier, healthier, and well-adjusted kids.

Blatz believed that investigating the relation of the individual to the environment shed light on social adjustment. According to his ideas, the child became increasingly aware of the consequences of its manipulative actions on this environment as a consequence of free explorative activity. This learning process was seen as crucial for adjustment later in life, which Blatz defined as the willingness of the individual to accept the consequences of one's actions.⁹ Free exploration would result in the formation of habits, or "scientific forms of attack and defense."¹⁰ The romantic and somewhat outdated masculine ideal of the explorer still vividly lingered in the dreams of American psychologists.

Blatz's perspective on learning had strong individualistic tendencies. The child should be given the opportunity freely to manipulate the environment--the child is a tinkerer, rather than a sponge. This would arguably bring about self-confidence, self-control, independence and initiative. One cannot escape the impression that Blatz was observing kids but was actually seeing the boys. Exploration, rather than the contacts with the people close to the child are important for the formation of personality. Parents had to become the invisible managers of the child's environment in order to create the right conditions for their children's managerial or engineering skills to develop. Norms and rules were viewed as objective environmental constraints that a child has to face while engaging in his explorative activities. Other people appeared as elements of the environment as well--as objects for manipulation. Blatz's explorer seems to have some traits in common with Robinson Crusoe.

Emotions were placed in the same dialectic of individual and environment--they were seen as an unsuccessful way of interacting with the environment or an inadequate mode of adjustment, occurring when the child could not deal with a problematic situation.¹¹ Emotional behavior became more and more adaptive and, consequently, disappears to make room for adequate adaptive behavior that successfully meets the demands posed by the environment. With increasing experience and the development of habits, "an analytical attack on the conflicting situation becomes possible."¹²

The negative attitude towards emotions reveals itself in the advice literature written by Blatz. Blatz argued that parents would provide faulty training if they reacted to the

emotional episodes of their children: it was best to ignore their temper tantrums and fears. Reacting to them would reinforce unsuccessful adjustment and cause maladjustive behavior later in life. Parental emotions were seen as equally detrimental to the development of the child. Blatz shared Watson's feelings on the dangers of too much mother love.¹³ From a perspective in which learning was considered as exploration of, and experimentation with, the physical world, there was not much place to appreciate social contacts, closeness, and warmth. A new era of mother-blaming had commenced.

Blatz subordinated emotion to learning, yet his approach differed from Watson's behaviorism, in which emotions were explained by referring to innate drives. It also differed from then current physiological approaches. In order to shed light on the nature of a given emotional episode, Blatz analyzed the antecedent situation for which a given emotional episode is seen as a response.¹⁴ From Blatz's functionalist perspective, emotions were responses, albeit inadequate ones, to particular situations. Blatz and his colleagues tried to identify the general characteristics of the situations that would evoke emotional responses. For example, he identified the difficulty of adjusting to a rigid school routine as a source of emotional episodes.¹⁵ Other research sketched more general characteristics of situations which evoked emotions. Among them was the unfamiliar or strange situation--a situation from which the child could not extricate himself and for which he does not yet have an adequate response.¹⁶

At the peak of his career, Blatz developed security theory to unify his previous research. He defined security as the ability and the confidence to make one's own decisions and the willingness to accept the consequences of these decisions.¹⁷ Rather than focussing on separate actions, he chose to place emphasis on an inner disposition which could be assessed with the aid of rating scales and self-inventories. The emphasis on learning and habit formation made room for inner growth.¹⁸

There were the theories that were presented to Mary Salter during her education in Toronto. In her dissertation, she elaborated on the concept of security as it was preliminary formulated by Blatz. According to Ainsworth, a feeling of security obtains when children feel confident to face a given situation or feel assured that they can depend upon someone else to handle it. The latter response shows dependent, the former independent, security,

and, ideally, in the course of development "a movement from the dependent security of childhood towards the independent security of adult life" can be observed.¹⁹ This definition of security differed markedly from Blatz's, who only emphasized independence. Her observations proved that the secure group showed greater social participation.²⁰ The secure group scored high on familial security as well. Apparently, "security in the family plus emancipation from it is associated with security in intimacies outside of the family."²¹ Deviations from this ideal pattern could result in continuous dependency on the family, other forms of dependent security, or compensations for insecurity such as intolerance.

According to Ainsworth, dependent security early in life is a condition for the development of security later in life. In her own words:

Familial security in the early stages is of a dependent type and forms a basis from which the individual can work out gradually forming new skills and interests in other fields.²²

Dependent security is a necessary condition for development, rather than an impediment to it. Adult security is characterized as the development of social skills "as well as new bases of dependence outside the family," rather than "mere" independence or having formed an adaptive set of habits.²³ Instead of viewing emotions as an indication of a failure to adjust to the challenges offered by the environment, emotional security becomes a condition for explorative action, which is now considered secondary. In these respects, Ainsworth's approach differed radically from Blatz's. However, she retained his emphasis on the value of longitudinal research and a functionalist approach to development and emotions.

In 1950, Mary Ainsworth moved to London, where her collaboration with John Bowlby started. At that moment, Bowlby was trying to modify psychoanalytic theory by emphasizing the importance of early mother-child interaction, rather than internal conflicts. He was deeply inspired by the ethological research of Niko Tinbergen and Konrad Lorenz: the unimaginative behavior of ducklings provided Bowlby with a mighty weapon to surmount the Father's neglect of "the mother" in psychoanalytic theory. Later on, he quoted the work of Harry Harlow as a confirmation of his own theories: separations from the mother are disastrous developmentally because they thwart an instinctual need.²⁴ In Harlow's world, observations of frustrated and depressed baby rhesus monkeys desperately

clinging to iron mothers led naturally to conclusions on the damaging influences of impersonal day care on young children.²⁵ Ainsworth collaborated with Bowlby on a number of research projects investigating the effects of maternal deprivation. Using Bowlby's words, (in-)dependent security became (in-)secure attachment, placing even more emphasis on emotional attachment as a condition for mental health.²⁶ Now it was seen as beneficial to cuddle children.

Time in Africa afforded Ainsworth the opportunity to test her theory with extensive observations on the development of African children. From this period stem the observations which inspired her to design the Strange Situation Test.²⁷ She observed that a group of children she considered securely attached explored the world freely in the presence of their mother while making sure every once in a while that she was still there. This explorative behavior stopped when the mother left. These children were secure that, in case they became uneasy or frightened while exploring, they could retreat to a parent figure to receive comfort and reassurance. The parent's availability provided the child with a secure base from which to explore and learn. Ainsworth seemed to have an open eye for both boys and girls while observing children. In her approach, the child is still an explorer, though with an occasional small ego that needs to be supported.

Instead of focussing on the relation between familial and extra-familial security and the general development towards independence, Ainsworth made many minutely detailed observations of actual mother-child interactions. These observations were made over a period of a few years. Ainsworth concluded that the observations made over a relatively short time interval of mother-child interaction were a reliable predictor of the attachment pattern between mother and child. The Strange Situation Test was designed just to observe the explorative behavior of one-year-old children in the presence and absence of their mothers. According to her, this would be an indication for the later behavior as well.

After moving to Baltimore in 1956, Mary Ainsworth was appointed at Johns Hopkins University, where she designed the Strange Situation Test, thereby initiating a distinct research tradition in developmental psychology that still flourishes today. Her work initiated a change in developmental psychology away from Piagetian cognitive psychology and behavioristic studies. Leafing through recent issues of the journal *Child Development* shows

the enormous impact of her work.

Conclusion

In 1989 the American Psychological Association awarded Mary Ainsworth and John Bowlby the Award for Distinguished Scientific Contributions. The researchers responded with a joint address, "An Ethological Approach to Personality Development,"²⁸ in which they gave an overview of the history of their collaboration as well as current research in attachment. Although Mary Ainsworth began her career in developmental psychology among behaviorists and functionalists and ended with pioneering with the use of psychological research to validate psychoanalytic attachment theory, her career demonstrates important continuities.

From a historical perspective, it can often be seen that most psychological theories are based on some basic observations that are taken as **exemplary situations**. For Blatz, children playing with blocks or exploring the woods served as exemplary situations, while for Ainsworth the behavior of African children had a similar function. The choice of these exemplary situations is clearly influenced by popular ideas: in Blatz's time the emphasis on efficiency and industry, and in Ainsworth's days the renewed emphasis on emotions that arose during the sixties. Psychological research is also used to reinforce existing trends--as becomes clear in the debates on infant care and the different construction of the responsibilities and the feelings of mothers. The importance of gender in exemplary situations for theorizing and in the conclusions that should be drawn from this research has only been hinted at in this paper. These issues deserve more research.

Notes:

I wish to thank Jean Silver-Isenstadt and Lynne P. Snyder for helpful and stimulating comments on earlier drafts of this paper.

1. The article is: Jay Belsky, "The 'Effects' of Infant Day Care Reconsidered," Special Issue: Infant Day Care. *Early Childhood Research Quarterly* 3 (Sept. 1988)3: 235-272. Belsky appeared in talk shows to present his research even before his article was published, which caused dismay among his colleagues. Rebuttal appeared in the same special issue: K. Alison Clarke-Stewart, "The 'effects' of infant day care reconsidered," *Early Childhood Research Quarterly* 3 (1988)3: 293-318; and John E. Richters and Carolyn Zahn-Waxler, "The Infant Day Care Controversy: Current Status and Future Directions," Special Issue: Infant Day Care. *Early Childhood Research Quarterly* 3 (1988)3: 319-336. For an exploration of the public reactions to Belsky's article see: Ellen R. Shell, "Babes in Day Care," *The Atlantic Monthly* 262 (August 1988)2: 73-74; and Robert Karen, "Becoming Attached," *The Atlantic Monthly* (February 1990): 35-70. For later research on attachment see: Jay Belsky and Teresa Nezworski, eds., *Clinical Implications of Attachment* (Hillsdale, N.J.: Erlbaum, 1988) and current issues of *Child Development*.

2. For John Bowlby's theories see: John Bowlby, *Child Care and the Growth of Love*, abridged and edited by Margery Fry. 2nd ed., with two chapters by Mary D. Salter Ainsworth (London and Baltimore: Penguin Books, 1965); *The making and Breaking of Affectional Bonds* (London: Tavistock, 1979); *Attachment and Loss*, three volumes (New York: Basic Books, 1969-1980). Bowlby's first publication on this topic was in a report prepared for the World Health Organization: *Maternal Care and Mental Health: A Report Prepared on Behalf of the World Health Organization as a Contribution to the United Nations Programme for the Welfare of Homeless Children* (Geneva: World Health Organization, 1951). For an evaluation of this publication see: M. Ainsworth and others, *Deprivation of Maternal Care: A Reassessment of its Effects* (Geneva: World Health Organization, 1962). Other contributors to this volume were, among others: Robert G. Harlow and Margaret Mead.

3. The first results were published in Mary D.S. Ainsworth and Barbara A. Wittig, "Attachment and Exploratory Behaviour of One-year-olds in a Strange Situation," in: Brian M. Foss, ed., *Determinants of Infant Behaviour*, Vol. 4, based on the proceedings of the fourth Tavistock Study group on mother-infant interactions held at the house of the Ciba Foundation, London, September 1965 (London: Methuen, 1969). For a comprehensive overview see Mary D. Salter Ainsworth, *Patterns of Attachment: A Psychological Study of the Strange Situation* (Hillsdale, N.J.: Erlbaum, 1978).

4. The work of Benjamin Spock evoked similar reactions. See Nancy P. Weiss, "Mother, the Invention of Necessity: Dr. Benjamin Spock's *Baby and Child Care*," in: N. Ray Hiner and Joseph M. Hawes, eds., *Growing Up in America: Children in Historical Perspective* (Urbana: University of Illinois Press, 1985).

5. John Bowlby was a student of Melanie Klein, who developed the theory of object-relations. He was part of a faction within the psycho-analytic movement that emphasized the importance of relations, bonding, and attachment in the early emotional development of children, rather than investigating internal conflicts and drives. Other psycho-analysts in this group were René Spitz and Donald Winnicott.

6. For a biography of Blatz see Jocelyn M. Motyer, *The Nursery World of Dr Blatz* (Toronto: University of Toronto Press, 1991). His most important works were: William E. Blatz and Helen Bott, *Parents and the Pre-school Child* (Toronto: Dent, 1929); *The Management of Young Children* (New York: Morrow, 1930); William E. Blatz, *Hostages to Peace: Parents and the Children of Democracy* (New York: Morrow, 1940); *Human Security: Some Reflections* (Toronto: University of Toronto Press, 1966). These were books written for parents rather than for fellow researchers. More academic is: William E. Blatz, *The Five Sisters: A Study of Child Psychology* (New York: Morrow, 1938).

7. For a description of the research that was initiated during this period see: Johannes C. Pols, *The School as Laboratory: The Development of Psychology as a Discipline in Toronto, 1915-1955* (York University, Toronto: Master's Thesis, 1991) and *The World as Laboratory: Mental Hygiene Inspired Psychology as Alternative Investigative Practice. The Case of Toronto Psychology 1920-1956*. Paper presented at the 23rd CHEIRON Conference, Slippery Rock, Pa, USA, 20-23 June 1991. See also: Karl S. Bernhardt and others, *Twenty-five Years of Child Study: The Development of the Programme and Review of the Research at the Institute of Child Study, University of Toronto, 1926-1951* (Toronto: University of Toronto Press, 1951). For a more general description see: Teresa Richardson, *The Century of the Child: The Mental Hygiene Movement and Social Policy in the United States and Canada* (Albany: State University of New York Press, 1989), chapter 3 and 7.
8. See, for example, Helen Bott, *Method in Social Studies of Young Children* (Toronto: University of Toronto Press, 1933). Most observations were made in the University Nursery School. See Veronica Strong-Boag, "Intruders in the Nursery: Child Care Professionals Reshape the Years One to Five," in: *Childhood and Family in Canadian History* (Toronto: McClelland & Stewart, 1982).
9. Blatz proposed this view mainly in the advice literature on child-rearing or child management, as he called it, that he wrote. See, for example, William E. Blatz, "Discipline vs. Corporal Punishment," *Childhood Education* 5 (1928)3: 144-149; "The Mental Hygiene of Childhood," in: *International Congress Mental Hygiene, Washington, D.C., 1930. Proceedings* (New York: American Foundation for Mental Hygiene, 1930); *The Habit of Success* (Chicago: Visiting Nurse Association, 1930); *The Importance of Failure* (Iowa: State University of Iowa Press, 1934); *Human Needs and How They Are Satisfied* (Iowa: State University of Iowa Press, 1934).
10. William E. Blatz and Dorothy A. Millichamp, *The Development of Emotion in the Infant*. Child Development Series no. 4 (Toronto: University of Toronto Press, 1935), 6.
11. William E. Blatz, S.N.F. Chant, and Mary D. Salter, *Emotional Episodes in the Child of School Age*, Child Development Series, no. 9 (Toronto: University of Toronto Press, 1937), 3, 9.
12. *Ibid*, 39.
13. Watson dedicated a whole chapter on the dangers of too much mother love. John B. Watson, *Psychological Care of Infant and Child* (New York: Norton, 1928), 64-77.
14. Blatz and Millichamp, *Development of Emotion*, 20.
15. Blatz, Chant, and Salter, *Emotional Episodes*, 11, 19.
16. Blatz and Millichamp, *Development of Emotion*, 22.
17. See William E. Blatz, *Human Security: Some Reflections* (Toronto: University of Toronto Press, 1966).
18. This change reflected similar changes in the psychological advice literature in general. See A.M. Sulman, "The Humanization of the American Child: Benjamin Spock as a Popularizer of Psychoanalytic Thought," *Journal for the History of the Behavioral Sciences* 9 (1973): 258-265; Kathryn T. Young, "American Conceptions of Infant Development from 1955 to 1984: What the Experts are Telling Parents," *Child Development* 61 (1990)1: 17-28.
19. Mary D. Salter, *An Evaluation of Adjustment Based on the Concept of Security*. Child Development Series no. 18 (Toronto: University of Toronto Press, 1940), 7-8, 43.
20. *Ibid*., 31.

21. *Ibid.*, 41.

22. *Ibid.*, 45.

23. *Ibid.*, 43, my emphasis.

24. See, for example, his 3-volume work *Attachment and Loss* (New York: Basic Books). The positive reaction of the psycho-analytically trained John Bowlby towards these ethological theories is interesting in itself. Ainsworth's positive reaction can partially be explained because of her education: the ethologists, like Blatz, emphasized extensive observation in a natural environment, rather than laboratory experiments. In their 1989 APA Award Recipient Address, Mary Ainsworth and John Bowlby referred to their approach as an ethological approach to personality development (see *American Psychologist* 46 (1989)4: 333-341).

25. See Donna Haraway, "Metaphors into Hardware: Harry Harlow and the Technology of Love," chapter in: *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (New York and London: Routledge, 1989).

26. See M.D. Ainsworth and John Bowlby, *Research Strategy in the Study of Mother-Child Separation* (Paris, Courrier de la Centre International de l'Enfance, 1953).

27. See Mary D. Salter Ainsworth, *Infancy in Uganda: Infant Care and the Growth of Love* (Baltimore: Johns Hopkins Press, 1967).

28. Mary D. Salter Ainsworth and John Bowlby, "An Ethological Approach to Personality Development," *American Psychologist* 46 (1991)4: 331-341.

History in Psychology *

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Summary of a paper to be presented at the eleventh annual conference of Cheiron-Europe, Groningen, The Netherlands

☆☆☆

At one point John Anderson had developed so many versions of Adaptive Control of Thought (ACT) that he lost count and simply called it ACT* (pronounce: ACT-Star). In the same manner, this paper is called History in Psychology*. Every autumn over the past five years or so, our host in Groningen, Piet van Strien, managed to lure Dutch historians of psychology to give talks at his department and to discuss historiography. The present paper results from his tireless efforts - and mine.

Main Currents in Psychology

Many authors have drawn attention to a phenomenon which may well seem typical of psychology, namely the going in and out of the discipline of key-concepts and ideas.

It will be argued that in this process clusters may be perceived, which will be called Main Current in Psychology (MCP). MCPs denote clusters of concepts that can be demonstrated to have played a foundational role in psychological theory-formation over relatively long periods of time. Many psychologists, approaching the development/history of the field from equally many angles all ended up with basically similar MCPs (e.g. Allport, Gergen, Haselager, Kimble, Leahey, Wertheimer, a.o.).

Conceptual Instability of Psychology

According to some of these authors, MCPs may provide the conceptual means by which order can be created in the apparently chaotic development of the field.

At the same time, however, the historical zig-zag course that can be seen to occur between the MCPs themselves forms an example at a higher level of aggregation of the very process that they are hoped to order.

Pulling together the strands of the MCPs and the dynamic relation between them, it will be suggested that in this zig-zag course a characteristic feature of the discipline may be perceived: From a conceptual point of view psychology seems less established than many of the older sciences.

Psychology's past is part of its present

Concepts and even entire MCPs going in and out the discipline would seem to entail that the past may at any moment crop up again and thus become relevant to (parts of) the field as currently pursued.

In philosophy the same situation obtains to an even stronger degree. In that discipline, history is accorded an inherent position. It will be argued that in view of its conceptual instability psychology is comparable to philosophy. Hence, in psychology history is also to be seen as an essential part of the field. Put somewhat stronger: in psychology the past is part and parcel of the present.

Internalism for psychologists

The special brand of presentism which is defended in this paper seems to require a particular historiographical approach. An approach geared to psychology's key-concepts and ideas as distinguished from factors and events that are external to the field and hence outside the control of active researchers. In other words, besides presentism an internalistic historiography (history of ideas) is also maintained.

How about historians?

It will be pointed out that this speaker does not intend to defend history of ideas as the only game in town. Rather, what would seem called for is the systematic examination of the relative merits of the old and the new historiography. There is some reason for uncertainty, however, as to whether historians of psychology will be prepared to take up that issue.

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Title:
Research Development in Swedish Psychology 1968-1990

The poster^{*} presents qualitative and quantitative data collected through bibliometric procedures, interviews and documentary research.

The first section discusses bibliometric research methods in the wider context of sociology of science, and various models for its utilisation. After settling for the interactive model proposed by Weingart, Sehringer & Winterhager (1989), the case study proceeds step by step, in the first part focusing entirely on Swedish psychology. Data is presented concerning the growth and differentiation of academic psychology as reflected in its institutionalisation, reflected in the creation of professorial chairs, denomination of new research specialities, etc. Then, the growth of a specific scientific community of psychologists is traced through the output of Ph.D's during the period.

A sample of their dissertations is collected at five-year intervals, and this sample is analysed more closely. Following the Psychological Abstracts classification categories, research areas are identified; theory and method choice; whether the research is basic or applied, to name the more important variables. The aim of this is to empirically examine claims made about the character of Swedish psychology, its major research orientation and whether this has changed over time or not.

The second part consists of a bibliometric study of the general research development in international psychology. The database PsycINFO has been utilised as a source for collecting data concerning the growth of psychological research in general and the development of international research orientations. The distinctly Swedish contributions are identified and the section closes with a comparison between international and Swedish research, as well as

*The contribution was originally written as a poster session.

a study of the impact Swedish psychology has had via searches in the Social Sciences Citation Index. There is also a discussion of the particular biases in the sources used, and a critical assessment of the strengths and weaknesses of bibliometric data.

In the third part, data is presented concerning how the funding situation has developed in Sweden. The major research councils' financing of psychological research is analysed, and interpreted in the light of general science and research policy goals affecting psychology. The aim is to discuss whether there is any discernible change within the discipline towards more applied research within specific areas that have been associated with social policy interventionism, and if psychology in that respect has come to serve the role as purveyor of expert knowledge to a greater extent than previously.

The fourth and final part consists of data gathered through interviews with professors of psychology in Sweden. It consists of an attempt to connect the bibliometric data on psychology to what might be termed the "insider's view" of what the discipline is like, and how academic research has developed during the period of interest. Other things discussed are: the relationship between academic and practical psychology, explicit and implicit scientific ideals and thoughts on the future of psychology as research area.

In this way, the poster attempts to present a complex description of scientific psychology in Sweden, its links to international research and its interplay with general research and science policy.

