BOOK OF ABSTRACTS

First Joint Meeting of

ESHHS
European Society for the History of the Human Sciences
(26th annual meeting)

and

CHEIRON
The International Society for the History of the Behavioral and Social Sciences
(39th annual meeting)

University College Dublin, Ireland
25-29 June 2007

MONDAY 25TH JUNE

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Historical Evolution of Studies of Creative Personalities

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In the history of the behavioural, human and social sciences, the psychological interpretations of the structure and dynamics of the creative process only began to have their own epistemologically scientific statute at the beginning of the twentieth century. In fact, the different interpretations of psychodynamic, gestalt, humanistic, factor-analytic and cognitive forms concentrate their investigations in very different dimensions, sometimes conductive to each other and sometimes not, surrounding the creative act and placing it at times in the unconscious process, or within the primary abilities, in the potential within the individual or within the cognitive strategies. With reference to this here some of the most significant contributions to psychological theories of creativity are briefly presented in order to underline the importance of the creative act itself in the realization of the individual.

A critical pluralism has been chosen for this presentation because complex phenomena such as the creative process cannot be reduced to a single taxonomy or terminology in light of the difficulties in evaluating the weight of its diverse factors; genetic, temperamental, dynamic, environmental, cultural and behavioural; which mix continually with each other and are highly interdependent. According to Freud creativity needs to be seen as the result of an intense and incontrollable impulse owing to primary processes; deriving origin from deep within the unconscious mind of the artist and, overcoming the barriers of censorship, of removal and of resistance, creativity is first and foremost an expression of sexual character. These defence mechanisms are responsible for the transformation and the channelling of the artist's personal unconscious fantasies into the universal area of art. Freud identifies a connection between the creative process and childhood play because both allow the artist to utilize sublimation in order to transform conflicting impulses into constructive processes (Freud, 1907). Klein, in agreement with Freud, considers creativity as an expression of unconscious psychic life (Klein 1932).

Ego psychology theories adopt a perspective according to which creative manifestations are expressions aimed at the neutralization of aggression in order to allow the emergence, or the rehabilitation, of the ego's adaptive functions. (Hartmann 1950, Kris 1952, Rapaport, 1958). Adler (1920) theorises action as governed by aims, both conscious and unconscious, which are able to construct the story of the individual's psychic development.

For Adler (1912) the core of creativity is the attempt to compensate for an inner sense of inferiority. Jungian analytical psychology sees creative impulses as originating from the unconscious in a perspective that goes beyond individuality and that instead belongs historically to the collective. A collective unconscious populated by archetypes, symbolism and material linked to dreams and mythology. This psychology acknowledges a greater freedom in the artistic work compared with the experiences of its creator, connecting such work to the dynamic of a collective unconscious from which it is originated directly. Introversion and intuition, which characterise the personalities of many artists, bring them to collect the most intimate nuances of their meetings with the reality around them. The creative individual is able to marvel at his actions, to experience himself as architect of these acts but, at the same time, to lose himself and go beyond his personal limits (Fromm 1972).

Lichtenberg presents the operations of five motivational systems that moderate the self's way of functioning during infancy and also in adult life; all of these motivational systems-those of psychic regulation of physiological needs, those of attachment-affiliation, explorative-assertive, adversarial and sexual-sensual are present at birth and able to regulate the meeting of the individual's needs and both his conscious and unconscious his preferences and tendencies (Lichtenberg 1989; Gennaro & Bucolo 2006). In particular, play and creativity involve the motivational explorative-assertive system.

The Gestalt school sees creativity and creative act as acts of intelligent reorganisation of phenomena that consist of the possibility of change in structure resulting from an intelligent modification of the previous one, giving origin to new objects and new relationships.

In the nineteen-fifties the need for measurement was seen as being essential for a realistic measurement and evaluation of the creative personality. The employment of techniques of factor analysis is based upon the premise that stable characteristics exist, identifiable aptitudes or dimensions within the individual, in the comparison of creative personalities. These factors are: fluency, seen as the capacity to create as many ideas as possible originating from a single stimulus; flexibility, seen as a measurement of the capacity to change setting or move from one scheme to another in the course of conception arriving at effective results in unusual and original ways; ability for synthesis, seen as an operation unifying various parts or elements in a globality inclined towards alternative solutions; re-

organisational ability, seen as a capacity to organize ideas according to wider and more comprehensive schemes (Gennaro, Bucolo, 2006).