The Tavistock Effect: Notes on the penetrative powers of psychoanalytic expertise

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ABSTRACT

Many authors have noted the rise, in most advanced liberal market democracies, of a 'therapeutic apparatus', entailing ways in which conduct is acted upon, regulated, governed - by others and by ourselves - in a variety of locales (home, factory, school, hospital) - in broadly psychoanalytic terms. In this paper I suggest some ways in which we might understand this phenomenon in terms of connections between ways of *problematizing* events, activities, difficulties and distress in terms of an inner psychological world and an intra-psychological realm of human relations, ways of *diagnosing* by rendering a domain intelligible in terms of languages, judgments and diagrams of 'psy' (psychoanalysis; social psychology), ways of *remedying*, or seeking to remedy, via a calculated project of action upon these psychological and inter-psychological realms and relations. I exemplify this through an investigation of some of the activities of the Tavistock Clinic and the Tavistock Institute of Human Relations (which I collectively term 'the Tavi'). I suggest that the 'transfiguration' of professionals plays a key role in the spread of therapeutics, and that the mechanisms of 'training' developed at the Tavi have a particular potency in this regard. I illustrate this through the development of 'Group Relations Training' in the 'Leicester Conferences' run by the Tavi since 1957. Through its training schemes, and the transfiguration of personalities which they produced, the Tavi was to overcome its limitations of time and space and to 'reticulate' itself into a widely dispersed realm of effects.

**THE TAVISTOCK EFFECT:
NOTES ON THE PENETRATIVE POWERS
OF PSYCHOANALYTIC EXPERTISE**

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The Tavistock Effect: Notes on the penetrative powers of psychoanalytic expertise¹

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Many authors have noted the rise, in most advanced liberal market democracies, of a 'therapeutic apparatus', entailing ways in which conduct is acted upon, regulated, governed - by others and by ourselves - in a variety of locales (home, factory, school, hospital) - in broadly psychoanalytic terms. In this paper I suggest some ways in which we might understand this phenomenon. I exemplify this through an investigation of some of the activities of the Tavistock Clinic and the Tavistock Institute of Human Relations (which I collectively term 'the Tavi'). I suggest that the 'transfiguration' of professionals plays a key role in the spread of therapeutics, and that the mechanisms of 'training' developed at the Tavi have a particular potency in this regard. I illustrate this through the development of 'Group Relations Training' in the 'Leicester Conferences' run by the Tavi since 1957.

3 dimensions By therapeutics I mean the connections established over the last 50 years, between: (i) ways of *problematizing* events, activities, difficulties and distress in terms of an inner psychological world and an intra-psychological realm of human relations (eg: marriage, productivity); (ii) ways of *diagnosing* by rendering a domain intelligible in terms of languages, judgments and diagrams of 'psy' (psychoanalysis; social psychology); (iii) ways of *remedying*, or seeking to remedy, via a calculated project of action upon these psychological and inter-psychological realms and relations (eg. Marriage guidance; management consultancy).

The life history of the Tavi is an exemplary condensation of these transformations. From the 1920s, work at the Clinic pioneered the application of insights from psychoanalysis and dynamic social psychology to child rearing, marital problems, the treatment of disturbed adults and children, and the training of professionals. During the Second World War, members of the Tavistock, as well as individuals who would later become affiliated to it, made key contributions to the war effort in the selection and training of troops, and in the rehabilitation of shell shocked soldiers and returning prisoners of war. These experiences provided the basis for important advances in psychoanalytic and social systems theories and their application to problems of group life. After the war, the Institute developed and extended such work, collaborating in a novel way with government and industry in the analysis and attempted resolution of problems of productivity, industrial relations and organizational functioning. In the 1950s, the Institute developed group relations training procedures, and undertook major long-term action research projects that sought to democratize and humanize working life in a

¹ This paper draws upon collaborative research with Peter Miller on the social and intellectual history of the Tavistock Clinic and Tavistock Institute of Human Relations, supported by grants from the Leverhulme Trust and the Tavistock Institute of Medical Psychology.

number of European countries. Meanwhile, the Clinic extended its work through innovative techniques of family therapy and marriage guidance, new methods of adult and child therapy, and through training general practitioners, social workers and others in the 'helping professions' in the psychodynamic aspects of their work.

In my analysis of these events, I differ from two other approaches to the relations of psychoanalysis and society. The first distinguishes 'sacred' and 'profane' uses of psychoanalysis, arguing that psychoanalysis proper has been marginalised, and that it has been taken up only in simplified and banal ways. The second also tends to divide a pure and pessimistic core of psychoanalysis from an optimistic and socially malleable psychotherapy, but links the spread of the latter to general cultural transformations such as the breakdown of community and the retreat into privacy. I prefer to analyse at a more modest, mundane and technical level. How has human conduct has been problematized in diverse sites and practices (divorce rates, productivity or lack of it)? Through what theoretical codes have these problems been rendered thinkable and manageable - simultaneously further problematizing that which had previously seemed unproblematic? How have some psy knowledges acquired the capacity to make themselves technical and hence generate effects? How has a therapeutic expertise of human conduct been born?

It is this upon latter question that I focus here. The success of the Tavi was to find a way in which a very small and localised institution could develop a set of technologies for the expert conduct of human conduct, and generate an 'effect' that would spread very widely and link - or 'reticulate', to use the Tavi term - a whole variety of activities into a therapeutic territory. The Tavistock answer to the question 'how can the few change the many?' was through training the diverse 'professionals' that came to inhabit the devices and practices of the post-war 'welfare state'. Training would not be a matter of teaching professionals, but of transfiguring them, remaking their personhood at a very fundamental level, so that the ways in which they experience the world and their own desires would be indistinguishable from the Tavi's own. Power, as Michel Foucault has suggested, is best seen as *action on the actions of others*. Such power implies freedom: it works best through shaping the ways in which others construe and enact their freedom. To multiply yourself through transfiguration relieves you of the task of having to calculate for all eventualities. Transfigured individuals themselves will apply, innovate, shape, develop, extend and refashion your programmes in a multitude of multitude of sites and in relation to a multitude of local problems.

Training had been offered by the Tavistock Clinic from very early on. By 1970, there was training of social workers, probation officers and others in work with 'marital problems', which was undertaken by the Family Discussion Bureau. There was training of general practitioners, through the general practitioners seminars begun by Michael Balint in 1953, and continued, after Balint's

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retirement, under the direction of Robert Gosling, serving some fifty general practitioners at any given time. There were regular seminars for medical officers of health, chiefly in the Children's Department. There was the Introductory Psychotherapy Course for psychiatrists. There were several different kinds of courses for probation officers. There were courses for dermatologists, for Borstal assistant governors and prison officers, for medical officers of health and health visitors in community psychiatry, There were external courses for educational psychologists and for the staff of schools for maladjusted children. There were the regular 'Leicester Conferences' run by Tavi staff from 1957 onwards, attended by professionals from industry, the civil service, the churches and many others.

The central device in the training developed by the Tavistock was 'the group'; this was the central technique of the group relations training in the Leicester Conferences. What was 'the group' and why was it so potent? The answer lay in the way it simultaneously re-described a whole variety of activities, and could transform the professionals that had authority over those activities. By 1930s, most social psychologists had rejected conception of group mentality and of the psychology of the crowd; the group was broken down into constellation of individual attitudes. But over the 1940s and 1950s, the group as an entity with its own dynamics was re-invented, by Lewin and by Sherif in the USA and by Bion and others in wartime UK, with the invention of the leaderless group and the therapeutic technology. In the post war Tavi, Bion developed a particular way of understanding group dynamics, based on the use of the 'unorganized group'. This was a group - of patients or of staff - whose members were there for no purpose except to study the group itself. These groups acted as laboratories for the reproduction, intensification and isolation of group phenomena. Bion concluded that "the group provides an intelligible field of study for certain aspects of individual psychology, and in doing so brings into prominence phenomena that appear alien to an observer unaccustomed to using the group" (Bion, 1961, p.134).

The power of Bion's argument lay, in part, in the simplifying vocabulary that it provided for grasping the vectors and valencies between persons in groups, a highly transferrable vocabulary that could spread itself widely to make group relations thinkable, describable and hence manageable. The group has something that draws individual into its sway. Bion termed these *basic assumptions*. At any one time it appears to have a 'basic assumption' about the kind of group it is. Under the sway of this assumption, it acts as if there could be no other possible reason for its existence than this: the group assumes that it is there for *pairing* (*baP*); to protect itself against threats and dangers by fight or flight (*baF*); to depend upon another, a leader, who it desperately wishes to provide it with security, whilst resenting for placing it in a position of dependency (*baD*). This coding of group life could be re-interpreted in terms of the psychology of the individual - for the psychology of the individual is itself a kind of internal representation of relations amongst a number of entities or, as

Bion termed them using Klein's vocabulary, part objects. The group evokes very primitive psychotic anxiety in its members that they are damaging the group or that the group is devouring them. These anxieties are forced out into the open in the group studying itself, whether the group is perceived as that which investigates the individual or that which is investigated by the individual. In such circumstances defences come into play: for example the splitting of good and bad parts, and the projection of these respectively onto the analyst and the group. The ultimate sources of all group behaviour lie in the primitive anxieties of part object relationships. The basic assumptions were, essentially, secondary formations designed to hold at bay the anxieties which the group itself evoked. And the fact was that individuals were *always* members of groups, even if the form of their membership was to deny it.

This way of conceptualising and instrumentalising this dynamic set of relations within and between psyches and persons was to be developed in various ways: a *clinical* model for working with patient groups; a *consulting* model, for working with work groups in an action situation, such as those which Elliot Jaques worked with at Glacier Metal; and a *training* model, that had as its task the training of those who attended in group dynamics. Such groups were to become the mechanisms *par excellence* for the transfiguration of professional personas.

The first application of group relations training to professional life was developed in the Human Relations Training provided in the National Training Laboratory in Group Development. This was founded by Lee Bradford and his colleagues, one of the halves into which Kurt Lewin's MIT group split after his death. The Training Groups, or T groups, were associated with the Research Center for Group Dynamics at the University of Michigan, and were run from Bethel in Maine from the late 1940s. They spread to allied institutions such as the Western Training Laboratory in Group Development of the University of California and institutes located at Arden House, New York City. T groups sought to studying the group itself, with experts explaining personal experiences and behaviours while the group was in progress. The task of the group was to study and learn from its own tensions. By 1959 over a thousand people had passed through the three week summer sessions at Bethel, including executives from industry and government; members of the fighting services, the churches, trade unions and educational organizations; community leaders and research workers.

In the UK, attempt to start similar groups by Tavi did not fare well, attracted 'patients in disguise' and produced distress in participants. After various attempts a solution emerged: to give the group a project, a task to which it should address itself. The consultant could then interpret the ways in which the group sought to carry out its task, and help it see the task in terms of its own behaviour. This would be the basis of the technology to be promulgated through the Leicester Conferences. It was to find its locus of demand in the post war drive for productivity, co-ordinated

by the European Productivity Agency. In 1956, the EPA undertook some projects to engender interest in Europe in improved methods of industrial training. The Tavistock was involved, in collaboration with NTL, and in association with the British Institute of Management, the Industrial Welfare Society, and with the advice of a steering committee appointed by the Ministry of Labour and National Service. The first Group relations Training Conference was held at Leicester in 1957. It lasted for two weeks and attracted forty five participants. The central mechanism for transfiguration was the small group, explicitly based on Bion's methods. The group met "without an 'external' task to be done, but with the specific task of examining the kinds of feelings and attitudes that arise spontaneously, these feelings and attitudes being those which each individual brings to any group situation, or which develop within it independently of whatever the external task may be" (Sutherland, in Trist and Sofer, Appendix 1). The group struggled to find and explore appropriate topics and through them came into contact with their own relationships: their unwitting resistance to certain types of understanding was analysed and interpreted by the consultant and the group. Most notably these feelings concerned leadership: the group feels dependent and frustrated by the absence of the usual leadership behaviour; it feels angry and resentful at the consultant's obduracy; it engages in competition for alternative leaders; it takes flight from the absence of leaders; it breaks down into pairings. Through all this, the Consultant remains objective, the very model of a transfigured persona, pointing out the groups expectations and assumptions, especially in regard to its leader.

Participants reported that, whilst the approach was not easy to apply, *they themselves had been improved* - self-insight, self control, a better appreciation of the nature of forces in groups, of the emotional aspects of groups and of one's own "personal equation" - more likely to "mobilize the positive forces which lie within them" (Sutherland, in Trist and Sofer, 1959, p.60). Over the next twenty years there were to be various technical developments and fissures within the original groups relations movement - the single task group, the inter-group experience, the two task group, the large group. Within the orthodox model, fostered by A. K. Rice, the focus remained on the single task the central question was one of authority and the problems encountered in its exercise: to assist the individual "to struggle to exercise one's own authority, to manage oneself in role and to become less a captive of group and organizational processes": a transfiguration of the persona of the professional, combined with an ethicalisation of the exercise of authority. The potency of this technology was its promise to make one simultaneously a better person and a better manager.

A national and international network of group relations training institutions were rapidly established deploying what was termed 'the Tavistock Model'. Margaret Rioch of the Washington School of Psychiatry set up an American version of the Leicester Conference with Rice's assistance: after Rice's sudden death in 1969, she named it the A. K. Rice Institute. Within ten years it had

generated six other local nodes throughout the United States including GREX in San Francisco and the Institute for Applied Study of Social Systems in New York. In Canada, similar conferences are organized by the Rosehill Institute of Human Relations; in Britain, analogous events were run by the Grubb Institute, the Chelmsford Cathedral Centre for Research and Training, the Centre for the Study of Group and Institutional Relations at Bath University and, after the retirement of Jock Sutherland to Edinburgh, the Scottish Institute of Human Relations.

The construction of the construct of the group was to force into existence a new domain of reality, a space for thought and intervention that was no longer confined to the consulting room. It existed wherever humans were gathered together - in family, school, hospital, prison or factory. It was there whenever a meeting was convened to take a decision, a seminar sought to discuss an argument, a case conference sought to agree a course of action. It could be recreated anywhere where one could find six people, a room, and a consultant. And it could offer itself to a potentially limitless constituency - not those who were sick and required psychotherapy, but all those who had to operate in groups to pursue personal and professional projects - in other words, everyone.

Through its training schemes, and the transfiguration of personalities which they produced, the Tavi was to overcome its limitations of time and space, to shape and transform the very notion of what it was to be a member of a 'helping profession'. Through exploiting the transfigurative possibilities of the group, along a number of different dimensions, the Tavi was able to make itself not simply one organization amongst others, but the fulcrum of a proliferating complex of thought and action that would enmesh all those different varieties of professional expertise operating within the networks of welfare. The lure of these groups was not merely that they contributed to professional advancement. They provided a matrix within which all the mundane tasks engaged in by professionals of human conduct could be given a new simplicity, coherence and dignity. They made the exercise of power over others, the conduct of conduct, itself a therapeutic activity. In promising what Michael Balint termed, in an analogous context, the considerable though limited transfiguration of personality necessary for those who would use psychoanalysis in their own practice, they lashed together the desire for professional development, the wish for an ethical justification for professional activity, and the desire of each professional of the soul to culture their own selves.

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Mental representation: the history of a concept.

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*Let in 1964
in Utrecht
by Langeveld!*

Abstract The paper consists of two parts: 1. an account, based on letters of William and Clara Stern as well as on material from the Archives of the Jewish National and Hebrew University on Givat Ram in Jerusalem, on Stern's lecture tours in the Netherlands; 2. an analysis of the reception of Stern's work in the Netherlands. It appears that from 1920, when the first lecture tour took place, there was a strong interest in all facets of Stern's work, and especially in his philosophy of personalism and personalistic psychology. For the period 1920 to 1936 the relationship between Philip Kohnstamm (1875 - 1951), professor of education in Amsterdam, and William Stern (1875 - 1938) is singled out for special emphasis. For the years 1937 to about 1960 the discussion of Stern's work by Martin Langeveld (1905 - 1989), student of Kohnstamm and admirer of Stern, is highlighted.

William Stern in the Netherlands

by Wilfred Schmidt, University of Alberta

The paper consists of two parts: first, Stern's lecture tours in the Netherlands, and second, the reception of Stern's work in the Netherlands.

Stern's lecture tours

① When I searched for evidence regarding William Stern's lecture tours in the Netherlands, I was surprised to find that the first such tour took place as late as 1920. I was surprised because throughout his academic career, which started in 1898 at Breslau University, Stern had been very much in demand as a speaker before diverse audiences. Before the end of the First World War he had given lectures not only in numerous German cities, but also in Austria (Vienna), Rumania (Bukarest), and Bulgaria (Sofiya)¹, and as early as 1909 he had given a series of lectures in America, when Clark University bestowed an honorary degree on him. I was also surprised because even before Stern had become Ordinarius in Hamburg in 1916, he already had much to offer in many areas of psychology, education, and philosophy: differential psychology, theory and practice of intelligence testing, psychology of testimony, language acquisition, psychology of early childhood, studies of school children, educational reform, and the philosophy of critical personalism.

But late as the first visit to the Netherlands may have been, from Stern's point of view it came at just the right time and was an exhilarating experience. This is what he wrote to his friend Jonas Cohn, professor of philosophy in Freiburg, on November 28, 1920:

I myself have been travelling quite a bit. - - - Towards the end of September two "pedagogical weeks" in Kassel and Hannover, where work groups for educational psychology have been formed and towards the end of October 10 days in - Holland That was a particularly nice time. I was invited by several scientific (wissenschaftliche) societies, gave lectures on psychology, pedagogy, and philosophy at both universities in Amsterdam and at Groningen University, found an amazingly intense interest in German Wissenschaft in general, and in (the philosophy of) personalism in particular, experienced the most generous hospitality, and - in radiant sunshine - came to know land, people, and art.

② Stern attended the International Congress of Psychology held in Groningen in 1926, and read a paper on personalistic psychology². The next lecture tour in the Netherlands that we know of³ was in March 1928. In a postcard to Cohn from Groningen and a letter from Hamburg⁴ Stern speaks of this tour. He had again lectured in Amsterdam and in Groningen, seven lectures in all. But apart from mentioning how hard he worked, how much he enjoyed the trips by car to the seaside, old towns and to museums, and again in radiant sunshine (!), he says nothing more.

④ From a printed leaflet that I found among the Stern papers in Jerusalem I know that Stern was on a lecture tour again in 1929. The leaflet contains an invitation to four lectures,

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issued by the **Genootschap tot Bevordering der Wijsbegeerte. Gevestigd te Utrecht. Sekretariaat Burgemeester Reigerstraat 78.** The program reads as follows:

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January 27: The philosophy of personalism. Den Haag, in the Realgymnasium, Nassastraat 112.

January 28: The science of the human person. In the Klein - Auditorium der Rijksuniversiteit Utrecht.

January 29: Modern child psychology; its tasks, its methods, and its influence. In the Groot - Auditorium der Rijksuniversiteit, Leiden.

January 30: The problem of the testimony of witnesses. In the German school, Rotterdam.

⑤ In January, 1933, just before Hitler came to power, Stern was in the Netherlands again⁵. The **Nieuwe Rotterdamsche Courant** of January 22, 1933 contains a report on a well attended and much appreciated lecture given by Stern on the topic of "The child in the troubled family"; it names the **Rotterdam committee for lectures on education** as the body responsible for the invitation, and Dr. O.G.M. de Vos as the person (a woman) who, in the absence of Dr **Johan van der Spek**, introduced and thanked the speaker. It is also clear from the report that Stern was on a lecture tour, but no details of the itinerary are given.

⑥ The next visit to the Netherlands that I could trace was in November, 1933. The date is important. During the summer semester already Stern had been barred from lecturing in Hamburg University, although he was finally and prematurely pensioned off only on October 31, 1933⁶. In a letter to Cohn, (December 19, 1933), Stern names colleagues, who had found positions, albeit only temporary ones, in England and the United States of America: Ernst Cassirer, Heinz Werner, Max Wertheimer, Kurt Lewin. He mentions that on his visit to Holland in November he himself had made contacts that would probably lead to a number of lecturing engagements, and that the advantage of being in Holland rather than in England or the U.S.A. would be that he would be able to lecture in German. He also writes that he and his wife plan to be in Den Haag early in January 1934 for a few months, returning to Hamburg in the spring, but not knowing what would happen thereafter. Then in the final paragraph of the letter he mentions with great satisfaction that during his November visit he had found a publisher - the most prestigious publisher in Holland, **Nijhoff** - for his "**Allgemeine Psychologie**", which he hoped to complete in the next few months.

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↓ The Sterns moved to Scheveningen (den Haag), Pellenaerstraat 27 early in January 1934. In a letter to Cohn (February 21, 1934) Clara Stern speaks of the steady progress of the "**Allgemeine Psychologie**", and of the lectures her husband is giving in a number of places. Although she does not name the places, we know from other sources⁷, that at least six of these were given in den Haag, one in Amsterdam, and two in Amersfoort. The topics of these lectures included psychology of testimony and sexual abuse, psychology of the school-aged child, memory, aspects of the philosophy of critical personalism and the psychology of the person.

As planned the Sterns returned to Hamburg in the spring, moving into a new, more modest apartment, in keeping with their reduced income. On July 28, 1934 they boarded the City of Norfolk

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steamer and were on their way to America, to Duke University in North Carolina, where William Stern⁴ had been offered a visiting professorship until May 1935, and Stern expressed the hope that he would be "back home" ("daheim")⁸ again in June 1935.

The Sterns did return, as planned. According to a letter to me by his daughter, the late Eva Michaelis- Stern, her father at this point had no intention to leave Germany permanently. She writes that in the summer of 1935, while she was attending a Zionist congress in Luzern, Switzerland, and her parents were holidaying nearby, she used all the persuasive powers she could muster, to convince her father that he was in real danger, and that the time had come to leave for good. William Stern gave in to her pleas. He travelled back to Holland, while Clara took the train to Hamburg to collect their remaining possessions, including all Stern's books and papers, to ship them to America, where he continued to teach at Duke University, and, by invitation, occasionally also at Harvard and other universities.

In June 1937 the Sterns arrived in Holland again, this time to assist their eldest daughter, Hilde, and her two children to emigrate to the United States. Hilde had just completed a two year prison sentence for a political crime (giving shelter to "enemies of the state"). Her children, having been refused permission to attend a Gymnasium in Germany, had already spent more than two years in a Quaker school in Holland⁹. What is amazing is that during this short stay in Holland William Stern still found time to plan a new book. In the Archives in Jerusalem there is a five-page neatly typed outline of a book Personalistik von Raum und Zeit typed by Clara Stern on 26.7.37, with the note added: "written during his stay in Holland, in a tiny place between forest and sea, where he lived alone, while I was busy in Germany".

Reception of Stern's work

The many invitations that Stern received to lecture in the Netherlands attest to a great deal of interest in his work during his lifetime. Let us now look a little more closely at the reception of his work during that time and up to the 1960s.

The sources I quoted on Stern's lecture tours give no indication of the persons he met. This changes when we trace the response to Stern's ideas by looking at the journal **Paedagogische Studiën**, which described itself as "Tevens orgaan van het Nutsseminarium voor Paedagogiek te Amsterdam, van de Vereniging voor Paedagogisch Onderwijs aan de Rijksuniversiteit te Groningen en van het Dr. D.Bos-Fonds te Groningen"¹⁰. In the table of contents of volume 1 (1920) there appeared the names of H.J.F.W. **Brugmans** (1884 - 1961), professor in Groningen, who had studied under Stern (and Claparede) in 1913-1914, and F.J.M.A **Roels** (1887-1962), professor in Utrecht, who was inspired by Stern's concept of "unitas multiplex"¹¹, and that of Dr. **Johan van der Spek**, whose name came up in the report of Stern's lecture in Rotterdam in 1933. In volume 1 there also appeared the first part of **Kohnstamm's** inaugural address on the place of pedagogy in the university; in it he refers to William Stern and Jonas Cohn on "Weltanschauung"¹².

The relationship between Philip Kohnstamm and William Stern deserves particular attention.

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Kohnstamm (1875 - 1951) was a professor of thermodynamics, who turned his attention increasingly⁵ to education and philosophy. On his initiative the "Maatschappij tot nut van het Algemeen" had founded the "Nutseminarium voor Paedagogiek" in 1919, and he had accepted the directorship of the Nutsseminarium and the professorship of education that was created¹³. At the time of Stern's first visit in 1920 Kohnstamm and his associates in the Nutssemanrium conducted research in areas such as selection of elementary school pupils for admission to high school, for which Stern had worked out procedures in Hamburg.

Over the years Kohnstamm published five reviews of books by Stern in **Paedagogisch Studiën**, and in 1935 a review of his **Allgemeine Psychologie**, which apparently was not reviewed by anyone else in Holland¹⁴. In 1931 Kohnstamm contributed an article to the **Festschrift** for Stern, the only Dutch psychologist to do so¹⁵.

Kohnstamm recognized the importance of Stern's work, but he was also critical. He is very positive about the fourth edition of Stern's book on early childhood, because of the increased emphasis on personalistic interpretation, but in a review of Charlotte Bühler's **Kindheit und Jugend**¹⁶ he judges this to be a better example of what a "personalistic psychology" should look like. In his review of **De pubertijdsleeftijd**¹⁷ he does not accept Stern's sharp distinction between eroticism and sexuality. In his review of **Allgemeine Psychologie vom personalistischen Standpunkt**¹⁸ Kohnstamm recognizes that Stern had performed a monumental task in reinterpreting the findings of general psychology, but was disappointed that the book had taken the place of what Stern had promised in the first part of the **Studien zur Personwissenschaft** of 1930, namely a psychology of the person ("personalistische Psychologie"). And he could not fathom how in speaking about the person Stern could disregard religious experience, formation of conscience, and Freud's account of id, ego, and superego, all issues which Kohnstamm saw as central to a personalistic psychology. But that the book would remain important to psychologists for a long time, that he did not doubt. We ask now: **did Stern's work live on in the Netherlands?**

For an answer we look at three Dutch textbooks of psychology. **Beknopt Leerboek der Psychologie**¹⁹ by Bigot, Kohnstamm, and Palland was used widely, and over many years, in teacher education: In the 7th. edition, 1961, the references to Stern outnumber those to any other author (Stern 15, Karl Bühler 9, Freud 8, Kohnstamm 6, Piaget 5), and refer to almost all aspects of Stern's work; there is also a five page exposition of Stern's personalistic psychology. **Inleiding in de psychologie**, edited by Langeveld, with chapters by Kohnstamm, C.F. van Parreren, J. Linschoten, was intended as a more "academic" introduction to psychology. In the third edition, 1961, the references to Stern come in third place (Freud 17, Ach 15, Stern 13 etc.), 6 of them in the chapter on "general functions" by Linschoten and 6 in the historical section by Palland. There is an eight page exposition and evaluation of Stern's personalistic psychology. The third book is **Inleiding tot te studie der paedagogische Psychologie van de middelbare-schoolleeftijd** by M.J. Langeveld. The first edition appeared in 1937, and the edition from which I now take my information is the sixth, published in 1957. The references to Stern are exceeded only by those to Kohnstamm (40

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and 45 respectively), and these far exceed those to any other authors (Charlotte Bühler 24, Otto Selz 6 22, Klages 21 etc.). But counting the number of references does not tell the whole story.

What makes Langeveld's book especially interesting is that he devoted three chapters to the topic of intelligence, and one to the psychology of thinking, and that 16 of the references to Stern are in these chapters. Langeveld (1905 - 1989) had studied with Kohnstamm and on a study tour in 1931 had attended lectures by Stern²⁰, and - as I know from many personal conversations with him - he admired Stern, but he was critical of Stern's use of intelligence tests in the selection of elementary school children for admission to high school. Ironically, his criticism sounds very similar to Stern's criticism of American mass testing of intelligence²¹. To appreciate Langeveld's criticism one must know that the research of Kohnstamm and his associates in the Nutsseminarium led them to move away from using a combination of intelligence tests (as measures of a "potential") and achievement tests (as measures of "learned content"), and to place the emphasis on teaching all children strategies for thinking. The work of Otto Selz and of Karl Bühler on the psychology of thinking inspired them to identify strategies of thinking required in the classroom. The aim was "learning to think" (leren denken) and thus developing intelligence.

Langeveld also discusses very thoroughly some basic concepts of Stern's differential psychology, but this is hardly the occasion for elaborating on this in detail²². It is relevant to note, however, that around this time A.J.M. Chorus, professor in Leiden, also refers to Stern's differential psychology²³.

Van Strien has shown that in the Netherlands the wholistic approach in psychology, which became dominant in the 1920s (the International Congress in Groningen in 1926 reflecting this), continued until the early 1960s, when a radical shift occurred²⁴. While Stern was never dogmatic with regard to methodology, his approach is best characterized as wholistic. It is certainly striking that in a history of applied psychology in the Netherlands, which was published in 1986, no mention is made of William Stern²⁵. In Germany today there is a revival of interest in William Stern, and to a limited extent that applies to North America too²⁶. I end with a question: what is William Stern's future in the Netherlands likely to be?

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I obtained information on his lecture tours and workshops both from material in the Archives of the Jewish National and Hebrew University Archives on Givat Ram in Jerusalem; from letters of William Stern to Jonas Cohn, professor of philosophy in Freiburg. The letters are in the Jonas Cohn Archive (Director Professor Dr. J. Löwisch) in Duisburg, Germany.

²⁰Personalistische Psychologie. In Proceedings of 8th International Congress of Psychology held in Groningen, 1926. Jac. van Dael in his *Geschiedenis der empirische psychologie* of 1929 refers to Stern's paper and devotes 7 pages to the personalistic psychology of William Stern (p.162 - 169)..

²³Considering the excellent reception he received in 1920, one would expect less than an eight year interval before another lecture tour took place, but none of the sources I had

at my disposal, including the journal **Paedagogische Studiën**, which sometimes published notices of forthcoming lectures, mentioned any lectures by Stern before 1928.

⁴Postcard presumably March 5, 1928. Letter dated March 25, 1928.

⁵This information I owe to Prof. Dr. H.J. Mönks of Nijmegen University.

⁶See Helmut Moser "Zur Entwicklung der akademischen Psychologie in Hamburg bis 1945. Eine Kontrastschizze als Würdigung des vergessenen Erbes von William Stern:" p.499 in Eckart Krause, Ludwig Huber, Holger Fischer (Hg.) **Hochschulalltag im dritten Reich: Die Hamburger Universität 1933 - 1945, Teil II: Philosophische Fakultät, Rechts- und Staatswissenschaftliche Fakultät.** Berlin u. Hamburg: Dietrich Reimer Verlag, 1991.

⁷Lecture notes by Stern, written mainly in shorthand, contained in the Stern papers in the Archives in Jerusalem. Also an announcement of the forthcoming lectures, and of a lecture already given, in **Nederlandsche Tijdschrift voor Psychologie**, vol.1, 1933-34, p.482,584..

⁸Letter of 25.7.34 from Stern to "my friends", presumably the Cohns...

⁹This information is contained in the same letter from Eva Michaelis-Stern.

¹⁰Title page of volume 5, 1924.

¹¹See L.K.A. Eisenga **Geschiedenis van de Nederlandsche Psychologie**, Deventer: Van Loghum Slaterus, 1978.

¹²See Ph. Kohnstamm "De plaats der Paedagogiek aan de Universiteit", p.102 in **Paedagogische Studiën**, vol. 1, 1920.

¹³See p. 11 of introduction (written jointly by M.J. Langeveld, G.A. Kohnstamm, and H.F.M. Crombag) to Philip Kohnstamm **Persoon en samenleving. Opstellen over opvoeding en democratie**, Amsterdam: Boom Meppel, 1981.

¹⁴The Koninklijke Bibliotheek in den Haag also found no other review of the book.

¹⁵Ph. Kohnstamm "Über Typen des Personalismus" in **Festschrift William Stern**. Beiheft 59.

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¹⁶**Paedagogische Studiën**, vol. 10, 1929, 248 - 253.

¹⁷**Paedagogische Studiën** vol. 9, 1928, 253.

¹⁸**Paedagogische Studiën**, vol. 17, 1936/37, 142 - 146.

¹⁹This and the next two books mentioned were published by Wolters, Groningen.

²⁰On the same tour Langeveld also attended lectures by Heinz Werner, Martha Muchow, and Cassirer in Hamburg, and by Heidegger in Freiburg and Theodor Litt in Leipzig. I thank Max van Manen, professor in the Faculty of Education, University of Alberta for this information.

²¹See p. 268

²²P. 232 - 236.

²³In **Inleiding in de empirische Karakterkunde** (1958) he comments on Stern's account of Heymans' typology. I thank Dr. van Strien for having pointed this out to me.

²⁴Pieter J. van Strien "Transforming psychology in the Netherlands II: audiences, alliances and the dynamics of change" in **History of the Human Sciences**, vol.4, no.3, 351 - 369.

²⁵See **Psychologische praktijken: een twintigste-eeuwse geschiedenis** by Jaap van Ginneken, Jeroen Jansz, Albert Boon, Agneta Fischer, Ben Vincent, Katja van Vliet. 's-Gravenhage: VUGA Uitgeverij, 1986.

²⁶See e.g. Werner Deutsch (ed.) **Die verborgene Aktualität von William Stern**. Opladen: Leske und Budrich, 1991, which contains a collection of papers read at a symposium on William Stern in Berlin in October 1988. Since its inception in 1989 the journal **Psychologie und Geschichte** has published three papers on Stern (by Bittner and Deutsch, by H. Lück, and by W. Schmidt) and two on the beginnings of the

Hamburg Psychological Institute (by Paul Probst) As an example of the new interest in the United States see Jaan Valsiner (ed.) **The individual subject and scientific psychology.** New York: Plenum Press, 1986.

SOVIET PSYCHOLOGY UNDER TOTALITARIAN CONDITIONS

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The socialist experiment in Russia had failed. Does that, however, mean that the related experiment, the attempt to create a marxist science, and in particular, a marxist psychology, also failed too? In our paper we enter the debate surrounding this question (previously developed in the works by Raymond A. Bauer, Loren Graham, Alex Kozulin, David Joravsky and others) seeking to clarify and sharpen some of the periodization of the social history of Soviet psychology.

Totalitarian societies are maintained not only by violence, but also by achieving the loyalty to official attitudes and values by means of ideology. While physical terror began just after October Revolution, dominance of a new ideology was obtained gradually, as mythologized marxism became a sole official ideology. During this time science and philosophy which were capable of producing other ideologies, were brought under the control of central

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authorities. However, the relations between the totalitarian state and science varied through the period 1917-1980s. We describe the evolution of Soviet psychology as it interacted with the social and political conditions during this period.

1917-1929: In this period the dictatorship of the proletariat led to a large number of projects on the global reconstruction of labour, culture, education and even human nature. Thus psychology was faced with grandios social task. Two marxist statements which were taken as postulates for the construction of psychology were: the social nature of human being, and the notion of practice, those were very closely tied to the contemporary revolutionary enthusiasm. The idea of new man inspired a number of tendentious views and utopian projects (by Enchmen, Gastev, Zalkind) as well as quite respectable scientific theories (N.Bernstein, Vygotsky). The later psychologist tried to reconcile the idea of practice, or man's activity in the world, with the "social constructivism" of marxism by understanding man's activity as a self-training and self-constructing one, as an "overcoming" of himself in the process of communication and cultural development.

Although rhetoric of Soviet psychology contained many marxist slogans about actual assimilation of dialectic and materialism, they remained declarations, for a "scientific" interpretation of the psyche had appeared in Russian psychology long before the October revolution due to Sechenov, Pavlov, and Bechterev and continued to exert a strong

influence. This line of thought was supported by the new political leaders, who considered Pavlovian theory "an iron weapon of the proletarian ideology".

In the atmosphere of relative intellectual freedom, when (careful) criticism of marxism was still allowed, almost all the theoreticians in Russian psychology began a creative dialogue with this philosophical conception. The internationally recognized crisis in psychology was another reason to the fruitfulness and strength of that dialogue. Kornilov, Bechterev, Blonsky, Basov, and others sought intellectual resources in marxism which would allowed them to overcome the crisis. The most fruitful ideas and conceptions were generated in this period, the 20s. And a relatively large number of approaches, including psychotechnics and pedology, as well as scientific schools such as reflexology and reactology, existed during this period.

Thus, as in fact it provided relative intellectual freedom, under more relaxed political conditions marxism proved to be a fruitful source, or sounding board for psychology. The two fundamental components of marxist philosophy, the idea that personality is socially conditioned, and the emphasis on practice, or activity strongly affected Russian psychology and inspired both vulgar, tendentious and more scientifically elaborated, psychological conceptions.

1929-1941: Between "the year of great break" and the Second World War the large-scale transformations of society

including industrialization, partial, and then total, collectivization went hand in hand with increasing repression. In parallel ideological repression in science were carried out under the rubric of "discussions". The "discussion" on reactology resulted in the rapid decline of this psychological school. Later the whole field of psychology, namely, pedology, was closed by communist party's decree.

As far as marxism came to be the sole official ideology, the variety of approaches to psychology tended to be reduced to a single "truly marxist" one. Basov and then Rubinstein used an Hegelian-Marxian scheme of activity to analyse behavior and psychic processes. The activity approach had previously played a positive methodological role in the works of the Charkov school and in Leontiev's theory of activity. Later, however, its main function was to cover different empirical studies with marxist rhetoric and thereby defend them from ideological critique. Serving as marxist rhetoric, the activity approach lost its theoretical content.

1945 - till the mid-50s: The "Cold War" inspired a movement to fight cosmopolitanism and led to further ideological infighting within the psychological communities. Psychology especially suffered from the so-called "Pavlovian session", after which it was decreed that psychology had to be reconstructed on the basis of a primitive version of Pavlov's theory. As a result, Soviet psychology fell into

isolation from foreign psychology: it could take only "facts", not "bourgeois" theories. A large gap formed between theory and experiment. Theory followed a "marxist" line of development; whereas empirical studies usually imitated Western studies (which, of course, were based on different theoretical foundations).

From the mid-50s till the mid-80s: The grip of totalitarian ideology began to relax due to the phenomenon of "double-thought". The real influence of marxist ideas declined, as "marxist" slogans were included as window dressing. The more eclectic, pragmatic and flexible System approach began to rival the activity approach in terms of its use of marxist rhetoric. On the other hand, Soviet psychologists continued to use experimental data and analysis procedures taken from foreign, mostly American, psychology. Psychologists began to loose trust in all theories which they identified as containing marxist underpinning. The old idea that psychoterapy and "psychotechnics", concieved in a broad sence, would help to revitalize psychological theory was revived.

From the mid-80s - present: A new more open and wider debate over methodology and theory started once the field no longer had to be "marxist". Unfourtunately, by this time the theoretical potential of Soviet psychology had been almost completely wasted.

In short, the influence on Soviet psychology of marxism varied: from the more productive critical and reflexive role

of the 20s, to the negative restrictive and domineering one under totalitarian ideology, when it led to much meaningless theoretical production.

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'Biting the bullet': The introduction of 'inhibition' into English

* Summary

This paper is a contribution to understanding the historical meaning and context of psychological language. The word 'inhibition' began to be used in English with something like its modern meaning between 1865 and 1880. The cognate German word 'Hemmung' was well entrenched in both physiology and psychology by this date. In Britain, by contrast, it was the distinctive interests of psychological medicine, especially in representing 'loss of control' in terms of nervous pathology, that brought the word into use and enriched its connotations. I sketch some Victorian notions of order and control in the body and society to portray how 'inhibition' became embedded in a web of metaphors and moral or political evaluations of human nature.

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'Biting the bullet': The introduction of 'inhibition' into English

Order, discipline and restraint are closely connected notions in modern life. In the nineteenth-century British army - and other armies too, I imagine - flogging was a common disciplinary measure. Discipline was cut permanently into the soldier's back, pain etched in memory into the soldier's mind. Order and power in society, mediated by order and power in the army, literally embodied itself. It was customary for the soldier undergoing punishment to place a bullet between his teeth. This saved his tongue but also silenced his screaming. It internalized, even naturalized, the punishment, rendering the pain private for a personal transgression, though the spectacle was public and the discipline exemplary. At the end, the round metal bullet would be found flattened. Hence, disciplinarians of many persuasions still refer to 'biting the bullet', significantly enough suggesting that this is something individuals should choose to do rather than something that society imposes.

My illustration concerning the body and power is inspired both by the rhetoric of the political right in Britain and by the late Victorian neurologist T. Lauder Brunton. Writing in the scientific journal Nature in 1883, Brunton alluded to 'biting the bullet' in relation to a theory of inhibition as a general 'interference' mechanism within the nervous system, the means for regulating and integrating actions. Though he intended to be scientific, putting forward testable proposals about physiological causes, it is clear that 'inhibition' appealed to him as a concept with which to explain clinical psychological

phenomena such as the effects of drugs. Brunton also drew on a rich and multi-faceted Victorian metaphor concerning the bodily economy linking thought about savings and expenditure in the personal and political bodies. The disciplined body, such as that of the soldier during flogging, required an outlet or expression for its energies, that it found in biting the bullet. Used metaphorically, 'biting the bullet' transferred this controlled expression to the individual making him (it is manly imagery) inhibit outward display to achieve inward control. Uninhibited babies, by contrast simply yelled; women cried.

Brunton's illustration added vividness to what was otherwise intended to be a technical paper on the inhibitory mechanism in the nervous system. In order to explain such phenomena as the effects of different reflexes on each other in different conditions, Brunton speculated that 'inhibition and stimulation are merely relative conditions' dependent on the interaction between different nervous pathways. Nevertheless, psychological examples rather than physiological details set the agenda. As he observed in relation to the purposes of education: 'The first lesson that a child gets at school is one of inhibition; it is taught to sit still and restrain the movements which external impressions acting on its excitable nervous system prompt it to make. The whole education is, or ought to be a continuation and expansion of this lesson.'

I have begun this paper with reference to 'biting the bullet' to illustrate 'inhibition's' place in a complex discourse. I want to convey the richness of language describing order, bridging bodily and mental events, bridging technical science and everyday descriptions, and

bridging the disciplined person and political economy in order to interpret how one word became common in the language. Some historical background is now needed.

'Inhibition' was originally a legal word in English, referring to the power of a higher party to restrict a lower one, e.g., a bishop restraining a priest. It was also occasionally used as a synonym for prevention, e.g., in referring to the inhibition of sleep. It was first used as a specifically physiological term in an address to the Royal Society by Joseph Lister - famed for his work on antisepsis - in 1858. His subject was sympathetic inflammation, a medical rather than experimental topic, and in this context he discussed recent German research on the visceral nerves. In this context, he translated the German Hemmung as inhibition and created a precedent for the English word having physiological meaning.

But I know of no evidence that Lister's usage was influential. Physiology had little institutional presence independent from medicine in Britain, a situation contrasting with the German-language world where, by the 1850s, there was largescale academic investment in the experimental study of reflex actions and automatic bodily functions. These studies of control in the body found many uses for a concept of Hemmung. In Britain, it was not academic experimental scientists but medical psychologists who, in the late 1860s and 1870s, began to make the word 'inhibition' common.

Brunton, Henry Maudsley and David Ferrier embedded the word in the literature of mind-body relations. These physicians were all associated with psychological medicine and sought for correlations in nervous pathology for psychological symptoms. Brunton and Ferrier worked for a while in the laboratory J. Chrichton Browne established in the West

Riding lunatic asylum in Wakefield - an institutional setting again illustrating the ad hoc arrangements for physiology in Britain. When these men adopted the word 'inhibition', they were attracted by its range of applicability. It referred to the arrest (but not elimination) of one function or action by another, and such functions or actions could be represented interchangeably as controlling relations within the brain and the nervous system or between the moral will and the body. It was a concept admirably suited to their occupational interests, linking expertise to the body but practice to the mind. Further, they all described inhibition in relation to a hierarchy of functions - higher brain and lower nervous system or mind and body - and this naturalized values traditionally associated with the Christian language of the spirit and the flesh, even of original sin.