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19th-century Anthropology and Psychology in Italy: The studies of Paolo Mantegazza

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In the scientific literature the early Psychological studies are generally traced within the Psychiatric area. Nevertheless analysing the scientific context in the end of 19th century it is possible to note that the Psychological analysis was promoted within the Physical Anthropological research too. The Anthropological tradition in Italy developed through some promoters, among all stands out Paolo Mantegazza (1831-1910) Professor of Anthropology at the University of Florence. In that period the scientists intended through Anthropology to make an objective study of men based on measurement and comparison among different people to understand the natural history of men and of different populations. The Anthropological research, promoted also by Paolo Mantegazza, included the analysis of Psychological functions of individual (Landucci, 1977). Among the main movements of Italian Anthropology there was the Criminal Anthropology made by Cesare Lombroso. This approach paid attention at the morphological characteristics of individuals which was associated with psychological characteristics (Gibson, 2002). Another index of Italian Anthropological studies was led by Giuseppe Sergi. This author proposed a Psychophysiological study of individual and his interest was directed to analyse the biological characteristics which was considered the substratum of psychological function (Mucciarelli, 1987). Also Paolo Mantegazza based his research on Psychophysiological principles but unlike Sergi he centred on measurement and observation of psychological functions.

Mantegazza to make a Psychophysiological study worked together with the physiologists Alexandre Herzen and Moritz Schiff which was in the same institute in Florence. The researches made by this scientists in the Physiological Laboratory represented the early psychological studies based on the measurement and experimental method. Mantegazza regarded so much important the Psychological and Physiological researches that he proposed these topics in his course of Anthropology. Mantegazza moreover decided to establish the Psychological Museum to promote the studies of dawning Psychology. The author proposed the new Psychological researches made by him and by the researchers of the period also through the journal *Archivio per l'Antropologia e l'Etnologia*.. Mantegazza wrote also some psychological and physiological books which was translated in some languages. The translations represented the will of collaboration among the international scientists wanted by Mantegazza to increase the knowledge of every part of the human being.

The present study intends to consider how the 19th-century Anthropological research allowed the development of Psychological study. This concept is represented in Italy by the model of Paolo Mantegazza.

Affecting ontologies: Cultural Studies a decade after the Sokal hoax

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During the past two decades, Anglophone humanities, and to some extent the social sciences, have been conducted largely within the epistemological and ontological frameworks of Francophone philosophy. The 'post-structuralist' vocabulary assumed is frequently vague and abstract, and claims to knowledge are criticised from various relativistic, anti-realist positions about which most undergraduates are likely to be ignorant and must therefore take on trust. This interdisciplinary field is usually labelled 'Cultural Studies'.

Despite the vigorous debates that were provoked by the publication of the Sokal hoax in 1996, inter-disciplinary writing in Cultural Studies has become increasingly idealist and antagonistic to empirical research. Ironically, I argue, writing inspired by Gilles Deleuze and by a kind of abstract phenomenology, for example, has become almost orthodox in some fields of film studies, audience psychology, and literary studies.

Just as realist epistemologies are increasingly derided in Anglophone interdisciplinary humanities so academic Psychology is increasingly dismissed as scientistic or positivist. Yet, when post-structuralists find they must write about the material world or psychological reality, their anti-realist concepts frequently lead them into incoherence or to invent or invoke novel hypostatisations that amount to little more than unnecessary metaphorical speculation.

I illustrate this contention principally by reference to Brian Massumi's psycho-philosophical book *Parables for the Virtual* (2002), to recent cultural/media studies writing about 'life and technics', and to the curious ontologies of 'affect' employed in diverse disciplinary contexts in the recent literature.

In my main example, the 'hyper-realist' concept of 'the virtual' is advanced , although it appears to be only a verbal solution to Deleuzians' epistemological assumptions. Indeed it confounds the anti-realists' dilemma because it is claimed according to Deleuze's follower Colebrook that: *There is not a domain of the real that is ... given to the subject. There is simply givenness, and the giving of the given cannot be located in the subject (sic). .... It is not as though there is a real world (realism) that is repeated virtually in the subject (as ideal). The actual is a constant becoming virtual* (ibid).

Other recent examples from Cultural Studies that I criticise include: a new kind of vitalism based on neo-phenomenology, and the vacuous invocation of 'affect' to 'explain' diverse psychological and biological phenomena. Many of these are claimed to be 'infra-empirical', relational, or 'virtual'.

I cannot present a coherent narrative account of the proliferation of such idealisms, nor of the ironic claim that these are proposed with realist intent. Instead, I can only call attention to the intellectual contortions which, for many Cultural Studies writers, are meant to escape the epistemological implications of the Sokal hoax and of Sokal and Bricmont's book-length expose. I see this mode of writing as a kind of neologistic verbal 'performance' that only purports to illuminate a novel ontology. So I find it difficult to avoid a pessimistic prognosis for the humanities in which Cultural Studies' epistemological malaise remains untreated a decade after Sokal's diagnosis.

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Models of Psychological Practice in China: An Analysis of the Content of Chinese Psychology Journals (1922-1945)

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The present study aims to investigate some features of the practice of experimental psychology in China during the Republican period using a content analysis of its journals. It follows a line of research begun by Danziger[1], who sought to uncover the origins and assumptions of experimental investigative practices and concluded, on the basis of his analysis of American psychology journals, that the discipline is not so much a science committed to the unfolding of truths about the real, as an enterprise which continually (re)constructs its objects of enquiry as well as (re)setting its terms of reference and criteria for what count as significant findings. Danziger's project was based upon studies of investigators who were themselves the products of an 'individualistic civilisation' (p.186), who reproduced a myth of individual behaviour being not due to social settings but to internal attributes of individuals. Our study aims to answer the question of whether a very different civilisation—one based not at all on individuals in isolation but rather on deeply rooted social collectivities—has implicitly responded to recently uncovered social practices in the same way as their western predecessors, upon whom they relied for psychological knowledge in the first place. An answer to this question has implications for our present understanding of the discipline of psychology in China and the future direction it is likely to take.

In our preliminary investigations we concentrated upon China's first psychology journal, Xinli [Psyche]. Only nine other psychology journals appeared in China during the Republican period (1911-1949) and these began in the late 1920s and early 1930s. Xinli was the official publication of the Chinese Psychological Society. It first appeared in 1922 under the editorship of Zhang Yaoxiang[2] and was published by the Shanghai Zhong Hua Bookstore. It ran for 14 issues and comprised a total of 150 articles. It ceased publication in 1927.[3] Details of these studies have been previously reported in terms of their fields of enquiry[4] but not their research activities seen as social practices. Some of these are summarised in the table.

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[1] Danziger. K. (1990). *Constructing the subject: The historical origins of psychological research.* Cambridge: Cambridge University Press.

[2] The editing was done in the China Psychological Society main office in the psychology laboratory of Beijing Higher Normal University.

[3] See Jiao Liru (1992) *The State of Psychology - The origin and development of China modern psychology*

[4] The terms most frequently used are *bei shi* or *bei shi zhe* (i.e. "subject"). Sometimes it appears as *shou shi zhe* (i.e. "examinee") connoting the same relationship.

Table: Type of investigative practice across year of publication of China's first psychology journal

Type of study	1922	1923	1924	1925	1926	1927
(a) Individual data	2	9	5	2	2	0
(b)Group data	12	8	6	1	1	2
(c)Basic research	31	24	17	5	8	5
(d)Applied research	10	12	10	7	2	3
(e)Indigenous studies	27	25	16	7	8	7
(f)Translations	7	4	3	2	0	0
Total†	48	40	34	16	10	10

† total = (a) + (b) + (e) + (f)
or (c) + (d) + (f)

Although the figures are skewed towards the earlier years when four issues a year were possible (see note 2) three things are readily apparent: (i) the practice of group studies was already dominant even in this early period (ii) a number of papers were simply translations of papers practiced in the West showing a reliance of direct importation of foreign knowledge and yet (iii) over half the studies were "indigenous" this being understood as an examination of phenomena internal to the culture as well as of the thoughts and belief systems which motivate social life of Chinese.

Closer inspection of the content of articles which appeared in this journal (covering fields as diverse as history of psychology, experimental, abnormal, educational social, adolescent and animal psychology, and testing), reveals that the changes which affected two of the experimental investigative practices in the West -- a subtle shift in the role of participants from mutual investigators to passive "subjects", and a move from individual attributions to aggregated data -- were in effect from the beginning, i.e. there was no paradigm shift of the Wundtian to the Galtonian kind as had happened in West.

Another distinguishing feature of the journal was its insistence on not only being a platform for the study of foreign psychological ideas but on allowing older traditional ideas in China to flourish so that, based on the two kinds, new theories and experiments could be devised. This set of aims ensured that not only would the journal entertain a number of translations of foreign material but that it would not restrict itself to forms of investigative practice which would prevent such work from appearing. Unlike the West sensitivity to indigenous forms of psychology seeing it as cultural as much as scientific product was there from the beginning.