In the following passage, Maudsley reflected on the enervation of will characteristic of mental pathology: 'The failure of will and its deepening degrees means an increasing dissolution passing into disruption of the federal union of nerve-centres, whereby the present thought or feeling, losing its proper inhibitions, has unbridled sway and way.' He merged physiological and psychological categories ('union of nerve centres' and 'failure of will'), represented moral judgement as clinical description, and exploited the ancient image of human nature as a power needing to be bridled. The image of the bridle, of course, had its classic source in the 'Phaedrus', where Plato likened human nature to a pair of horses, one noble and the other ignoble, which the charioteer struggled to hold together. Maudsley provided the Victorian fear of 'the beast within' with a scientific voice: 'Take away from a young child's mind the germs of those highest inhibitory functions that are presupposed by a potentiality of moral development, and you leave the natural passions and instincts free play.'

Medico-psychological usage linked 'inhibition' with political images as well as moral ones, as Brunton's reference to 'biting the bullet' indicated. The Edinburgh alienist Thomas S. Clouston, who taught medical students for many years, referred to 'inhibition' as 'the colonel-in-chief of the brain hierarchy'. His lectures drew on his experience with the terminal effects of venereal disease and alcoholism, dramatic consequences of a lack of inhibition. Individual moral failings were also social problems. John Hughlings Jackson, followed in relation to 'inhibition' by his pupil Charles Mercier, was fond of the same military metaphor, implying that the removal of authority - 'loss of control' - released anarchy. Nations as well as people 'lose their heads'. Britain was not worried by the prospect of revolution but educated men in the period did ponder anxiously the consequences of increased pressure to extend the political franchise (which was done in 1867 and 1884) and by feminism. The working-class vote and decision making by women raised the spectre of previously contained powers, powers linked in language to the labouring hand and the reproductive body, overwhelming the rational mind. If Maudsley could portray mania as the breakdown of inhibition, it was not much of a leap to fear political disorder in the same terms; madness, after all, was an ancient emblem of social collapse. Reason and order, it seemed, required the inhibition of spontaneously active bodily forces. The British medical psychologists took the notion of inhibition in this general evaluative sense and began also to use it in more technical studies concerning nervous functions.

There is much more to this context than what I have been able to say in a short space. Nevertheless, I hope this sketch suggests what might be involved in studying the history of a word like 'inhibition' which is now commonplace in the physiological and psychological sciences. I

have suggested that the word appealed to physicians and, in turn, to their audiences, precisely because it connoted moral evaluations at the same time as it described the technicalities of recent experimental research. The language of inhibition drew on metaphors and images with which the public was well acquainted and thus the word had a ready audience. It was words such as this that mediated the public's acceptance of scientific culture, of causal, physiological ways of thinking about human nature since they rendered science compatible with, even supportive of, moralistic and political demands on human nature.

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This paper is based on the extended argument in my book, Inhibition: History and Meaning in the Sciences of Mind and Brain (published in North America at Berkeley and Los Angeles: University of California Press, and in the rest of the world at London: Free Association Books, 1992); and in 'The meaning of inhibition and the discourse of order', forthcoming in Science in Context, autumn 1991 issue.

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Abstract

Women in the History of German Speaking Psychology - Roots of a Feministic Psychology?

by
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Paper prepared to the "Cheiron Europe - 11th Annual Conference 1992", Groningen, The Netherlands, August 26 till 30 1992.

The history of Women in the academic psychology is very short. In 1901 the first German university opened the door for women.

In the twenties of this century we had two important groups of women in the German speaking countries. The first were at the university of Berlin around Kurt Lewin (for instance Bluma Zeigarnik, Tamara Dembo, Gita Birenbaum, Anita Karstens, Wera Mahler, Sara Fajans, Käthe Lissner, Maria Ovsiankina, Sarah Sliosberg, Sara Forer, Margarete Jucknat etc.). The second were at the university of Vienna around Charlotte and Karl Bühler (for instance Hildegard Hetzer, Lotte Schenk-Danziger, Else Frenkel-Brunswik etc.).

In our paper we give a report about the work in one of these groups and the conditions for a creative achievements of women in sciences, especially in psychology. These conditions are in our opinion the main content of the so-called "feministic psychology".

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**Frauen in der Geschichte der deutschsprachigen Psychologie -
Ursprünge einer feministischen Psychologie ?**

(Women in the History of German Speaking Psychology - Roots of a
Feministic Psychology?)

von
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Frauen in der akademischen Psychologie gibt es erst seit etwas mehr als einem Menschenalter.

Blicken wir zurück: Im Jahre 1901 hatte als erstes deutsches Land Baden seine Universität für Frauen geöffnet, Bayern folgte 1903, Württemberg 1904, Sachsen und Thüringen 1906 und Preußen 1908. Weibliche Studenten aber blieben lange Zeit eine Minorität (vgl. z.B. Meyers Lexikon 1926).

Auch nach dem ersten Weltkrieg, in der Weimarer Republik, war das Hochschulstudium für Frauen noch keineswegs eine Selbstverständlichkeit und oftmals wurde es sogar noch absichtlich erschwert. Trotzdem waren an der Berliner Friedrich Wilhelms - Universität zwischen 1923 und 1933 bereits ca 18% der Studierenden Frauen (Bock und Jank 1990, S.47; vgl. auch Verein Feministische Wissenschaft Schweiz 1988).

Unser Lehrer Kurt Gottschaldt berichtete uns einmal in einer seiner Vorlesungen in den fünfziger Jahren aus seiner Studentenzeit in den zwanziger Jahren in Berlin. In Erinnerung ist uns u.a. geblieben, daß das Frauenstudium damals noch keineswegs allgemein von allen Professoren akzeptiert wurde. Gottschaldt erzählte uns, daß er noch Hochschullehrer kennengelernt habe, die erst dann mit ihrer Vorlesung begannen, wenn die anwesenden Frauen den Hörsaal verlassen hatten. Frauenstudium in dieser Zeit bot demnach noch eine Fülle von Minoritätenproblemen.

In Anbetracht dieser Tatsache ist es erfreulich und zugleich verwunderlich, daß in dieser Zeit z.B. bei Kurt Lewin in Berlin von 16 der von uns bisher identifizierten Schülern 11 Schülerinnen waren.

Zur selben Zeit bildete sich in Wien um Karl und Charlotte Bühler ein Kreis von Wissenschaftlerinnen, die wesentliche Beiträge zur Entwicklung der Psychologie leisteten.

Diese frühen Beobachtungen, die bis in unsere Studentenzeit reichten, waren für uns der Anlaß, einmal zu hinterfragen, welche Eigenschaften z.B. Kurt Lewin's, sowie welche Eigenschaften seiner zahlreichen Doktorandinnen, die Voraussetzungen für die heute noch anerkannten wissenschaftlichen Leistungen seiner Berliner Dokto-

randinnen bildeten.

Die Tatsache, daß sich zu Kurt Lewin in seiner Berliner Zeit als Schüler vor allem Schülerinnen hingezogen fühlten, und daß diese weiblichen Doktorandinnen unter seiner Anleitung so beachtliche Leistungen vollbrachten, führen wir vor allem auf folgende Bedingungen zurück, die wir allerdings nicht als Rangreihe verstanden wissen möchte:

1. Auf Lewins heuristisch produktives Konzept einer wissenschaftlichen Handlungs- und Affektpsychologie, die eine hohe Alltagsnähe und Praxisrelevanz besaß.
2. Auf Lewins ökologisch hoch valide Paradigmen, mit denen er die empirischen Untersuchungen in lebensnahen Experimenten durchführen konnte.
3. Auf Lewins demokratischen Führungsstil, der mit einer hohen Akzeptanz und Selbstständigkeitserwartung den Schülerinnen gegenüber gepaart war.
4. Auf seine Förderung durch Forderung, wobei er vorlebte, was er von anderen verlangte.
5. Auf Lewins positives Frauenbild, das von Gleichberechtigung und durch geistige Partnerschaft geprägt war.
6. Auf die hohe Studien-, Bildungs- und Arbeitsmotivation seiner Doktorandinnen, die sich alle schon vor der Zusammenarbeit mit Lewin durch ihre mehr oder weniger schwierigen Bildungswege als beharrlich und nachhaltig erwiesen hatten.

Gerade solche Bedingungen, wie sie Lewin seinen Schülerinnen bot, sind nach unserem Verständnis allgemeinere Voraussetzungen, unter denen es Frauen leichter gelingt, in der Wissenschaft kreativ zu werden. Insbesondere kommt das durch Akzeptanz und Gleichberechtigung bestimmte soziale Klima in Kurt Lewins Forschungsgruppe dem Bedürfnis vieler Frauen nach partnerschaftlichen Arbeitsformen entgegen (Wagner, 1985).

Das Problem "Frauen in der Wissenschaft" ist noch heute - trotz hoher Studentinnenzahlen - in einem nicht geringen Maße noch immer

als Minoritätsproblem aufzufassen.

Nicht wenige Frauen in der Wissenschaft fühlen sich bis zum heutigen Tage in einem gewissen Ausmaße noch immer in einer "Außenseiter"- bzw. in einer "Pioniersituation", wie es 1985 Ina Wagner formulierte. Eine andere Autorin, Myriam Salzmann, hat im selben Jahr diese nicht selten anzutreffende schwierige Rollensituation weiblicher Wissenschaftler noch zugespitzter formuliert und sie mit "Fremdarbeiterinnen" verglichen (Salzmann 1985).

Nach unseren Erfahrungen bedürfen Frauen in wissenschaftlichen Arbeitsgruppen in einem stärkeren Maße als Männer eines günstigen sozialen Klimas, um sich wissenschaftlich entfalten und kreativ werden zu können. Sie bedürfen einer stärkeren sozialen Unterstützung zur Festigung ihres Selbstbildes als Wissenschaftlerin.

Das gruppendynamische Modell "Lewin und seine Berliner Schülerinnen" scheint uns daher ein gelungenes Beispiel einer kreativen und frauenförderlichen wissenschaftlichen Arbeitsgruppe gewesen zu sein, von dem man auch nach einem Zeitabstand von etwa 60 Jahren vieles lernen kann.

Von einer "feministischen Psychologie" im abgrenzenden Sinne zu sprechen, scheint uns jedoch nicht notwendig zu sein.

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JULIAN OCHOROWICZ (1850 - 1917) AS A FORERUNNER OF THE INTERNATIONAL ↑ CONGRESSES OF PSYCHOLOGY

From August 2 to 10, 1889, Paris hosted the 1st International Congress of Physiological Psychology. Although it is commonly known that the author of the first published project of international psychological congresses was a Polish psychologist and philosopher, one of the leading theorists of hypnotism during the second half of the 19th and the beginning of the 20th centuries, Julian Ochorowicz, his efforts made between 1881 and 1889 to realise this idea, seem to be hardly known at all, particularly to anyone who cannot read Polish. The aim of my paper is to reconstruct the course of events which took place since Ochorowicz's project publication and led immediately to holding this historical convention.

After graduating from Warsaw University in 1872 with a degree in natural science, Ochorowicz continued studying psychology at the University of Leipzig under Fechner and Drobisch. On his return to Poland, Ochorowicz received in 1875 his habilitation at the University of Lvov and became the first in a Polish university Dozent of the empirical psychology. In 1882 Ochorowicz went to Paris where, during his ten-years' stay, was engaged in hypnotic therapy and experiments on the transmission of sound over a distance.

Before his travel to Paris, Ochorowicz submitted the article entitled "Projet d'un congres international de psychologie" for publication in Th.A. Ribot's *Revue Philosophique de la France et de l'Etranger*. The article was published in the 1881 issue (vol. 12, pp. 1 - 17). On the basis of a detailed "analysis of the contemporary psychology's situation at large and not only of its relations to medicine" (Ochorowicz, 1916, p. 227), he arrives at the conclusion that "what is lacking is organisation of work (...). The only way to achieve this aim which I submit for psychologists' consideration is an international congress" (Ochorowicz, 1881, p. 8 - 10).

Ochorowicz's project didn't find approval for the time being. Even Ribot himself considered it as chimerical! At Ochorowicz's project became enthusiastic a Mr. Potonié-Pierre. This devotee of psychology was a member of the Parisian *Société d'études philosophiques et morales* which "appointed a commission to realise my project (...)" Mr. Potonié-Pierre extended an invitation to me to attend the meeting of 'people of great note' (...) I came to the meeting and delivered a lecture in broken French on the status of contemporary psychology and on the necessity of the congress. The lecture attended some old women, three tradesmen, and one Freemason; the other 'people of note' had reasons for not going. The meeting arrived at the conclusion that it is necessary to organise the congress, whereas I - that this is rather impossible, and there was an end" (Ochorowicz, 1889, p.1).

In the meantime of Ochorowicz's efforts had heard dr. Baréty from Nice and encouraged him to form the psychological society. "In 1885 a group of the devotees of psychology busied itself with setting up the proper association in Paris. And when it was clear beforehand that none of the best known philosophy professors would assume the leadership, they approached Prof. Charcot, a physician. At first he resented: 'What do you want with me?' - he said - 'Psychology is spiritualism' (...) After much persuasion and submitting a list of the first respectable members to him, Charcot eventually agreed to take over the leadership, however providing that the title *Association of Physiological Psychology* should be submitted for *Psychological Association*. That is why both the Association and the first congress had to use the name which improperly restricted their range" (Ochorowicz, 1916, p. 5). Having given birth to the first congress four years later, the Psychological Association ended its days.

Two years since foundation of the Association Ochorowicz approached Ribot with the proposition: "How about the congress now?" "Let's try" - he agreed. In order to attach a greater importance to his application, Ochorowicz asked Ribot to present the matter personally to the Association. He was glad to do that, and this time everything went swimmingly. The motion not only was accepted unanimously, but also with enthusiasm (Ochorowicz, 1889, p. 2). A Committee of Organisation was appointed and Ochorowicz has been named as one of its

six members.

In his opening address Professor Ribot (1889, p.177, my translation) made a personal confession which, in his words, didn't credit to him: "Eight years ago, in 1881, one of the participants of this congress sent to me from Lvov a full programme of the international congress of physiological psychology. A proposal appeared to me at that time to be very encouraging but also somewhat chimerical. Nevertheless, I published an appeal of a colleague of ours to psychologists in the hope that it may be successful in the near future. I didn't expect it to happen so rapidly; I blame myself for faint-heartedness, and so I am happier than anybody else that the current of events didn't declare me to be in the right". That unnamed "one of the participants of this congress" and "a colleague of ours" whom Professor Ribot had in mind was, of course, Julian Ochorowicz.

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Them and Us

Continuities in the Changing Modes of Constructing Ethno-
psychologies

Abstract

My paper's concern is with some fundamental questions which, I think, need to be considered in a historical analysis of modes of constructing "ethnopsychologies", a term used for projects as diverse as e.g. the Stern-Thurnwald approach to ethnopsychography, the classical culture-and-personality school as well as its recent revivals, and ethnopschoanalysis. It is suggested that the problems posed in these projects, the social relations involved in research practices, the politics involved and the ethnopsychologies produced ought to be analysed in the light of the specificity of modernity's concern with the Other, its lasting implications for colonialism, and some continuous features of (neo)colonial-scientific relations.

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Them and Us

Continuities in the Changing Modes of Constructing Ethno-
psychologies

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I believe that the quality and
significance of our research would
be enhanced if we spent more time
on the formulation of the questions
we are asking Guthrie 1979

When I began last year to survey the domain of ethnographic-
psychological interaction, it was with a strong motive and a
seemingly clear idea of what was to be done. Considering the
current social transformations on a global scale, the images and
categories in which we conceive of ourselves and others have
become increasingly important for the attempt at participating as
responsible and conscious human beings in current collective
history. Western views of the global social space and the
multiplicity of relatively autonomous and yet mutually
interdependent agents populating this space (Bauman 1989) need to
be reconceptualized. A historical critique of the human science
contributions to these views might at least support the process of
working through the lasting constraints they have imposed on our
thought. With this motive on my mind, I reread the theories of
civil society by the Scottish and French Enlightenment authors and
surveyed projects like the psychography of primitive peoples
(Institut für Angewandte Psychologie 1912), the culture-and-
personality approaches at Columbia university, cognitivist cross-

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cultural psychology and recent ethnopschoanalytic studies. These projects provide ample evidence of opportunities to widen psychology's epistemic access to multiform subjectivities - opportunities that were nevertheless to remain marginal in the development of the discipline. The aim of a historical critique of ethno-psychological joint or complementary ventures got hold of me. For a start, my task would consist of a reconstruction of the problems posed in the various ethnopsychological projects, the cooperative research practices established, and the politics involved.

Student interest in a recent survey seminar devoted to this task inspired my greed of reading through heaps of psychological, social and cultural anthropology, as well as historiographic accounts of emerging ethnography and some modern schools. However, the more I read the less confident I became about the kind of questions involved in the intended historical critique. This is why I will not present a neat piece of historiographic analysis of ethno-psychological joint ventures but rather take you on a spiralling trip of sorting questions.

Setting the Stage

Let me begin with a puzzling observation. Around 1980, historically oriented accounts (Bock 1980; Jahoda 1982) as well as research collections (Williams 1975; White & Kirkpatrick 1985) of what has recently be renamed "psychological anthropology" express a striking optimism with regard to the future of ethnopsychology. On the other hand, ethnographic self-reflection on the ephemerality of anthropological constructions of the Other abounds. Deconstructions of the savagery/civilization dichotomy

(Pearce 1980), "primitive society" as "invented" by the British tradition (Kuper 1988), the "poetics and politics of ethnography" (Clifford & Marcus 1986), and the means by which "the anthropologist as author" used to convince readers of the claim of "having been there" (Geertz 1988/1990), to mention but a few, provide sensitive critiques of the ethnographic enterprise. The "question of the Other/other" (Todorov 1982), of identity and alterity (Voestermans 1992) has noticeably entered the agenda, calling into question the very notion of modern Western culture.

What is specific about modernity's concern with the Other?

It has often been pointed out that in Western culture auto- and hetero-images are constructed by way of dichotomies, of which savagery/civilization is the most prominent one, mirroring colonial practices. To be more specific, a closer look at the Western concept of culture suggests that some roots of the Western imagery of selves and others can be traced back to the emergence of the project of modernity. Drawing from Markus' (1992) analysis of the historical semantics of this concept, three points are important in my context: First, the Western notion of our world as of something made by earlier generations of humans and remakable by the living ones is unique in that Western culture sees itself as culture and as a culture among many. This notion was first developed by Enlightenment intellectuals and entails a historicized view of culture as a process of (self)education of humankind to maturity as well as the notion of human unity and universality. In 18th c. theories of the history of civil society, the societal meaning of culture had fused with an earlier pedagogical meaning, stimulating reflection on the relationship

between individual capacities and the culture of a whole society, between individual innovations and sociohistorical change. Second, "an interest in exotic cultures, these living examples of the variety of mores and 'opinions' all to be judged and transformed by reason, went together with the casting of the 'lower classes' in the role of aliens at home, 'savages'...whose 'culture' could equally become the object of a distanced interest" (ibid.,9). Thus ethnography abroad and at home emerged in the same process and within the same conceptual framework. Third, it was through the concept of culture that the emerging bourgeois society "announced its claim to historical superiority as a form of order able to ensure the fullest development and unlimited perfectioning of all human abilities" -and through the same concept that this claim became criticized: "Turgot and Rousseau stand almost symbolically for the ambiguity of an affirmative and a critical power pertaining to the same concept, an ambiguity that was to last through Western history" (ibid. 10)).

Assimilating or preserving, colonizing or understanding - a continuing ambiguity underlying ethnopsychological constructions?

Since the rise of the specialized human sciences which coincided with the height of colonialism, anthropology has become one of the battlegrounds where the ambiguity of the Western concept of culture is being carried further. Preservationist claims with strong undertones of Noble Savagery" (Stocking 1987,289) are levied against progressivist assimilation, understanding is invoked against (neo)colonization. Yet the Europeans' capacity to "understand" may have been the very vehicle for the successful colonial policy of indirect rule (Langham 1981;

Todorov 1982). <to be elaborated>

changing modes of constructing ethnopsychologies

<captions to be elaborated: 1850-1950 From the natives observed to "the natives point of view"; the social relations of ethnographic data construction; ethnographers, psychologists, psychoanalysts: who does what with the data to deal with what kind of questions>

What has changed with the fall of colonialism and scientism?

As remarked by Geertz (1990), colonialism and scientism fell at the same time. The anthropologists' claim of speaking for the unheard has been shattered since the "native hits back" (Lips 1937), "writes back" (Clifford), since people from non-Western cultures speak for themselves, sometimes even as anthropologists. Anti-colonial struggles strongly inspired the anti-positivistic rebellion in the metropolises, and incited some awareness of the psychosocial relationships between colonizer and colonized (Fanon 1961; Mannoni 1950); the "pathologies" of the colonized also came to be seen as products of colonization. Soon the "wildness"-in-others became interpreted in terms of the "wildness"-within-ourselves (Devereux).

Yet while scientific self-confidence received a lasting blow, postcolonialism transformed into neocolonial patterns of political autonomy and economic dependency, bringing about a new pattern of a triangular relationships of neocolonial economic power, indigenous political power groups, and indigenous labour force (Streiffeler 1982).

Continuing absences in ethnopsychological investigation

The changes in social relations and interaction imposed upon non-Western cultures by colonialism and neocolonialism have hardly been analyzed. The absence of an interest in processes of social change that has for long characterized most of Western social science on its own territory has also been exported to others. Participation in the ethnopsychological endeavour has not changed the psychological/psychoanalytical preconcens with mind, behaviour and personality, either. Attempts at a social psychology of cultural change remained neglected with regard to both Western society itself and the psychodynamics of neocolonial relations.

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PSYCHOTHERAPY IN THE MOVIES, 1920-1992

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Abstract

The mythical man in the street about whom we know so little holds a variety of opinions about madness and everyday neurosis, including ideas on the ways they may be cured and the role of professionals in this field. One the sources of his attitudes and opinions is the popular picture of psychiatry and psychology depicted in the movies. At the very least, commercial films may serve to reinforce existing attitudes and, reversely, be used by the historian as an index of prevailing cultural stereotypes at a given time in a certain cultural context.