The project has also involved looking at the remaining nine psychology journals each of which increasingly took a narrower more specialised view of the discipline developed over what was to become a very turbulent period in the development of the country during which the life of each journal was brought to a premature end.

The Study of the "Psychological Present" in Italy during the First Decades of the Twentieth Century

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The study of the "subjective experience of time" constitutes a classical research program of experimental psychology developed in many European laboratories during the first half of the twentieth century. Investigations of this kind were conducted also in Italy, where three psychologists in particular, Vittorio Benussi, Enzo Bonaventura, and Renata Calabresi, in the laboratories of Graz and of Florence, concentrated their attention on the study of the "psychological present" or "perceived presentness," making an original contribution that was recognized and discussed also on an international level. With the proposed Poster we would like to illustrate the results that they obtained both on the theoretical and methodological plane, and on that of the instruments and experimental techniques adopted.

From the theoretical point of view, the Italian psychologists pointed out, in different years and different ways, that the experience of the "passing of time" ("internal or subjective time") is a derived experience with respect to a still more primitive one, the experience of the "phenomenic change": our consciousness is continuously presented with diverse phenomena, and this "diversity" of which human beings have immediate awareness is indissolubly accompanied by the experience of a "temporal order" among the phenomena, which are precisely ordered into present, past, and future events by the functions of perception, of memory and of representation. The temporal experience therefore implies, among other experiences, an "experience of the present," which the Italian psychologists defined as a perception of events related by a temporal connection of belonging to a more complex event, as a unitary perceptive act that achieves the integration of particular sensory data acquired in a brief interval of time.

These considerations of a theoretical nature were followed up especially by Bonaventura and Calabresi with a series of experiments—which will be illustrated in the Poster—conducted with the tachistoscope (which they modified to be a "double tachistoscope"), with which they proposed to "measure" the "duration" of the psychological present. These experiments consisted in the successive presentation of a series of visual stimuli with very brief temporal intervals between one and the other (for example, a sequence of letters) and in the acquisition, the "registration," of the temporal experience perceived by the subject, who referred with an introspective act. In this way, the two Italian psychologists perfected a methodology that contemplated the systematic use of introspection, inserted within a rigorously controlled experimental setting.

With these experiments, they obtained the result of identifying in 700 milliseconds the minimum extension of the "experience or perception of the present," i.e., the minimum duration of an act of unifying synthesis—carried out by consciousness—of distinct stimuli presented in succession. Above this amount of time the "perception of the present" could cease, and there could appear the experience of "temporal succession" according to a "before" and an "after."

The theory and the experiments of Benussi, Bonaventura, and Calabresi represented, therefore, an important step in the research on the so-called "microstructure of time."

Challenges and new areas to explore in interethnic relationship and inclusion in Romania during the EU accession

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As a result of the communist collapse in 1989, which brought about a lot of challenges in terms of democratization, the 90's represented a controversial period as far as the interethnic relations were concerned. The interethnic issues became a common topic in mass-media debates and it soon became politicized topics as well. Were these debates presenting a real situation or was it just the product of the imagination and frustration?

View that there were no scientific researches sustaining these discussions, it soon became clear that there was a need for civil debates and sociological analysis of this phenomena. A whole process of monitoring and evaluation of the interethnic relations was initialized by the successive governments of Romania as intercultural cross democracy programs.

The intercultural democracy's approaches are an interdisciplinary program which aims at awareness-building students and social actors with the intercultural learning's issues conceived as an introduction of the theoretical and practical framework and action at national and international levels. Intercultural learning is related to global citizenship, by which we take into account the cultural diversity within our society and Europe for developing cohesion and solidarity in diversity. Intercultural learning is conceived as an ethical approach in the holistic perspective, as a consensus, a political volition and an institutional configuration based on the philosophy of State and its relations with civic society and on the philosophy of European integration. This causal relation aims at learning and understanding different cultural codes, networking the target-groups in intercultural learning, sharing attitudes of loyalty and respect towards each other into a larger context of responsive participation in intercultural democracy sites.

Involvement's trends of programs are: know and participate, cohesion in diversity and development, intercultural democracy sites (e.g. departments of Timis, of Dobrogea, of Sibiu and so on), promoting and participating in intercultural democracy' policy of new Europe

Intercultural learning develops from the main culture, with which the other contributing cultures communicate. The intersections and changes among the participant cultures develop, in equal measure, in the mutual richness of the exchange with the main culture. We can talk about multiple and interconnected perspectives: humanistic, cultural, strategic and symbiotic-synergetic (focused on the interrelation between subject and background).