The aim of my paper is to explore several images of madness and the psychotherapist in the movies, ranging from a relatively sympathetic reception in the forties and fifties (e.g. Hitchcock's films, such as *Spellbound*) to the increasingly more complicated and largely critical images presented in films during the seventies and eighties (*Dressed to Kill*, *One Flew Over the Cuckoo's Nest*, *Morgan*, *Silence of the Lambs*; *Final Analysis*: cf., however, the sympathetic portrayal of the therapist in *Ordinary People*). I try to answer questions such as: How successful is psychotherapy in defending a monopoly of expert's knowledge and interpretation? What kinds of rhetorical techniques are used in influencing the public, and do they succeed? What are the characteristic differences between the image of psychotherapy in the movies and the self-image of the profession? Do films serve as a medium of diffusion of professional knowledge? Does this lead to so-called *proto-professionalisation* in the public? My talk will be illustrated and interrupted by fragments from moving pictures on video.

**THE ROLE OF THE HISTORY OF PSYCHOLOGY IN THE PROFESSIONAL
ORGANISATION OF SPANISH PSYCHOLOGY**

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INTRODUCTION

The Historiography of Psychology has almost always been characterised by four different models: evolutionary or genetic, personalised or centred on the great personages, cultural or of the "Zeitgeist" frame of thought, or with the main focus on problems, topics and subdivisions of research. Nonetheless, at the beginning of the 1960's, the History of Psychology was to undergo a crucial transformation generating a situation highly different to what Watson (1961) was grieving upon thirty years before (Grunwald, 1984; Brozek, 1990). The situation changed to the extent that many authors look upon the 1960's as the beginning of a 'new era' in the history of psychology, an era, identified, amongst other ways, by the search of new explanatory, theoretical models (Eckardt and Sprung, 1981).

Although the first great development towards the professionalisation and institutionalisation of psychology was basically achieved in America, various interesting evolutions have occurred in a wide range of countries spanning the five continents, and especially in Spain. In recent years, Spanish psychology has displayed a series of traits which must be considered if a precise picture of its present complexity is to be portrayed: a great institutional development, strong emphasis on the application of techniques, insufficient delineations of the role of the professional, theoretical-conceptual pluralism, and interdisciplinary activity. These traits are also visible in the increasing professional sphere of the History of Psychology.

Material on the History of Spanish Psychology has since become abundant in both general and specific areas (Carpintero and Lafuente, 1991) being: institutional (Encinas and Rosa, 1990), specialised areas (i.e. traffic (Tortosa and cols., 1989), industrial/organisational (Peiró, 1984), social (Jimenez-Burillo), behavioural modification (Cruz, 1984), and educational (Gilolmo, 1989)). Consequently, the development of the History of Psychology has become a speciality in its own right (Tortosa and cols., 1991, Mulberger, 1991). However, still to be studied is the role of the History of Psychology within the professional framework of Spanish Psychology, a framework which is adequately defined by the 'Colegio Oficial de Psicólogos' (Official School of Psychologists) and its activity. This 'School' is protectionist to a great extent, and dedicated to the profession it represents. And this area is precisely the aim of the work in hand - to offer a picture of the structure, the functioning and the main events for which this institution is responsible; and within its field of interests, to be specified is the position occupied by the History of Psychology as a speciality.

**THE ORGANISATIONAL AND INSTITUTIONAL ASPECTS OF THE STUDY
AND PRACTICE OF THE HISTORY OF PSYCHOLOGY IN SPAIN.**

Despite the delay in the institutionalisation of psychology as a discipline in Spain, it is essential to emphasize that the History of Psychology has undergone a major development in the last decade.

Spanish Psychology, which has almost consistently been removed from the reality of modern psychology, and has experienced a limited participation in the creation of positive science, appears to be replete with ups and downs. Indeed, it is as if its evolution, in terms of a discipline and a scientifically oriented profession, had experienced an estrangement away from society. In this century, liberal-minded psychologists, of the republican ideal, supported the development of the first real-life psychological applications; but the Civil War and the dictatorship brought with them a swing towards the opposite pole, submerging psychology in a scholasticism which flooded the universities of the official post-war scene, and attempted to do

Meeting (1987); and the 'Sociedad Española de Historia de las Ciencias y las Técnicas' (Spanish Society of the History of Science and Technology) with the V Congress (Murcia, 1989), all of which entailed numerous sessions on the History of Psychology. Furthermore, professional societies (Colegio Oficial de Psicólogos) scheduled conferences on areas specifically dedicated to History in their conferences (I and II National Congress). Consequently, an active participation began in International Congresses, i.e. XXII and XXIII International Congress of Psychology (Leipzig, 1980, Acapulco, 1984), the meetings of CHEIRON-Europe, etc.

Nevertheless, the fundamental institutional milestone was the emergence in 1980 of the "Revista de Historia de la Psicología" (RHP- The Journal of the History of Psychology), the first journal specialised in historical subjects in the Castilian language. Under the direction of Helio Carpintero and José María Peiró, it continues to be edited in the Department of General Psychology in the Faculty of Psychology, University of Valencia. The publication was launched in an internationalist spirit, opening up its pages to articles in English in 1982 (i.e. including authors such as Brozek, Bridgmann, Sprung, Zusne, Danziger, Ash, Eysenck, and Ardila). However, the major concern of the journal was to recover the historical tradition of Spain, and to approach the Latinamerican world (Lafuente and Ferrándiz, 1991). The journal has also published various monographic articles (i.e. on Wundt, Adler, journals, Spanish History, and also a volume of essays in Professor Josef Brozek's honour with contributions from a great many scholars from various countries).

A few years later, in 1984, the long process of the principal organisational milestone of the 1980's began: the birth of the "Sociedad Española de Historia de la Psicología" (SEHP- Spanish Society of the History of Psychology). This occurred after the "I Jornadas de Historia de la Psicología" (First Day of the History of Psychology) celebrated in Salamanca and culminating in the first general study of History performed by Spanish historians (Rodríguez, 1985). Since its initiation, the Society has been publishing a periodical 'Bulletin', and both directly and indirectly, through the "Revista de Historia de la Psicología" (the Journal of the History of Psychology), the body of scientific expression of the SEHP since 1990, the journal has published the minutes of the successive annual meetings: Madrid (1988), Valencia (1989), Barcelona (1990), Seville (1991), San Sebastián (1992), (Cfr. Quintana, 1991).

Finally, the teaching circumstances. In the short time since its full establishment, it could be supposed that, hypothetically, Spanish psychology would be dominated by professional and technological programmes to the detriment of the existing teaching. This, however, is not the case, since the History of Psychology constitutes an obligatory section of the curriculum of General Psychology, and has done since the initiation of the degree in 1968. In the present day, this subject is taught in almost all of the faculties offering education in psychology (including the Universities of Barcelona, the Balearic Islands, Málaga, the Canary Islands, Murcia, Seville, Pontificia, Madrid, Oviedo, Valencia, Duesto, the Basque Country, and Salamanca), and will usually be found in the Department of General Psychology in the second or third years of a five year degree course. Today, seven chairs (Two in Valencia, and one each in Barcelona, Madrid, San Sebastián, Salamanca and Murcia) and numerous assistant professorships and lecturers are specifically oriented towards the History of Psychology.

Furthermore, historical and historiographic subjects are included in numerous doctorate programmes, a fact which has favoured the specialisation and development of diverse programmes of research, in many cases with subsidies from public and private, national and international entities. To be highlighted here is the broad programme of theses of the degreee and the PhD in Valencia and Barcelona, backed up by well defined historiographic models - being sociohistoriometric and paradigmatic. And, out of the two, the most exceptional is that initiated by H. Carpintero in Valencia.

This programme was launched almost immediately after the lecturer, Helio Carpintero, obtained his professorship in the History of Psychology in the University of Valencia in 1971. It experienced a strong growth from 1976 onwards, when the application of bibliometric methodology of historical research began. One of the invariables of the programme has been the recognition and study of national traditions in psychology, i.e. from the USA, GB, Germany, France and Spain. Psychological domains have been analysed such as: Therapy

and Behavioural Modification, Social Psychology, Psychology and Road Safety, Psychological Evaluation, Industrial and Organisational Psychology, Sport Psychology etc.; the impact of authors such as: Marañón, Germain, Mira, Barnes, Luria, Freud, Piaget, Watson, Claparede, Maslow, Hebb, Estes, Baldwin, Olds, Vygotsky...; and movements such as: Psychoanalysis, Behaviourism, Genetic Epistemology, Functionalism... And not to mention, along with these topics, a certain attention paid to historical matters of a more global or methodological character. Up to 1991, a total of 54 degree theses and 36 doctorate theses had been submitted within the programme, and passed, (López-Latorre, 1991).

METHODOLOGY AND SOURCES

The zero hypothesis we consider here is that within the most protectionist framework of the professional societies, the presence of the History of Psychology is scarce, incidental, of low importance, and intermittent; while the alternative hypothesis is the contrary - a presence which is abundant, coherent, substantial and consistent. With the purpose of verifying which would be correct, an investigation has been designed to study the appearance of material on the History of Psychology and Historiography, in the most general framework of 'Theoretical and/or General Psychology', (as defined in "Psychological Abstracts") in the professional and public activities undertaken by the "Colegio Oficial de Psicólogos", since its foundation, at the end of 1979, until 1990.

In order to carry out the analysis, a sociohistoriometric model has been designed which is backed up by objective and quantitative methodology, but does not reject the most classical critical analysis of documental sources. This model leans upon the use of bibliometric techniques, the perception of science as a complex organisation, and the conviction that scientific knowledge is public and communicated. Consequently, the analysis of the channels of distribution of this communication allow the reproduction of the welfare of a science at any determined point in time, or throughout a particular period of time (Carpintero and Tortosa, 1990).

The professional ambience has effectively been defined in terms of material published in the Minutes of General and Specialised Congresses at National and International Levels and in Professional Journals Dependent upon the 'Colegio Oficial de Psicólogos', the professional institution which legally represents this field, created by the Law of the 31st December 1979. The journal selected for the analysis is actually 'Papeles del Colegio' (Papers of the School), the basic body of expression of the scientific and professional policy of the Society, which has been published non-stop since 1981, and prints some 27,000 copies. Also selected are the Minutes of the General Congresses of Madrid (1984) and Valencia (1990), and of the Congresses of the Psychology of Work and Organisations of Madrid (1983), Tarrasa (1985), and Madrid (1988). The content of all the works has been studied independently of the thematic spectrum, as well as Guest Conferences, general reports, sessions of Free Communication, and panels and posters.

The First General Congress, "Psychology, Human Development, and the Quality of Life", discussed the following themes: Psychology and Profession, Educational Psychology, Psychology and Health, Psychology, Society and the Quality of Life, and, finally, Work Psychology. The second, "Spanish Psychology in the Europe of the 1990's: Science and Profession", consisted of 11 topics of discussion: Psychology and Profession, Psychology and Education, Psychology and Health, Psychology of Work and Organisations, Psychology and Social Services, Theoretical Psychology, Psychological Diagnoses and Evaluation, Psychology and Road Safety, Judicial Psychology, Psychology of Physical Activity and Sport, and Psychology of the Armed Forces.

The objectives of the three Congresses of the Psychology of Work and Organisations (the third of which coincided with the First Latinamerican Congress) were generic: to promote the university education of the psychologist, to encourage general and applied psychological research, to aid communication between specialists, and to consolidate the psychological profession. The first, "The Present and the Future of Work Psychology in the Firm", covered the themes of: Psychology of Organisations, Psychology of Conflict, Training and Development in the Firm, Motivation at Work, Psychology and Mental Health, and the New Dimensions of Work Psychology. The second, "Psychological Intervention in

Organisations", boasted a greater thematic spectrum: Psychosocial Intervention and the New Perspectives of Organisations, Psychology and Road Safety, Power, Influence, and Role-playing in Organisations, Psychological Intervention and Organisational Efficiency, Exchange of Experiences in Private Organisations, Exchange of Experiences in Public Organisations, Spanish Experience of Quality Circles, the Role of the Hierarchy in the Process of Change and Organisational Development, and the present panorama and prospects of European Organisational Psychology. The third, "Psychology, Work, Organisation and the New Forms of Employment", dealt with a reduced number of themes, but was more specialised in character: Development of Human Resources, Organisational Development, the Psychosocial Impact of Technological Innovation, Quality of Working Life, Techniques of Measure, Psychology of Consumption and Specific Employment Situations.

After extracting the information from the Database Filemaker II, a databank was created that grouped together 1,796 works from the various selected sources. These were distributed as follows: First Congress of COP (Official School of Psychologists) (349), Second Congress of COP (528), First Congress of Psychology of Work and Organisations (146), Second Congress of Psychology of Work and Organisations (62), Third Congress of Psychology of Work and Organisations (178), and "Papeles del Colegio - Psicólogos" (Papers of the School - Psychologists) (533). The articles related to historian activity in the professional field of psychology have been determined from this collegiate productivity, using various judges to read the 'abstract', and, when necessary, the whole article.

THE MOST PRODUCTIVE AUTHORS IN THE HISTORY OF PSYCHOLOGY

Generally-speaking, the decade in question represents a time of expansion and professionalisation in Spanish Psychology (Carpintero and Tortosa, 1990), embodying a certain urgency to create an authentic tradition of research and intervention. Indeed, in basic terms, it may be said that the interest in historical studies has grown from a concern to build a bridge between recent psychological studies and the scientific tradition of pre-Civil War Spain. This could be why there have been major developments in the research of the basic processes and epistemological and historical concerns from a fundamentalist point of view and perspective. This has been a characteristic feature of the disciplinary traditions in times of confrontation and expansion, which in previous times, when history and historians were unprofessional and more oriented towards the justification of the present situation of the subject, lead to ideologised and ceremonial uses of History. However, in the 1980's, with an emphasis on the aforementioned institutional and organisational development, the discipline was able to boast several, far from amateurish, projects, as those already produced in other academic spheres (Cfr. Tortosa and cols. 1991; Lafuente and Ferrándiz, 1991).

The number of different authors in the field is 139, being just 7% of the total amount, but significant. In the journal "Papeles del Colegio", the thematic field of Theoretical Psychology, including History, occupies a considerable position, (Civera, 1991). History has been highly linked to a practice which, externally, is very ceremonial: Homages to deceased Spanish figures (i.e. J. Germain, M.E. Romano, and J. Mallart) and those still alive (i.e. J.L. Pinillos); the history of other Spanish journals, i.e. "Revista de Psicología General y Aplicada" (Journal of General and Applied Psychology, launched in 1946), "Revista de Psicología y Pedagogía Aplicadas" (Journal of Applied Psychology and Pedagogy, 1955), "Revista de Historia de la Psicología" (Journal of the History of Psychology, 1980); monographs dedicated to Spanish Psychology in 1989; and the constant attention given to scientific documentation and the managing of sources, bases and databanks.

Additionally, the General Congresses have dealt with areas dedicated to Psychology and Profession, and Theoretical Psychology, together with specialised presentations (i.e. "A Historical View of Psychology in Spain, directed by J.M. Gondra in the First COP Congress, and "The History of Psychology in Spain", directed by H. Carpintero in the Second). History had played a less formal role in the Congresses of Work Psychology, but whether in the sessions of 'Free Communication', the General Speeches or Conferences, the Thematic Sessions or the presentations of specialised subjects, themes, technological developments and dominant models have repeatedly been chronicled.

Although with very sporadic appearances, three groups have clearly stood out whether for their consistence at the Congresses, or even for their active participation in these Congresses and the Journal of History itself. Here we are referring to those associated to the Universities of Valencia, the 'Autónoma' of Barcelona and of Murcia.

The prominent group in this collection, due to volume of different researchers, to their appearance in all the documental sources used, and to their level of productivity and collaboration, is that centred around H. Carpintero and F. Tortosa from the Faculty of Psychology, University of Valencia. This group is also the most active in the academic scene, holding three of the seven chairs, and, as we have already mentioned, responsible for the most productive doctorate programme, the editing of the "Revista de Historia de la Psicología", and with a strong position in the direction of the SEHP.

The major cores of interest of this group have been: the historical evolution of the application of Traffic Psychology; Spanish presence in the International Congress organised by the present 'Unión Internacional de Psicología Científica' (International Union of Scientific Psychology, always highly related to the fields of Applied Psychology); the arrival and evolution of movements in Spain (essentially psychoanalysis and behaviourism); the analysis of works of predominant personages in the process of institutionalisation and professionalisation of Spanish Psychology (especially J. Germain); communication networks under diverse spheres (psychology of work and organisations, clinical psychology and education); as well as a global perspective of the development of psychology in Spain.

The second group, in order of importance, is that surrounding E. Quiñones, from the University of Murcia, including various researchers such as M. Mateu, J.A. Vera, M.J. Pedraja, and M. Vera. After an initial collaboration with Mateu, where traditional and new tendencies in the field of Organisational Psychology were compared (I Congress of Work Psychology), there was a shift towards the history of Spanish psychology. This shift can be clearly seen in the II COP Congress with three studies of Spanish and Latinamerican psychology: Argentinian Psychoanalysis, an analysis of the journal of the "Historia de Ciencia" (The History of Science - Llull), and an analysis of the contributions of the Murcians, Alberca and Valenciano. Finally, we find the group centred around M. Saiz-Roca from the 'Autónoma' University of Barcelona, which concentrates on diverse aspects of the development and the image of psychology in Catalonia, and especially on the work of R. Turró and E. Mira i López.

In addition to these, other small groups have been created, such as that formed by Valderas, Echevarría, and Blázquez, dedicated to the History of Psychology in the Armed Forces; or, as the group of M. Kirtchner, who approaches various aspects of the study by Mira in the context of the development of applied psychology in Catalonia before the Spanish Civil War.

Nonetheless, the afore-mentioned groups are by far the most active. What is more, they have a common feature, in that they all share the bibliometric method of work. Their studies are within the historiographic conception, claiming to obtain a social means of determining the future of any science, through the objective analysis of scientific literature. This model claims to integrate, and not exclude, what appear to be diverse methods and approaches. The basic hypothesis is that the analysis of scientific literature permits access to the dimensions of the scientific organisation constituting psychology, being qualitative (conceptual and theoretical), and quantitative (material and social). Journals, dictionaries, textbooks, repertoires, minutes of congresses and meetings, and the work itself published by the authors, are all means of access to the aforementioned dimensions of science. The principal focuses of attention of the above groups, apart from the persistent methodological concern, are: the evolution of domains and technology, the eminence and impact of foreign authors and movements, the influence of political, ideological, and/or social factors in the development of the discipline, and the studies of authors (especially Germain and Mira) and national traditions (especially Spanish).

ANALYSIS OF MATERIAL

The analysis of the content has been inspired by the categorical system used by Tur and cols. (1983) for the analysis of the "Journal of the History of Behavioural Sciences". This system was later maintained in order to make easier comparisons in the analysis of diverse sources, such as the "Revista de Historia de la Psicología" (Journal of the History of Psychology - Lafuente and Ferrándiz, 1991) or the "La Práctica Historiadora Española Contemporánea" (Practice of Spanish Contemporary History" - Tortosa and cols. 1991). An inclusive classification criteria has been used so that each article can only appear in one category.

The thematic spectrum is restricted in terms of general categories. The broadest and most methodological categories, being History of Psychology in General, Teaching of History and Historiography and Method, do not accumulate any articles. The category dedicated to authors, on the other hand, acquires a great number of articles, with the only subjects approached being researchers of the Spanish language, and the acceptance of various ideas of foreign researchers. The significance of the distinction of the 'School', of great influence in the historiographic models dealt with by North American historians, is practically non-existent here. This is due to the fact that references to these models i.e. psychoanalysis, behaviourism, human psychology, and neo-scholastic, are always made in the context of the acceptance of the development from the Spanish point of view. However, on the other hand, the significance of History, as can be expected, is great in the following specific areas of psychology: psychiatry, clinical, armed forces, traffic, education, and social/organisational. Finally, the History of institutions and means of communication has received a great amount of attention, not only in journals and dictionaries, but also in various institutional developments. Nevertheless, the conditioning variable has been the 'national tradition', and practically all of the studies embody the common factor of the Spanish perspective. This is probably the result of a concern to justify a scientific and professional tradition, a long time since embedded in Spain, but which has been continuously controlled and ideologised by political stances and groups. Therefore, there emerges this persistent interest of those who need to justify a disciplinary and professional tradition which has existed a long time, but suffered serious socio-labour problems. Spanish historians appear to have focused upon surpassing the simple author-school mechanism, (or vice-versa), and have, instead, concentrated on more elaborated historiographic models, combining those which dominate the constructivists, those which are supported by models of change, (which are today's classics, i.e. Kuhn, Lakatos and Toulmin), and those with quantitative methodologies.

The historical literature found in the professional framework, limited by a Society which defends a strict, radical protectionism, does not contain exclusively obituaries or 'ceremonial nostalgia', lacking in creative criticism. Or is it motivated exclusively by internalist or justifying urges, or show a scarce, acritical and irrelevant use of the sources available. This, however, is to be expected within a more professional, scientific and institutionalised conception of the practice of History, as that which exists today. This organisation, generally-speaking, has maintained a profound interest for the field of General Psychology in terms of its theoretical, conceptual, methodological, biological, epistemological and historical aspects. This could have lead to an instrumentalisation of History with objectives concerning only the present. However, the development of this speciality and the professional level of its practioners, has made way for an important development entailing the complex scientific-professional episteme that Psychology is today.

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ZSUZSANNA VAJDA:

Change of the Moral Paradigm in Education -
a Comparison in the First and the Second Half of the Century

ABSTRACT

Education of the past - until the 50's of our century - was often globally labelled with the domination of authoritarianism, and that is why it is not accepted in our time. In this paper I want to argue, that the major changes in the childrearing didn't come up from decreasing of the authoritarian spirit, but they took place because of transformation of the moral paradigm. It seems, as if in the interwar period there existed a common faith in a world of constant, unchangeable values, which made actual bad experiences of people accidental - and temporary. For the education of our days this common faith in abstract moral values is lost because of different reasons. Its disappearance might have a strong impact on the childrearing, the self-reflexion and the mental functioning of the new generation.

ZSUZSANNA VAJDA:

Change of the Moral Paradigm in Education -
a Comparison in the First and the Second Half of the Century

Education of the past - until the 50's of our century - was often globally labelled with the domination of authoritarianism, and that is why it is not accepted in our time. In this paper I want to show, that decreasing of the authoritarianism had already begun at the turn of the century. I want to argue, that the major changes in the childrearing didn't come up from that, but they took place because of transformation of the moral paradigm.

Changes in childrearing have not only historical, or scientific importance. The child's relations to the world outside and to his own self are formed under the influence of his human environment. In this way education really must have an impact on the functioning and structure of the society - at least those, who wanted to reform the education always were and are now convinced of this. The illusions of the followers of psychoanalysis and the leftist movements in the 60's concerning the revolution of the childrearing are well known. These illusions /or maybe real expectations?/ appear again and again. An example of this is Carl Rogers, the creator of the humanistic education. In his work "Power or people?"¹ he states, that the people developing under the influence of humanistic education may become a significant factor in politics. Gordon², the maker of the parent and teacher effectiveness training - it is very successful in Hungary now - writes in the preface of his book for parents, that the spreading of his method, the understanding listening to the child, may help to solve international conflicts as well.

Let us sum up briefly, why it happened, and what was the essence of the change of the basic concepts in educational sciences?

One of the basic experiences of the twentieth century, that the

development of our culture wasn't followed by the radical changing in the human behaviour and interactions, didn't make them more constructive, and it wasn't possible to eliminate war and inequality. This experience strengthened the doubts, formulated by Rousseau in the time of enlightenment: does the adult society have the right to force the new generations to accept its values and disfunctioning moral principles?

Rousseau set the children's innocent soul, what he considered to be the original human nature, against the global critics of society. The ideal of natural human was discovered again in the turn of 20th century, when spreading of the measuring and experiments in the social sciences, the new results of psychological research informed about qualities of the children's mental functioning, which were not known before. Experiences and observations proved, that the children's mind functioning is in fact different from that of adults and it is autonomous. These new discoveries offered a solution for educational sciences: to put the childrearing and teaching on the base of natural sciences, that is on the base of positive facts.

But as childrearing concerns the socialization, educational sciences always imply antropological preconceptions, however much facts we know on children's development. Interesting to note, that Freud and the representatives of conservative education equally were convinced, that children's real nature is originally asocial, and even antisocial, that is opposed to the rules of society. But their conclusion was absolutely different. Conservatives' opinion was, that the purpose of the development of the individual is an adaptation, creating the harmony between man and society. Freud didn't disputed that the individual must adapt to the society either, but he didn't believe that a harmony can be realised as a result; life is a permanent struggle between individual and society. His leftist followers - among contemporaries for exemple Reich, Bernfeld, the

Hungarian Béla Székely and the poet Attila József - thought that the harmony can be created, and it is necessary, but for it's sake the society must be radically changed, for it would be more suitable for the natural human properties. This point of view was accepted - explicitly or implicitly - by the later representatives of psychoanalytical and modern liberal education as well.

But whether we can accept the view, that the autonomy of children's mind reflect the real human qualities? And can be children able with these inborn properties using minimal influence of the adult world - by the principles of autocreation, represented by the followers of psychoanalytic education - to create preliminary conditions of communal, social life? From the other side whether the requirements of social morality are to be globally refused, or there are only some requirements, or some societies' requirements which are really harmful? The main differences between education of the first and the second half of the century are connected with the different answers to that questions.

At the end of the last century with the movements of New Education and other reformist movements the importance of knowing the children's mind was evident also for conservatives. But it wouldn't bring about a change in moral paradigm itself. In this time almost all professionals of education strived to limitate authoritarianism and grown ups' domination in childrearing. But in the definition of the principles and purposes of the education was used a social, or moral frame of reference. Extension of knowledge, which helped the authoritarianism loosing its positions, at the beginning served also the purpose to mediate social requirements to the child more effectively and to make accept them easier. Yet later in the modern childrearing mental functioning itself became a central question.

Let us see now some illustrations for that from some works on education in the first decades of the century. Antal Frank, one of the

well-known, conservative experts of the education, wrote a book with the title "Well behaved children - naughty children"³ for parents. The titles of chapters are formulated in the same spirit: "Fibber child - honest child; laisy child - industrious child; obedient child - disobedient child. The author states, that absolute obedience is a requirement, he believes, that a child cannot be allowed to criticize his parents' actions. But he calls the parents not to be cruel and too much demanding. He doesn't agree with cheating the child, for exemple punish him after promising him forgiving, if he confessed his mistake. Frank also points out, that if a child is lying, the parent has to eliminate the causes. Also he considers very important the exemple of parents.

We can find similar views at the Hungarian followers and representatives of reformist directions, for exemple the School of Activity. One of them, Erzsébet Baranyai⁴ writes, that the in the past childrearing was a kind of struggle, the "separation of the interest of the present and the future". But this opposition is not necessary. Teachers, she writes, must win the child for their our purposes instead of employing force. Another author, Béla Ambrus⁵ in his paper criticises parents, misusing the fear of the child. In the 30's the journal of the movement of New education had a debate on beating the children. Most of participants - professionals of education from different schools of thoughts - sharply criticise beating and refuse to use it in their practice.

A definition of personality by another wellknown expert of that time, Sándor Imre shows, how much the moral values were present in education. By his definition "Personality is a moral character, that is a conscious and nobile individual". Quite a lot of authers used this definition in their works.⁶

It is worthwhile to mention, that however the dominating moral of the era was Christianity, from these writings we can see, that the

concept of morality, used in the education is more abstract and more general. Authors usually speak simply about "moralistic" man, and it seems to be evident, that this notion meant the same thing for everybody. Gerő Bárány ⁷ in his work on the role of ideals in the education emphasises, that it is morality which connects individual and community.

It is interesting to examine some parallels in the methods and concepts, which can be found in the views of reformist movements, psychoanalytic education, which was rather close to them, and the conservative education. Behind these apparent similarities there are fundamental differences.

For example happiness is an ideal equally for conservative schools of thoughts and for reformers. But it can be realised by their views in completely different ways. For reformers and psychoanalists it means the mental health. By that they understood that the person isn't forced to subordinate his individual desires to the requirements of the society, the world outside, more, than it is necessary. /The measure, what does it mean, "necessary" belongs to unsolved problems of these branches of science/. In contrast, for the conservative education happiness depended on merits. Happy person is, who has a clean conscience; those are worthy for the happiness, who merited it by their behaviour. By the statement of Imre Sándor, mentioned above, happiness is calmness of the soul. By him happy person is, who is healthy, who has an independent thinking and clean character. This is the point, where we must take note to something, which is a part of the basic change in the views, and which has its role in the different way of self-reflexion. If the happiness depends on merits, it depends also on the will of the individual. But if it means the mental health, it practically cannot be influenced by him, as the mental health by the actual opinion of the science depends mostly on the behaviour of the parents and environment.

A similar problem is the importance of self-awareness, which was accepted by all school of thoughts, which otherwise had opposing views in other fundamental questions. However their definition on it was completely different.

Antal Frank, cited above, wrote a book under the title "Self-awareness"⁸. Giving reason, why self-awareness is important, he uses the arguments, very similar to the views of psychoanalysis and new education: he says, contemporary education deals with only symptoms instead of causes. Frank said, the children's mind is a labirint, and he points out the risks of discovering of that labirint: "If we don't descend into the labirints of children's thought, we cannot find the reason for acts, and if we descend there, missing the light of intelligence we can easily stumble" - he writes. But, as we can see from the self-confessions, cited by him, in that case there is no place for free association, for narrative of the life-hystory, for the analysis of inner motives. The question posed is not the relation between acts and words, neither is the hidden motive for acts. For conservative education self-awareness means, that the individual analyses, how much his behaviour meets the requirements of an abstract world of moral values. Whereas self-awareness in psychoanalitical /and nowadays/ sense means comparing two mental functions to each other. We must note again, that the first - conformity to rules, meeting to requirements - can be a choice for the individual, since in comparing his inner mental processes to each other the person can be only spectator or analyser.

Psychopatology of the child at the twenties and thirties is also inspired with moral criteria. The main attribute, used in it is the capacity of the child to be socially adapted. By the opinion of Sándor Szanas⁹, director of the state's asylum for children, those, who grew up in antisocial environment, are healthy also if they are antisocial too, as health means a good adaptation to environment. That is why the

antisocial behaviour must be evaluated taking into account, whether the earlier environment could cause it, or it couldn't. In conclusion, Sándor Szana is quite optimistic concerning the prognosis: as soon as the child, who became antisocial because of his environment, is put into a new one, he will change his behaviour. The standpoint of the psychology and pedagogy of our days is almost opposed to this, telling, that grave traumatizing effect of the early environment is not correctible.

Jozsef O. Vértés,¹⁰ an expert of so called "healing education" /in our days it means only the education of mentally retarded children/ wrote a couple of books about mental disorders of children. In his diagnostic categorization hysterical and nervous children belong to a group of "people with abnormal life-objectives". Epileptics, postencephalitics and those who have chorrea are qualified as "people with abnormal character". We cannot even imagine these categories in a modern book about mental disorders. Professionals of our time focus on the disorder of a certain function /mental function, emotional function, etc/. They suppose to be a "normal way" of emotional functioning, which has something to do with the adaptation, but they never compare it into the whole, or possible life of the children. Healing the mental disorders by our views can be influenced by the will of the person only through complicated ways: we think, the symptoms of hystery or phobie can be eliminated, if the ill person with the help of an expert discovers its hidden causes. In contrast with that József O. Vértés writes: "A mother, who is forced to work for maintaining her children, won't think of producing symptoms, like tremble, to be paralysed or mute". Thus he supposes that there exist a kind of choice for the individual also in these illnesses. In the same time József Vértés writes, that the hystery can be acquired and inherited as well - so he supposes an organic cause for them. But for him it didn't exclude, that the intention of the individual may have a

role in producing symptoms.

As we could see at Sándor Szana, teachers more optimistic concerning the success of education or psychological intervention. We can find a lot of illustration for that. Gizella Stelly in a textbook on education¹¹ in 1924 definitely stated, that every child can be educated. "If the mother, or the teacher is clever, the child cannot get the better on them" - she wrote. She also wrote, that God gave the instinct to every mother for caring and rearing her child. Other books on education of that era represent a similar standpoint: if the parent's method is right, he or she can reach the desired effect.

In the journal of the New Education, "On the ways of the Future" we could read wide information about a child guidance clinic¹², worked partly by the methods of adlerian psychology. The author of the review reflects on a positiv critics in the following way: "We are happy to be praised, but we wish even more, namely if nobody would think today, that that's a miracle, if we lift up a fallen, kicking child from the earth, put into him love for work, wipe grimaces on his face, and form a useful little fellow from him".

In contrast with that Gordon, /and other experts of education of our days/ tries to prove the incompetence of parents in child-rearing. Child psychotherapist are also more sceptical concerning the success of their work.

Finally let us see exemple, how looked at their own education two representatives of the past, who grew up in bad circumstances. In the book of Frank Antal "Self-awareness" we can read a confession of an apprentice, who was a very good student, but loosing his father had no other choice, than to go to a master and become an apprentice, when he was 12 years old. In his confession he complains, that the master often beats him. "I spent my days without home, without just one lover heart. Telling the truth, I often rebelled against "People", why I must suffer so much, and to be humiliated ... But after so much

suffering now I start my life with strong will, and with a faith in a better future, and I believe, that the life will bring me the happiness too."

Another self-confession is cited by Márk Goldberger in the journal "Gyógyászat"¹³ - it was a medical journal, where quite a lot of psychoanalysts published their works. Author of the confession writes, that he was very ugly and often ill in his childhood. He was not loved by his mother, and practically was excommunicated from the family. Because of his cruel fate he run away from home at 14, and tried to go on himself. But not on the street, not like a drog-user, or thief, what we would expect now. He started to work, as a child, he studied, and became an esteemed clerk. All in his life he was struggling for his mother's love, and after her death he went regularly to her tomb. The reason: he knew of course, that his mother's behaviour didn't meet the moral expectations; but he didn't give up his ideal on mother-child relation. Also his next phrases merit attention:

"I am loved and esteemed everywhere, because I am really understanding poor people. But I am proud not of this. I am proud, that I can preserve my enthusiasm for every kind of human goodness and eternal beauty".

Contemporary literature proves, that these people were not rare exceptions. It seems, as if there existed a common faith in a world of constant, unchangeable values, which made actual bad experiences of people accidental - and temporary. This can be demonstrated also by an experience of Sándor Szana with antisocial children. He writes, they are talking about their delinquency, as about a foreign thing from outside them, and they feel, they have got it as an illness: "He talks about his delinquent act, as if we talked about a fever-fit".

For the education of our days this common faith in abstract moral values is lost because of different reasons. Maybe I could at least partly support, that its disappearance might have a strong impact on

the childrearing, the self-reflexion and the mental functioning in general. Of course, these changes don't come from the development of educational, or other sciences, but from other conditions of the civilization and the culture. Child-rearing /nor the practice, neither the science/ cannot be independent of them of course. But we have to think of the words of Bruno Bettelheim:

"Only a Darwin, and a Nietzsche, a Freud - educated by a fear-based serious absolutistic moral, and who lived all their life by this moral in a lot of important connections, however they freed themselves from the fear - they could permit themselves to examine this moral with more and more critic. They did it with a mature thought, when it no more can destroy their high ethical standards: their personality didn't brake out into pieces, and they must not retire from the world with an aversion. It was exactly that absolutistic moral, which they have learn't in their childhood, which made them so strong in later periods of their life, that they could reform the world by more matured moral conceptions".¹⁴

NOTES

- . Hatalom, vagy személyek? - két nevelési irányzat
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- . Frank Antal: Jó gyermekek - rossz gyermekek
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- 0. A gyógyító nevelés rendszere.
Kókay János kiadása, Budapest, 1940
- 1. Könyv a gyermekről
Budapest, 1924
- 2. A jövő útjain, 1938/4-6, 135. old.
- 3. A mellőzött gyermek. Gyógyászat, 1929, 42. old.
- 4. A végső határ, 101. old. Budapest, Európa, 1988

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THE ORIGIN MYTHS IN THE HISTORY OF PSYCHIATRY

PATRICK VANDERMEERSCH

Upon first sight it might appear fitting to claim that psychiatry is dependent upon medicine in so far as it evolved from the question of whether psychic disorders can be accounted for physically or not. History teaches us, nevertheless, that psychiatry originated from a completely different conception. Moreover, psychiatry enlarged its field in a period when there was indeed wide-spread belief in biology, but it occurred by the annexation of two forms of pathology for which there was no precise organic explanation: monomania and neurosis. What was it then that brought this diverse set of 'problems' together in such a way that we may consider them as psychiatric 'illnesses'? How did the apparent unity that we find so neatly classified in the DSM-III come into existence?

It is on this point that the *mythes d'origine* shed some light on a part of the question. When one gets behind their effect, one discovers in the end that it is a matter of the discovery of the susceptibility to the transference. Psychiatry was born of the moral treatment, out of the experience of how strongly people are inclined to give up their independence for the esteem of an authority figure.

The classical formulation of the ideal of moral treatment was introduced by Pinel with an account which could be considered as the first myth d'origine of psychiatry. Pinel tells of an incident in which one of his friends was driven by severe depression to attempt suicide. The man was found lifeless in a forest, with one of Plato's dialogues in his hands concerning the immortality of the soul.¹ After this account, which Pinel titled 'Histoire d'une manie où le traitement moral auroit été nécessaire' follows his famous definition of the moral treatment:²

'In the treatment of his mania, it was in my power to use a great number of remedies; but I lacked the most powerful of them all, which one can simply find in a well-ordered hospice, the one which consists in the art of subjugating and taming the alienated, to put it in this way, by placing him in a strict dependency upon a man who, by his physical and moral qualities, is apt to exercise on him an irresistible empire and to change the vicious chain of his ideas.'

mordacht!

When one considers the account, it is difficult to find a single reason in it to speak of the birth of a medical discipline. The same can be said of Esquirol, who further developed the idea of psychiatric institutions and stated that it was absolutely necessary that in the institution there would be one, and only one power figure. The moral treatment however failed.

The prospects of many cures that were proposed didn't turn up. In the desperate attempt to make the moral treatment still more effective, more efficient sometimes cruel means were grasped for.³ Protest arose against the inhumane manner by which patients were administered the therapeutic psychological shock for a cure that never seemed to arrive, especially when the brutality was becoming even more explicitly defended as in the work of Leuret.⁴

In this context two myths were created which from that day forward can be repeatedly found in any historical survey of psychiatry: one about Pinel and the other concerning demonology. Pinel became in the first place a doctor, elevated to the status of a philanthropist. As a reaction against the charges of brutality in the moral treatment the portrait of its founder as the figure who liberated the mentally ill from their chains was spread.⁵ + made them 'patients'

The stories concerning demonology had an analogous mythical function. From about 1840 a number of psychiatrists themselves began to be interested in the history Christianity, and in particular in its darker period in which belief in possession by the devil, in sorcery, and in witchcraft had arisen. Parchappe wrote a long commentary on the *Malleus maleficarum*, the famous book written in 1486 which provided the Catholic Inquisition with rules for the indictment, trial, judgement, and punishment of witches. Calmeil authored a book which employed an aggressive tone and which was to become very popular: *On Lunacy from the Point of View of Pathology, Philosophy, History and Law, from the Renaissance of the Sciences in Europe until the nineteenth Century. Description of the Big Epidemics of Simple or Complicated Delirium Overwhelming the People of Earlier Times and Dominating the Monasteries. Exposition of the Judgements Unjustly Passed because of Ignorance of Madness*.⁶ A Plethora of articles followed in the wake of Calmeil's book, It is in this context that Johann Weyer was made the father of psychiatry.⁷ The message that was sounding through was clear: it was absurd nostalgia to cling to anything that has to do with a religion which in earlier times and in such a terrible manner had something to do with obscurantism, and on the grounds of superstition had brought innocent souls to be burned at the stake. If this is the case, people had better put their trust in doctors...

With Freud and the spread of psychoanalysis in psychiatric circles the field of the neuroses was also recognized as within the psychiatric domain. Psychoanalysis was itself for decades after the 1940s the basic principle that would structure the psychiatric field, and this applied equally to its therapeutic methods as well as its systematic diagnostics. The history of psychiatry was re-written in this new perspective by G. Zilboorg and G. Henry, in which they reiterated and detailed the myths both concerning demonology and those associated with.⁸

Why did these myths continue to exert such a profound influence? For a moment, one could think they would now disappear. W. Leibbrand and A. Wettley brought the theme of demonology back to modest proportion in their *Der Wahnsinn*, and rightly put that it is singularly impossible to write a history of psychiatry as a discipline. The only possible thing to write is a history of insanity.⁹ The same applied for F. Alexander and S. Selesnick, who were convinced that it was only after Freud that one can speak of a 'real', valuable psychiatry. They criticized the way in which the battle against demonology, and in particular the figure of Weyer, were usually portrayed.¹⁰ When H. Ellenberger decided to write a history of psychiatry which would re-trace the footprints of Freud, he nevertheless returned to the demonological theme. Searching the pre-history of the Freudian concept of the transference, Ellenberger looked for its origins in hypnosis and Mesmerism. He returns to the time in which Mesmer was placed on a commission in Bavaria to give an opinion on the famous exorcist Gassner.¹¹ It is here that Ellenberger makes use of an explicit use of a *mythe d'origine*: 'The emergence of dynamic psychiatry can be traced to the year 1775, to a clash between the physician Mesmer and the exorcist Gassner.'¹²

Continually again you find in the textbooks: psychiatry has arisen out of the fact that illuminated minds left the obscurantism and the blind faith of religion behind them. There after the illuminated minds were the real philanthropists while religion, in spite of all appearances, housed much violence. Admittedly, it is often put less brutally. With regard to religion a 'benevolent neutrality' is the polite position to take. However, even today the statement can appear rather brash. Whoever takes the last edition of the *Comprehensive Textbook of Psychiatry* in hand may be surprised how the preface thus begins: 'It is a curious truth that the vast majority of scientists who have ever lived are living now. The fact reveals two important facets of science: It is relatively young, and its recent growth has been exponential. Historically, there had long been a tension between scientific inquiry and theological truth. These latter truths were often revealed and not subject to rational inquiry. The church was far more powerful than science, and it held the position that if logical inquiry contradicted revealed truth, then the results of the logical inquiry were false.'¹³

The complete text will appear in a book on the historiography of psychiatry that will be edited by R. Porter and should be published in 1993.

Indirekten:

- geschiedenis vd waanzin
+ praktijk



Interpretation of Freud's seduction-theory in Fairbairn's
theoretical system

(Abstract)

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Over years the cancellation of the seduction-theory theoretically and from the standpoint of the practice too, become sources of severe mistakes. A psychoanalyst had to undertake to gain patient over the fact that the experienced trauma wasn't a real one but a product of fantasy. Masson (1986) thinks that the cancellation of the origin seduction theory scientifically is unfounded; on the contrary, data showed by him and a number of new reports on the revealed cases seem to support the justice of the origin version of the theory. On the basis of these he rightly concludes that the concept and methods of neurotics' therapy should be altered. By ignoring the traumatic experience cannot be treated the ^{disease} decease. The Masson-research and searches made by others in this field indicate that the discussion of seduction-theory doesn't come to a rest up to now.

In this lecture we try to explain: what might be the cause of periodically flaming scientific discussions relating to the cancellation of origin conception?

In our view the answers to this questions might be through the analysis of object-relations from the standpoint of theology. Separating in part from the frames of traditional psychological interpretation might be terminated the vicious circle by which could be seen only the scientific scope of abuse if we elaborate a specific aspect of William Ronald Dodds Fairbairn's theory.

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In this lecture we try to explain: what might be the cause of periodically flaming scientific discussions relating to the cancellation of origin conception?

The insufficiency in the explanation of Freud's early theory and its 'substituent' that is the imaginary seduction is that the participants try to resolve a non-psychological but also non-scientific problem with the theoretical, experimental and other apparatus of the science in psychology. If the problem epistemologically is transplanted and reformulated into an other level of human existence (ethical-logical level or transcendental one in different formulation) could be answered two significant questions.

1. What is hidden in the function and content of the human psyche by the seduction-theory when a non-scientific question is presented as a scientific problem: whether is accepted the non-scientific origin of the prohibition?
2. In what respect is closed the way (both versions) from that knowledge by the seduction theory that the psychological existence of human being is intermingled with the other dimension of human existence that is with the need for experience transcendence by the classical psychoanalytical theory.

In our view the answers to this questions might be through the analysis of object-relations from the standpoint of theology. Separating in part from the frames of traditional psychological interpretation might be terminated the vicious circle by which could be seen only the scientific scope of abuse if we elaborate

a specific aspect of William Ronald Dodds Fairbairn⁴'s theory. Fairbairn's system in its final form was got ready in the end of 40's years. Unique characteristic of his thinking partly can be explained with the fact that the reaction also in the psychoanalytic movement was sensitive to the atmosphere of cultural differences⁵ in the end of 19th century and in the middle of 20th century. The consequences and scientific methodological limitations of the dominated physical, helmholtzian world-concept, in about 1880 when Freud began to work, was not accepted by Fairbairn. In his view the essential problems of the human existence are the anxiety and the guilt.⁶ His main objection against the psychoanalysis was that in the instinct concept is precluded the derivation of ego from human relationships. It is difficulty in Fairbairn's understanding that there are some debatable point in his notions about early stage of personality development (the phenomenon are interpreted immediately from the new-born age). For example, how can be made statements on the events in the earlier stages when these statements mostly are unjustifiable scientifically. This problem will be solved if we accept the possibility of hermeneutical analysis given by Fairbairn. This system of ideas includes the possibility of the verification but this become possible in the metaphysical approach. According to Fairbairn's view it is a false intellectual concept to evaluate the dogma and symbolism of the religion as certain kind of facts in the scientific statements. If the scientific approach is primarily made to its

object with intellectual instruments then the personal experiences are primarily used in terms of emotions (the personal relations are examined with the help of emotional experiences) by the religion. Fairbairn as a theologian discovered in the religious terminology of salvation-history the natural and simple expression of people's feelings about own personal problems. For him the religion is an impressive action and experience passed through during the centuries. According to his view the religion in its goals and processes assures more obviously analogue with the psychotherapy than the education and the science. In his notion the object-relations are causal and determinant factors in the personality development. They represent the substance in the human existence and clues to the understanding of personality phenomenon. Fairbairn in his early writing⁷ points that the natural wish for good objects as a primary motivation how easily might be flown toward religion. According to Fairbairn the religious experience is a kind of manifestation in the human nature which roots in the existence of a primary need to a good and personal object (finally the God). In the light of this it's understandable that in the psychotherapy the essential factor is seemed the religion rather than the science, the focus of its system is on personal relation rather than on impersonal knowledge and technique. The power of the good object-relation is preserved by the religion and the knowledge in the absence of which the love might be ineffective is assured by the science (Paul's Roman to

Philippians, 1,8-10).

1. Fairbairn's interpretation of the psychopathology as a moral deficiency represents a more extended use of the concept of fault than the Freudian psychoanalysis. The fault isn't identical with the child's sexual and physical abuse (though they are faults too) but fault is all kind of events in the first object-relations whose spirituality are opposite to the categorical imperativus.

2. Fairbairn is probably an unique thinker in the 20th century psychology with his ambition that the moral development is started in the earliest stage (far before the oedipal stage) moreover (on the basis of theoretical-theological considerations) the ability to moral decision is an inherent phenomenon. Therefore can be presumed that in the system of the object-relations the diffuse anxiety on guilt is present in the early oral phase of the premoral stage.

In Fairbairn's personality concept the essence of psychopathology is that subegos are split from the homogeneous ego and are got to the unconscious. The dramatic situation called intrapsychic reality has sociological consequences. If for example the sadistic anti-libidinal ego is developed in the unconscious because of the bad object-aspect of a very rejecting parent the person in his adulthood because of the infantile dependency probably will be inclined to sort out relationships in which

playing rejected and pursued role too. (See personality formation of victims in incest! [Owens, 1984; Becker, Skinner and Abel, 1982; Nasjleti, 1980; Condy, Templer, Brown, Veaco, 1987; Simari and Baskin, 1979; Spinetta and Riger, 1972; Robinson, 1982; Brown and Finkelhorn, 1986; Lukianiwicz, 1972; Faria and Belohlavek, 1984; Bagley and McDonald, 1984; Wooley and Vigilanti, 1984; Wilbur, 1984; Bowman, Blix and Coons, 1985; Hyde and Kaufman, 1984; ect.]) Or in the contrary: the person because of the persisting infantile needs will be inclined to play pursuing and rejecting role in the relationships vis-à-vis others. (See personality formation of the perpetrators! [McIvor, 1986; McIvor and Duthie, 1986; Groff and Hubble, 1984; Scheurell and Rinder, 1972; Seghorn, 1970.])

On the basis of described above let's see some hypothesis in relation to the validity of the revised seduction theory:

A/ The life-historical event underlying the development of the later pathology is linked to the oedipal situation by Freud. In turn our view is that not a concrete situation is the point in question and the later pathology is established already from the beginning of earliest stages in the development.

B/ In the case of Freud the psychic events underlying the pathology are referred to the sexual sphere. On the other hand, agreeing with Fairbairn, the pathology is founded by the moral

insufficiency. The fault has a more comprehensive meaning than might be limited to the phenomena of sexual abuse or physical one.

C/ Freud took his personal unconscious before a self-created moral tribunal by cancelling his seduction theory and making his self-analysis.

D/ Freud frightened the etiology of the deceases by transplanting the causal pathology (the fault) into the sexual sphere in a way having causeless and incalculable consequences.

On the basis of notions formulated under the points A, B, C and D might be found explanation why the discussions on the seduction theory isn't resting. Probably because the answers are not searched in an adequate place.

Notes

1 According to Freud's theory the traumatic experience is a sexual-like manipulation and a neurotic-like act relating to it. The perpetrator is the child's parent or caretaker or other adult entrusted with control him or elder sibling or relative or teacher. He had reported about his finding in Vienna on the conference of Psychiatric Neurological Association on 21 April in 1886, after was published it in a French scientific review too (Revue neurologique 30. March 1886. under the title 'The etiology of inheritance and neurosis'.) In 1905 Freud cancelled his seduction-theory in public because he was convinced by his experiences gained from 1887 to 1903 of his female patients reporting about traumas that they deceived both themselves and Freud. (Freud explained the cancellation on the basis of the following observations: in the childhood the masturbation is a frequent event relating to the incomplete work of memory. So the female patients produced only the product of their fantasy when they reported of their seduction by their father. This manoeuvre served the elimination of the masturbation-guilt whose essence

was that the consideration from the own infantile sexuality was directed to the imagined victimiser. According to Freud the child was led by the revenge because the work of imagine is a derivation of the oedipal situation: take vengeance on the envied and hated parent, the genital-like delusions are the consequences, rather than causes of the hysteria, the hysteric patient invents such kind of trauma to draw attention and care of his or her surroundings upon oneself.

② While Masson the cancellation of seduction-theory explains through motives being beyond the science (he was anxious about separation, he was afraid that his colleague and closest friend Wilhelm Fliess will be abandoned too if he sticks to the 'father-theory' which was accepted like a stumbling-stone), for him the construction of psychoanalysis is a false idea, a pseudo-scientific method for translating the real trauma into a never-occured one. The analyst spontaneously cooperates with the person caused the trauma; he reproduces the attitude of abuser.

③ Masson refers to Wilhelm Fliess's son's, Robert Fliess's book was published in 1973 whose title was 'Symbol, Dream and Psychosis' and also urges on recalling Freud's early seduction-theory in the analytic praxis. In the book (Freud and His Father) of Marianne Kühl published in 1986 concludes that the cause of cancellation of the seduction-theory is to be found in the relation of Freud with his father. Peter Svailes makes interviews with still living Freud-patients, and he visited Wilhelm Fliess's daughter, Pauline Jacobson living in Israel who showed for Svailes the Fliess-inheritance placed in Hebrew University. Svailes reported on his results. (Svailes: Freud, Minna Bernays and the Conquest of Rome. New Light on the Origin of Psychoanalysis, The New American Review, Spring/Summer 1982).

④ He is a thinker of English neo-psychoanalytical tendency. He was theologian, physician and philosopher (1890-1964). He got his psychoanalytic education at Ernst Connel, Melanie Klein's theory on early structuralisation of the spirit was determinant for him, especially the concept of internalized object led him to unique ways within the psychoanalysis. On the basis of analysis for patients with schizoid personality was established his theoretical standpoint in the end of 40's years: Freud's libido theory should be replaced by his personality development theory as well as the function based on ego-structure established without experience should be replaced by facts. Because of his reneger view he became separated as psychiatrist and psychoanalyst; to our knowledge it wasn't established any structured scientific school on his fragmentary work. References of his main works:

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5 Guntrip, Harry: Personality Structure and Human Interaction. London, Hogarth. 1961. 246-257.

Pay special regard to the sub-chapter 'The Attitudes of Freud and Fairbairn to Science and Religion'.

6 See at the same place p. 248.

7 Fairbairn, W.R.D.: Notes on the Religious Phantasies of Female Patient (1927) In: An Object Relations Theory Of the Personality, Basic Books. New York 1952. 183-197.

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Challenges to the self. Empiriocriticism (Avenarius, Mach, Ziehen), psychomonism (Heymans) and the first "crisis of psychology" (1900) (Abstract) by G. Verwéy (KUN, Nijmegen)

Present day's "crisis of psychology" as it takes shape in the border region of cognitive science and philosophy (of science, of mind) suggests that the issue about the autonomy of psychology is primarily related to clashing ontological commitments of the parties in the debate. In the first crisis of psychology (cf. R. Willy, "Die Krisis in der Psychologie", 1899) it was not the ontology of physicalism (as in the present crisis), but of "psychicism" as it was (or seemed) implied in varieties of empiriocriticism (Cf. Mach's "Empfindungsmonismus") and in psychomonism (Heymans) which caused the division. Historically and systematically related to the Spinozistic paradigm (via Fechner) these monistic positions supported developments towards psychologies without self (cf. Mach → Skinner (1931); Heymans → the philosophical theosophy of J.J. Poortman (ca. 1930)). The criticism of opponents (in the debate about psychologism) - Husserl and the Neokantian epistemologists (Windelband, Rickert; Natorp, Cassirer) - in point of fact implied a vindication of the ontology of the self, basic to an autonomous scientific psychology.

Challenges to the self. Empiriocriticism (Avenarius, Mach, Ziehen), psychomonism (Heymans) and the first "crisis of psychology" (1900).

Paper presented at the Cheiron conference,
Groningen 1992, by G. Verwey

Being part of Europe's cultural crisis (1880-1920), scientific psychology in the decades around the turn of the century stands in the crossfire of quite a number of theoretical controversies affecting the very foundations of its thematic and methodological identity as well as its ontological presuppositions. I refer to the debate about the object of psychology (Brentano, Wundt a.o.), about its method (Ebbinghaus vs. Dilthey; Binswanger vs. Jaspers), the controversy of elementaristic associationist psychology vs. "holistic" approaches (Wundt vs. Mill; Külpe, Ziehen vs. Wundt), of introspectionism and Machian positivism, and last but not least, the so-called parallelism controversy (Heymans, Paulsen, Eissler a.o.). At the same time conflicts like that of monism vs. dualism resp. theism, and (Christian-)religiously inspired personalism vs. materialism enhance the impression of universal crisis and intellectual and spiritual division, quite apart from whatever also in that time in the sphere of political, economic and social events may have contributed to seeing the epoch 1880-1920 as a period of crisis par excellence.

The first to write a book about "the crisis in psychology" (from an empiriocriticistic perspective) was P. Willy (1899), and ever since the literature about the crisis in or of psychology has been steadily growing as the young science moved on from crisis to crisis up to the present day, its first crisis about 1900 being followed by a second one during the Weimar republic, lasting from 1920-1933, after which came in psychology's "age of

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theory", that is, between roughly 1930 and the early '50s "a period of relatively bland crisis" (S. Koch 1975), succeeded by a number of more severe crises about every ten years, up to the 1990s. Indeed, S. Koch in 1975, reflecting on the "current 'crisis' with respect to alternate paradigms in psychology" makes a point when he contends that crisis is endemic to psychology (S. Koch 1975: 477). Scientific psychology in its infancy may plausibly be said to suffer from childhood diseases, but after a hundred years or more one is inclined to reject the developmental analogy implied in "crisis" and "childhood disease" as superficial and tries to get a deeper, i.e. structural understanding of what is at stake.

Present day's crisis as it takes shape in the border region of cognitive science and philosophy (of science, of mind), especially in the controversies surrounding eliminative materialism and connectionism vs. "folk psychology" (P. Churchland 1991; Fodor/Lepore 1992 a.o.) may give us a lead. It suggests that the issue about the autonomy of psychology - central to every crisis in psychology in the past century - is primarily related to clashing ontological commitments of the parties of the debate. The paradoxical nature of the claim of an autonomous psychology becomes manifest when one considers that for psychologists with a physicalistic bent psychology can only be scientific if it cuts itself loose from its folk psychological roots, that is, when it reduces to neurophysiology and ceases to exist as an independent, "autonomous" scientific discipline. On the other hand, psychologists with anti-physicalistic convictions are landed with the problem that an autonomous scientific psychology, which maintains (some) continuity with the concepts and modes of explanation or understanding characteristic of folk psychological common sense,

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runs the risk of becoming un-scientific according to prevailing standards of scientific methodology. Psychology's susceptibility to "crisis" rests on the fact that it wants to be both - autonomous and scientific, and as to most psychologists today the autonomy claim entails a commitment to a non-physicalistic ontology, whereas the acceptance of the prevailing scientific methodology is taken to imply subscription to a physicalistic ontology, the problem is absolutely insoluble. The result is a structural instability, psychologists being caught in an inexorable swing of the pendulum, moving between the two poles of an un-scientific, but autonomous psychology, rooted in an ontology of the "self", that is, the recognition of a realm of the "inner man", which is irreducible to external reality, and the basis of specific psychological conceptualisation (ego, self, subjectivity), intentionality, rationality etc.); and, on the other hand, a scientific, but non-autonomous "psychology", firmly rooted in physicalistic conviction, which in fact is a "psychology" without self. It is the continuous battle of psychologists/philosophers who oppose the exclusive rights of the third person perspective and those of their colleagues that favour the expulsion of the first person perspective - be it implied in academic or folk psychology - in order to promote the third person perspective to undisputed monopoly.

The focus of my presentation, however, will not be the present "crisis" in psychology, but the first one, that is, the one in the decades just before and after 1900. Unlike present day's problem situation in which the defence of an autonomous psychology centres around the acceptance/rejection of physicalism (as ontology), we find that about 1900 the threat to the autonomy of psychology did not come exclusively or even primarily from

physicalism ("materialism"), but from psychicism (sit venia verbo), that is, from psychological-philosophical positions, which cherished (or at least could be plausibly perceived as cherishing) a "psychistic" ontology. Starting from the psychistic premiss it became possible to conceive a psychology which, though strictly natural scientific in its methodology, was not physicalistic, but (at least without question in the case of Heymans and Ziehen) psychistic - a figure of thought which is virtually nonexistent in the present day debate. The proposition I want to advance is that the empiriocriticism of Avenarius, Mach and Ziehen on the one hand and the psychomonism of Heymans on the other hand are mutually complementary positions that both, in spite of their psychistic ontology, finally led to psychologies without self: empiriocriticistic psychology in time would lead to the behaviorism of Skinner (1931) and the psychomonism of Heymans eventually would result in the philosophy of his pupil J.J. Poortman (about 1930), which represents a philosophical foundation of theosophy. In the first case the self (that is, everything conceptually related to the first person perspective) is eliminated by reduction (that is, by description from a third person perspective); in the second case the self loses its contours as it becomes integrated in the supra-individual world-soul. The net result is in both cases the same: alienation from the first person perspective which is basic for an autonomous psychology.

It is worthwhile to consider in how far both developments towards a "psychology without self", historically as well as systematically, ultimately derive from the Spinozistic paradigm: if it comes to reflection upon the relationship of the mental and the physical, Avenarius, Mach, Ziehen and Heymans, all of them,

in one way or another, develop positions which, notwithstanding their criticisms, clearly stand (via Fechner) in the *Wirkungsgeschichte* of Spinoza's identity philosophy.

Therefore it does not come as a surprise to note that (leaving Wundt's devastating criticism of Avenarius apart) the criticism voiced against "psychologism", as this was (or seemed) implied in empiriocriticism and - partly - in Heymans' views (cf. especially Husserl and the Neokantian epistemologists: Windelband, Rickert; Natorp, Cassirer), in point of fact meant (to vary the title of a book by A. Lovejoy) a "revolt against monism" and a vindication of the threatened ontology of the self. The final rehabilitation of the dualistic perspective was then well under way (following the course of the *erklären-verstehen* debate: Dilthey, Weber, Jaspers, Spranger etc.), in time helping to bring about psychology's second crisis, which lasted from 1920 to 1933 (cf. K. Bühler 1927; H. Hildebrandt 1991).

As to the cultural situation in the Netherlands about 1900, it may be observed by way of aside, that the simultaneous mutually supporting interests in empiriocriticism (a.o. Koster, Julius, Ziehen (1900-1903, Utrecht)), psychomonism, "psychical research" (Heymans, F. van Eeden), theosophy, eastern spiritualism etc., and the extremely intensive reception of Spinoza's philosophy in literature, philosophy/theology, and social/political thought, had the effect of making the monistic outlook for the time being a dominant characteristic of intellectual life in the Netherlands between 1880 and 1920, which, temporarily, drove the older antagonism of Christian personalism and scientific materialism, prominent in the latter half of the nineteenth century, to the background.

Wittgenstein and Psychology

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Abstract

In this paper I would like to draw some very far-reaching consequences from the position Wittgenstein took with respect to language and the foundation of its use in the 'forms of life'. The turn to forms of life implies that the *psychological* or *social-scientific* investigation of meaning-giving should not be restricted to this being a linguistic affair, despite the fact that language is paramount and seems to be the most prominent form of communication among human beings. Meaning-giving (and -taking, for that matter) is an experiential affair. It belongs to peoples' way of going about things in the world they live in.

Wittgenstein can be credited of having pointed out to us that all human experiences, and in particular the discourses about these experiences, are culturally bound and conditioned in so far that both stem from language games which are confined to life forms. Historically, the best of phenomenological tradition forces us to view body and feelings as being part of the forms of life. If one tries to bring Wittgenstein's view a bit further and tries to take his message primarily as a message pertaining to *all* our efforts at an understanding of the forces through which behavior is structured in accordance with the prevailing modes or ways of seeing; in other words if one tries to understand why the world is as it looks, one has to come to grips with what presses people to experience the world in this and not the other way. That is to say, one has to come to grips with what discourages people to challenge their prevailing outlook on things and events. What is it? What causes peoples' tenacity, their holding on to life forms which preclude a change of aspect?

In taking up this general question the analysis of people's culturally informed body comes in. This analysis should also comprise the bodily structured sentiments and the use of language confined to these bodily and sentimental structures. Historically speaking, psychology gets a new task here. Wittgenstein has left us with something that cannot be neglected: psychologists should not just take over his view on language use, use of psychological concepts, his view on 'the grammar of psychological words', but foremost his clue as to what lies at the basis of all this: a genuine reflection on experience in so far this experience is rooted in forms of life, a reflection which should not get trapped into the play of language games for their own sake.

WITTGENSTEIN AND PSYCHOLOGY

In this paper I would like to draw some very far-reaching consequences from the position Wittgenstein took with respect to language and the foundation of its use in the 'forms of life'. The turn to forms of life implies that the *psychological* or *social-scientific* investigation of meaning-giving should not be restricted to this being a linguistic affair, despite the fact that language is paramount and seems to be the most prominent form of communication among human beings. Meaning-giving (and -taking, for that matter) is an experiential affair. It belongs to peoples' way of going about things in the world they live in. A truism as this may seem, the examination of experience along this line has hardly begun.

Already in the early fifties Merleau-Ponty argued for a cross-fertilization of the phenomenology of direct bodily experience in the 'life world', psychology and the neurosciences (Merleau-Ponty, 1953/1963). In a way his message is akin to Wittgenstein's. Merleau-Ponty's concept of the 'body-subject' emphasizes the fact that meaning does not start with a mental consciousness but with a so called 'ego-body' as the origin of the perceiving, motoric, elementary-practical, sexual, and expressive intentions. Merleau-Ponty speaks of a *corps sujet*, which is neither consciousness nor a thing, but manifests a "third way of being" (Merleau-Ponty, 1945/1962, p. 415).

The central thesis of my paper is that this 'third way' can be elucidated by drawing rather radical consequences from Wittgenstein's philosophy of psychology, particularly from his analysis of the use of language and the forms of life of which language speaking is a part.

Wittgenstein's fog

In a recently published text of Wittgenstein's notes on the philosophy of psychology (Wittgenstein, 1988) one can read:

"psychology is often defined as the science of Mental Phenomena. This is a little queer, as we shall see. (...) (B)y this we mean what everybody means, namely, the science that deals with thinking, deciding, wishing, desiring, wondering...And an old puzzle comes up. The psychologist when he finds his correlations finds them by watching people doing things like screwing up their noses, getting rises in blood pressure, looking anxious, accepting this after S seconds, reflecting that after S plus 3 seconds, writing down 'No' on a piece of paper, and so on. So where is the science of mental phenomena? Answer: You observe your own mental happenings. How? By introspection. But if you observe, i.e. if you go about to observe your own mental happenings you alter them and create new ones: and the whole point of observing is that you should not do this - observing is supposed to just the thing that avoids this. Then the science of mental phenomena has this puzzle: I can't observe the mental phenomena of others and I can't observe my own, in the proper sense of 'observe'. So where are we?"

To this question Wittgenstein replied: "in a fog". To add more data will not help much. What can be done, however, is to start a conceptual investigation.

It is almost a fad in psychological circles nowadays to accept Wittgenstein's criticism of psychology's conceptual confusion. "Psychologists are prone to unclarity about everyday psychological concepts and the sophisticated experimental methods they employ fail to deal satisfactorily with the problems addressed", Budd wrote in his study of Wittgenstein's philosophy of psychology (Budd, 1991, p. xii). Therefore, much effort went into the so called 'grammar of psychological words'. Grammar does not tell or explain the effects of language. It is something to be described in terms of the way it directs the use of words. The type of description Wittgenstein was after was not so much one with *definitorial exactness* set as its goal, but one that would result in a clear view. A view, which produces understanding as a way of seeing connections. It ought to be some sort of bird's-eye view, Wittgenstein argues, and he called this view "perspicuous representation", a view earmarking his approach to understanding, his way of looking at things. The hard core of such a bird's-eye view of concepts is achieving mastery in moving about among all sorts of psychological concepts in an attempt to see kinship and differences. We see here Wittgenstein's retreat from essentialism resulting in a search for communalities guided by his influential solution to a more than two millennia old problem, the problem of what is the nature of concepts. What is 'a table', 'a chair', 'a game', a sentence like 'five red apples'? In order to solve that problem one should *act*, so to speak, in accordance with his 'family resemblance' theory. By doing that the fog will disappear. Instead of trying to delineate essential features, he wants us to move about around things and events in the world.

Dilution of the analysis of forms of life

Have psychologists gotten that message clear? I really don't think so. The reason for my hesitation is simply this: Wittgenstein has been ambiguous himself about what is means to move about around things and events. This means that Wittgenstein himself has given an opportunity to escape from what he thought the prime task not just of philosophy but of all efforts at understanding including psychological understanding. He has lined out two main routes. On the one hand he introduces the notion of 'language game'. This notion "brings into prominence the fact that the speaking of language is part of an activity or a form of life" (Wittgenstein, 1953, 23). On the other hand he remained pre-occupied, one could say, with purifying strategies, that is with attempts to employ philosophical analyses for clarification of concepts. Along this latter strategic line the language game approach has become something in itself without much reference to the activity or the form of life of which language use is a part. This second route has given ample opportunity for evasive moves like a defence of relativism or an over-emphasis on the yoking relationship between concepts and the larger language game frame (there were, of course, also quite valuable off-shoots, such as the cognitive theory of prototypical

definition and classification). To my mind psychologists in general have favored this second route at the expense of really taking up the challenge of what it means "to move about among things", that is to say, to come to grips with the role forms of life actually play.

Wittgenstein's attempt to escape from the fog has resulted in a rather sterile follow-up of his initial 'grammar of psychological concepts'-approach. In that regard psychology has been affected by the rather fuzzy reception of Wittgenstein's anti-behaviorism. The Wittgenstein reception in general has been ambivalent right from the beginning, simply because there existed a well-organized circuit of Anglo-saxon analytical philosophy with its strong positivistic leaning in opposition to those who emphasize the moral and anti-scientistic implications of Wittgenstein's philosophy. Among psychologists in particular one can speak of a certain reluctance to really accept Wittgenstein's idea that understanding that consists in seeing connections and "moves around about things" is the understanding which results from a change of aspect. According to Wittgenstein, change of aspect involves a change of life, no more no less. He even suggests that it is a 'Weltanschauung' (Wittgenstein, 1953, 122). But that point is not seriously taken up because it is at odds with the analytical, positivistic spirit of the time. What was taken up was a rather diluted form of his criticism of behaviorism undertaken and maintained among psychologists from the language game perspective only.

Constructomania and narratomania

Consequently, life forms and its concomitant practices and activities, although emphasized so strongly by Wittgenstein himself, escaped serious psychological attention and analysis. By and large, the fog remained. One may even say that because of the coming into being of a somewhat more liberal scientific climate in which positivism was disavowed as a result of the adoption of the language game tactic, the confusion became even larger. For we now are confronted in psychology with the so called social constructivism (SC). In SC phenomena in the man-made world -social, psychological or whatever- are 'de-constructed', that is to say, their social and historical origins are highlighted. It implies a perspective on language in which the formative and rhetoric aspects are considered to be much more important than its referential and representational aspects. That is another result of the Wittgensteinian language game approach.

SC suffers from two serious shortcomings, apart from such philosophical issues, whether SC is tenable in view of so much criticism of the way it attacks naïve empirism and realism (Bem, 1989, p 171 ff.). One is that social-constructivism leaves the underlying life form structures untouched. They are taken for granted too much, to say the least. They are not analyzed in their own right. Therefore, language and the use of concepts remains paramount, whereas it is well-realized that there is more to the

experience constituting practice of people than just the use of verbal expressions or forms no matter how important language and the use of constructs, of course, are and always will be. The second one is that social-constructionism is 'body-less'. The neglect of the body is largely due to a narrow standard-biological definition of the human body as 'organism'. SC has placed itself in opposition to a body thus conceived. As early as Berger and Luckmann's first articulation of a social-constructionist perspective, SC took issue with a fixed biological substratum. Many social-constructionist have made the same point. But by being so vigorously against biology the 'body in the mind' or the 'embodied mind' is virtually unknown and left unanalyzed. Because of these two neglects constructivism easily can derail in an over-emphasis on stories told and metaphors lived by. It fosters narratomania as a by-product of constructomania.

The question now to be raised is, whether such is Wittgenstein's legacy. Is that what his emphasis on language use and the way this use is restricted by the language game as part of the forms of life amounts to? What I would like to do next is to show that the emphasis on non-linguistic action as the birth place of meaning requires the lining up of Wittgenstein's thinking with that of the best of the phenomenological thinkers about the so called 'life world'. Proceeding this way Wittgenstein's fog may really disappear and a new skill in dealing with "perspicuous representation" can be mastered.

Forms of life and the life world

I already mentioned Merleau-Ponty. He is the first philosopher who has critically examined the terms in which the crucial problem of how one gets hold of one's own awareness of 'being in the world' always has been expressed. He challenged the idea that the Ego can be looked upon as a consciousness that knows itself by means of reflection. Is it really true that I conceive of myself primarily as a consciousness, that is as an interiority of consciousness which is perfectly transparent to itself, to its own reflection? No, Merleau-Ponty argues. Grip on the world is primordial. That is to say, it is through the not yet personal 'ego-body' that I come to know about the world. Forms of life have a undeniable bodily aspect.

The emphasis on the ego-body or body-subject is often taken as an emphasis on communication and meaning at the so called practical-biological level. Strasser (1969) has interpreted Merleau-Ponty along these lines and claims to have shown that this is a good start but at the end will lead to the same Cartesian predicament from which Merleau-Ponty tried to escape. What Merleau-Ponty did not do, so Strasser argues, he did not give other egos their proper place, therewith neglecting the essentially dialogical nature of phenomenology. Strasser's argument is quite complicated and will not to be fully explored in my paper. I will focus instead on Strasser's attempt to free phenomenology of its still Cartesian and solipsistic features. To that end he turns to the best of

phenomenological reflection and calls Merleau-Ponty's 'ego body' to his rescue. He adds to it the emotional equipment which has its origins in the community of 'caring others' of which Ego is a part. Along this line he wants to keep the spirit of Merleau-Ponty but at the same time avoid its shortcomings.

Strasser is right in assuming that an emphasis on the *primal caring other*, presupposes not just an Ego-body, but a body which is equipped with primal 'feeling structures'. Before cognition as the alleged structuring force, so Strasser contends, there is this structure of feeling which has three important dimensions: *libido*, *securitas* and *potestas*, as Strasser has named them on the basis of thoroughly inspecting empirical (and for that matter "experiential") child development literature. Strasser concludes: "Together with Merleau-Ponty we look upon the body and its emotional equipment as "conditions of reason"(...) They constitute the first sources of giving meaning". In Merleau-Ponty's vain Strasser firstly emphasizes the practical world. In this practical world 'knowledge' is secondary. Primary is 'activity'. From the "we-Ego" of the primary group to which one belongs, emerges the "action-Ego"

So far so good, but then Strasser turns to language and naming again as the prime sources of awareness (ibid, p. 92 ff.), despite his earlier emphasis on the body-subject and its feeling structures. But that view of language, being essentially the view of Augustine, Wittgenstein has thoroughly and devastatingly criticized. Language use and naming originate in the forms of life to which practice belongs. Wittgenstein's lessons on language use need to be drawn into this discussion: Of language can be said at the very most that it co-originate in forms of life to which even those other ego-bodies are subjected, whose caring influence equipped Ego with his/her feeling structures.

To conclude and summarize: Phenomenology too must presuppose life forms, even at its attempt to explicate 'being in the world' in language. Husserl seems to have been aware of this problem, because he wants us to move back and forth between our named and labeled world -named and labeled by science in particular, even among lay people- and the 'original' and 'pre-given life world'. To his view phenomenology was capable of such a trick. Strasser saw through this trick as being too theoretically devised and took rescue to Merleau-Ponty's ego body and added the 'we-body' and shared 'feeling structures' to it, but had to turn to language and naming again to guarantee communication between the two. What even a Strasserian dialogically informed phenomenology leaves us with is with only some form of *discourse about* experience. But we know from the best of the phenomenological tradition that there is a body which precedes all this and we learned from Wittgenstein that there are life forms in which even this body and its feeling structures are embedded. What implications need to be drawn from that?

Culture, body and sentiment

Wittgenstein can be credited of having pointed out to us that all human experiences, and in particular the discourses about these experiences, are culturally bound and conditioned in so far that both stem from language games which are confined to life forms. The best of phenomenological tradition forces us to view body and feelings as being part of the forms of life. Wittgenstein, again, has blocked the road to explicate this bodily and sentimental involvement by taking recourse to naming and language as such, apart from the forms of life. So we have to take up the challenge of really trying to come to grips with the forms of life. In order to do that a thorough analysis of the culture-body-sentiment nexus is mandatory.

But let us first try to get an hold of Wittgenstein's scarce dealings with forms of life. A clue can be taken from the way he analyses 'seeing as' as a remarkable feature of perceiving. In a chapter called "Change of Aspect" Monk writes about Wittgenstein as being puzzled by a controversy between Goethe and Schiller about the so called '*Urpflanze*'. To Goethe this plant really existed and was no idea. Schiller thought it to be an idea and no object of vision. Both are right, Wittgenstein contended, because 'to see' means both seeing something 'as' in accord with some vision one *really* has, and seeing something out there in the visual field. Both happen before one's own eyes, as in the famous rabbit-duck drawing and in the recognition of the number 4 in a visual nonsense-figure. To this Wittgenstein adds:

"A philosopher says 'Look at things like this' - but in the first place that doesn't ensure that people will look at things like that, and in the second place his admonition may come altogether too late; it is possible moreover, that such an admonition can achieve nothing in any case and that the impetus for such a change in the way things are perceived has to originate somewhere else entirely" (Wittgenstein, 1980, 61).

That is so much to say that a change of aspect requires a change of life. The just cited so called 'somewhere else' can then have its benign effect.

Here we have to leave Wittgenstein, because he didn't carry his analysis of a change of life any further than a plea for personal courage to change one's own life. Many have discerned the mystic here or even the man of religion. Wittgenstein's famous distinction between 'showing' and 'arguing' has to be located in this discussion of a change of life also. But if one tries to bring Wittgenstein's view a bit further than personal courage and longing for religious experience and tries to take his message primarily as a message pertaining to *all* our efforts at understanding of those forces through which behavior is structured in accordance with the prevailing modes or ways of seeing; in other words if one tries to understand why the world is as it looks, one has to come to grips with what presses people to experience the world in this and not the other way. That is to say, one has to come to grips with what discourages people to challenge their prevailing outlook on things and events. What is it? What causes peoples' tenacity, their holding on to life forms which preclude a change of aspect?

In taking up this general question the an analysis of people's culturally informed body comes in. This analysis should also comprise the bodily structured sentiments and the use of language confined to these bodily and sentimental structures. Psychology gets a new task here. Wittgenstein has left us with something that cannot be neglected: psychologists should not just take over his view on language use, use of psychological concepts, his view on 'the grammar of psychological words', but foremost his clue as to what lies at the basis of all this: a genuine reflection on experience in so far this experience is rooted in forms of life, a reflection which should not get trapped into the play of language games for their own sake. That is at least the most serious and valuable part of Wittgenstein's legacy. Again, this legacy should not be interpreted solely in terms of Wittgenstein's own search for the meaning of life, for moral responsibility, or in terms of his undeniable longing for mysticism and religious experience. As a human being he had, of course, a life to lead in which he needed to come to grips with the *fin-de-siècle* Vienna, with homosexuality, his Jewish background, the wrong adoration of science and philosophy and all that. Fads and fashions around Wittgenstein abound, of course, but these should not obscure his message to psychologists, a message that needs to be wedded to the best of the phenomenological tradition in order to make the scientific study of culture-bound experience possible. The 'somewhere else' can be taken quite literally: other worlds, other cultures, a view of our own cultural forms among other forms.

One point still needs to be made: culture as form should not be opposed to nature, because then we are seduced again to play the trick of language games, that is to say, the game of 'natural' versus 'cultural' sciences, the game of 'constructs' vs. 'the real'. The opposite of cultural form or forms of life in general is not nature, nor reality, but *damaged form*

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PAPER
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Title of Paper
Patterns of Social Science Discourse:
The Professionalisation of Management and Political Science.

Abstract

The paper deals with the emergence of professional social science in Britain during the period 1945-60. The focus of the paper is related to how social science in practice articulated a concept of 'scientificity'. To what extent was the emergence of political and management science accompanied by a new discourse construction emphasising the scientific nature of social inquiry? In what respects was this new discourse a replication of American social science idioms and techniques ?

The paper will outline a break in both intellectual style and institutional structure in the post-war setting which consolidated a professionalisation of the social sciences in management and political studies.

The paper builds on a methodology paper delivered at the last Cherion meeting at Madrid, 'Reaching Understanding in the History of the Social Sciences'.

scientificity
indicative

PATTERNS OF SOCIAL SCIENCE DISCOURSE:

The Professionalisation of Management and Political Science.

During the 1950's a change of direction was taking place within the practice of political and management education. A new orthodoxy was evolving which indicated a shift in management and political studies to social science approaches. In sum, expectations and assumptions were changing in the academic community, a community influenced by wider social changes such as the emergence of a welfare state and the culture of post-war reconstruction. Commenting on the academic community Norman Chester remarked that the war changed the somewhat isolated 'ivory tower' attitude towards industry and government, "for a large number of academics became temporary civil servants and some remained permanently in Whitehall. Ministers, Civil Servants, banks and many organisations turned to academics for advice."

The paper focuses on leading educators in management and studies particularly Urwick (management) and Mackenzie (politics). In assessing their key roles and the discourses they adopt an attempt is made to 'catch' or illustrate the thesis that a new orthodoxy was developing in some sectors of the profession. A new orthodoxy that in effect created new idioms for management and political studies. By idiom I mean a dialect of a community,

a distinct language which provides form to everyday discourse or conversation within that community. Idiom in a sense represents the dialogue of a particular group and as such signifies taken-for-granted values on what constitutes the proper language of the group whether they be atomic physicists, biologists, or management and politics lecturers. For example the political studies community in Britain prior to the Second World War can be identified through the dialogues and idioms of constitutional law, philosophy and history. In the case of management in the same period, the idioms of industrial engineering including the discourses of productional control and factory planning illustrate a taken-for-granted discourse of management studies.

The post-war period ⁹ indicates a period of discourse reconstruction where the idioms of American social science and business studies, were re-presented, or translated into the British academic community reflecting changes in intellectual style and institutional structure.

The term **idiom** simply refers to what is considered to be proper conversation within a community or institutional setting. It is a term which is useful in illustrating more forcefully the interplay between content and context, language and institutional setting where new discourses are established not only through texts but performed in research seminars, lectures and meetings of the academic community. Consequently new idioms were emerging in political and management studies in the post-war period which characterise and illustrate the changing practices of social scientists.

The emergence of political and management science in Britain is a complex process. The research strategies and expectations of key educators are heavily influenced by institutional settings and wider social and cultural factors. For example the civic university in a post-war context emphasising the scientific and applied, prescribe a different methodological attitude to that of the inter-war period. Evidence shows that the academic community during the 1950s did not replicate the idiom of American social science but instead incorporated some of the discourse into the existing pedagogy. In fact many educators in this period were ignorant of American social science literature or were antagonistic to the behavioural or sociological turn in political and management studies in America.

In the 1950s the idea of politics or management as a science emerged with the growth of civic research networks, postgraduate programmes of study, encouragement of interdisciplinarity and the changing image of social science. These developments are early indicators of the rise of professionalism in the academic community and represent the beginnings of the shift or transition from the 'generalist' to the 'specialist' social science educator.

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PSYCHIATRISTS, PSYCHOLOGISTS, AND THE PROBLEM OF "FEEBLE" MINDS:
SCIENTIFIC CHALLENGES AND PROFESSIONAL CHANGE, 1900-1930

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BRIEF ABSTRACT:

PSYCHIATRISTS, PSYCHOLOGISTS, AND THE PROBLEM OF "FEEBLE" MINDS:
SCIENTIFIC CHALLENGES AND PROFESSIONAL CHANGE, 1900-1930

This paper explores the early twentieth century conflict between psychiatrists and psychologists over diagnosing the condition then called "feeble-mindedness" and later called mental retardation. It focuses in particular on the impact of the ideas of French psychologist Alfred Binet upon American professional practices. Using Binet's science, American psychologists began to challenge some of the professional prerogatives of psychiatrists. At stake were specific social powers, such as the power to determine who ought to be institutionalized, or educationally segregated in special classes, or legally exempted from culpability in criminal trials due to intellectual impairment. By the 1920s, the increasing acceptance of intelligence testing had brought these professional issues to the forefront. This paper will examine the conflicts which ensued as both psychiatrists and psychologists struggled with the problem of diagnosing "feeble" minds.

ABSTRACT:

PSYCHIATRISTS, PSYCHOLOGISTS, AND THE PROBLEM OF "FEEBLE" MINDS:
SCIENTIFIC CHALLENGES AND PROFESSIONAL CHANGE, 1900-1930

In summarizing the medical profession's attitude toward mental illness in the early years of the twentieth century, Dr. Thomas Salmon, then director of the American National Committee for Mental Hygiene, recounted what he called "A Modern Fairy Tale." In an allegory, he portrayed Psychiatry as a Cinderella who had lived unhappily in the "House of the Medical Sciences" until she was finally discovered by the "Prince of Public Favor." Their union was sealed forever, Salmon reported, when this Prince discovered Psychiatry to be a perfect fit--not for a glass slipper, but for a belt that he carried. Inscribed upon this belt was a single word: "Usefulness."

In 1926, Dr. Howard Potter brought this story up to date. Psychiatry, he reported, had since become a mother to two beautiful children, whom she had christened "Mental Hygiene" and "Psychoanalysis." Gossip had it, however, that she had been married once before "and had a child who was not, it seemed, 'all there.'" Finding this child, who was called "Mental Deficiency," a disgrace, she had boarded it out with a family of Psychologists. Strangely enough, the Psychologists had loved Mental Deficiency. Now and then Psychiatry had periods of motherly remorse, Potter wrote, and would favor some attention upon her unwanted offspring. At such times, however, "Mental Deficiency was supposed to be very still" and "not speak unless spoken to and, as you might expect, she was not spoken to very

often."

Such stories say a great deal about professional practices. Salmon's allegory recalls the longstanding historical tensions between medicine and psychiatry. Potter's updated version suggests a newer conflict--one that was beginning to pit psychiatry against psychology. At stake was the "parentage" of the entity then called "Mental Deficiency" or "feeble-mindedness," and later called mental retardation. By the early twentieth century, the problem of "feeble" minds was increasingly drawing both psychiatrists and psychologists into what might be called a professional "custody battle."

Viewed historically, the emergence of such a battle was in itself surprising, for mental deficiency had rarely been seen as a subject worthy of multiple professional claimants. After all, persons called "fools" or "idiots" or myriad other names had been described for centuries; their mere existence, however, had hardly attracted the attention of eager scientists competing for the chance to study them. And whereas the problem of insanity had long captured the medical imagination, idiocy had not. Medical research in institutions for the feeble-minded, one doctor estimated in 1917, was still running about a quarter of a century behind comparable work for the insane. The study of this subject, one medical chronicler recounted, had "not many charms."

What dramatically changed this situation was the invention of intelligence tests by French psychologist Alfred Binet. Beginning with his 1905 article entitled "Upon the Necessity of

Vamat 1870 wordt 7th journal als een
medische kwestie

Establishing a Scientific Diagnosis of Inferior States of Intelligence," Binet's writings offered a psychological challenge to medical science. In the decades that followed, Binet's ideas about diagnosing mental subnormality would profoundly affect American psychological practices.

This paper will explore the professional conflicts which ensued when this French psychological invention reached American shores. It will focus in particular on the ways that American psychologists used Binet's ideas to challenge both the medical understanding of mental deficiency, and the professional prerogatives of physicians. Scientifically, this battle focused on the criteria to be used to distinguish "normal" from "subnormal" mental development. At stake professionally was the authority to assess behavior and to protect or circumscribe political privileges accordingly--the power to determine, for instance, who ought to be institutionalized, or educationally segregated in special classes, or legally exempted from culpability in criminal trials due to intellectual impairment. By the 1920s, Binet's tests had brought these scientific and professional issues to the forefront. They had also begun to significantly transform career patterns, for American psychologists increasingly found their skills as mental testers in demand within medical settings.

This process of professional transformation is clearly illustrated in the career of Leta Stetter Hollingworth, an educational psychologist trained at Teachers College by Edward Thorndike. "I, who had prepared myself for work in schools, found myself working in a hospital--a great surprise to me," she confessed

verschminning Binet-test van onderwijsster → klinische ster

after being hired to fill what became New York's first Civil Service position in clinical psychology in 1914. Hollingworth was employed at the "Clearing House for Mental Defectives," a diagnostic clinic newly established at New York Post-Graduate Hospital by psychiatrist Max Schlapp. A conversation she reported upon assuming her next job--at Bellevue Hospital--shows how crucial intelligence testing had become to her professional identity. "And what do you do?" a hospital physician asked Hollingworth in 1915. "I am a psychologist," she replied. "And what is that?" he inquired. "I give mental tests," Hollingworth answered simply.

Yet if intelligence tests were increasingly gaining acceptance within American medical facilities, they did not do so unproblematically. During these decades, American psychiatrists and psychologists struggled to shape new answers to professional questions. Should tests be administered only by psychologists, they wondered, or could doctors (or schoolteachers) learn to give the same tests themselves? What sort of legal powers ought psychological testers be granted? What should be the relationship between psychiatrists and psychologists working in schools, or in clinics?

By the 1920s, answers to these questions were beginning to emerge as the problem of diagnosing "feeble" minds gained increasing attention from both psychologists and psychiatrists. This paper will explore these answers by analyzing the profound impact of Alfred Binet's scientific ideas upon American professional practices.

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