A recent study, regarding the interethnic climate in Romania in 2006 and the Ethno-barometer figure some interesting results on self and each other perception concerning the minorities of Romania, especially, two of them, Magyars and Rroma people. The self-proximity perception is frequently more or less similar for each group: Romanian consider themselves as hospitable, kind, industrious; Magyars as industrious, hospitable, kind; Rroma as hospitable, industrious, kind, etc.

As far as the perception of the others is concerned, the image that the minorities have about Romanians corresponds more or less with the Romanians' self-perception, while the attributes are completely different concerning the Magyars or Rroma people.

The concerned hierarchy suggests that the attributes indicated make a set of common values, a possible probable image of a local community.

An interpretation of the results of such a sociological research and social inclusion approaches are identifying any negative factors related, especially, to the social effects, and to the over-political pressing to the local communities.

As a recent member of the EU, Romania has still to do on the cultural diversity, The problem is finding a way of implementing strategies that could both protect the minorities' rights and avoid discontent within the individuals representing the majority or the members of excluded minorities. The side-effects of these kinds of positive privileges to one community can generate situations that have to be taken into account: ethnical parallelism, territoriality, a blockage in social and local dialogue, diminishing local development by political staff etc.

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Engineering a free mind: A century from the first Montessori's "Children's House" in Rome (1907-2007)

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Maria Montessori, on the basis of her medical, psychological and anthropological education, elaborated his "method," founding it on the "positive sciences," for a progressive education that aimed at emancipating the child's mind starting from the education of the sensorial experience. Her "method" is still debated in the most famous scientific journals as proved by a recent article published in September on Science that deals with its psychological and pedagogical effectiveness (Lillard & Else-Quest, 2006).

Her method was actually elaborated and experimented for the first time between 1907 and 1908 in the "Houses of Childhood" of the popular Roman district of San Lorenzo. On January 7th 1907, the first house of childhood was opened in 58 Marsi Street and Maria Montessori was invited therein to work as coordinator of the educational activities. She worked, at first, with "normal" children. The first "Houses of Childhood," a definition coined by the radical and feminist journalist Olga Lodi, were actually wanted and supported by Eduardo Talamo, a civil engineer and chief executive of the Istituto Romano dei Beni Stabili (IRBS) [Roman Institute of the Real Estates], with the double aim of (1) teaching the people

living in the district to take care of themselves and of their houses and, as a consequence, (2) to make the estates of the IRBS more profitable.

IRBS was a society founded by thousands shareholders of the Bank of Italy and strongly wanted by its first director Bonaldo Stringher to administer autonomously the real estates that the bank had first acquired after the bankruptcy of other banks and later entrusted to the IRBS. Founded in Rome in 1904, the IRBS, directed by Talamo, not only dealt with the renovation of entire popular blocks but also with the construction of some wide zones of Rome's main districts. Talamo promoted an intense campaign of modernization of the buildings in order to make them more luminous and hygienic but also to fragment them and to increase their number with the aim of eliminating the illegal rents by increasing the number of rentable flats. Talamo's idea was to offer some utilities to the inhabitants by rationalizing the buildings and, for this reason, he decided to endow the blocks with some recreational centres for pre-school-age children (3-7 years old), that later were called "Houses of Childhood." Between 1905 and 1908 the net profit of the IRBS increased by 375%. The "experiment" of the Houses of Childhood in the popular district of San Lorenzo became the place where Maria Montessori elaborated and experimented her new method on the basis of the anthropological knowledges she had acquired and described in the 1909 monograph "The method of scientific pedagogy as applied to child education in the Houses of Chidlhood" [Il Metodo della pedagogia scientifica applicato all'educazione infantile nella casa dei bambini]. Her method was very successful all over the world and represented a watershed in Montessori's scientific career. As from the 1910s she applied herself to the elaboration of her educational movement.

The present article, based on a research carried out at the Historical Archive of the Bank of Italy [Archivio Storico della Banca d'Italia] (ASBI), aims at reconstructing the context surrounding the first Houses of Childhood in San Lorenzo and, above all, at emphasizing Eduardo Talamo's economical and engineering ideas and Maria Montessori's scientific culture, whose interaction produced the famous "educational experiment." Actually, the "experiment" also aimed at "moralizing" the popular families in consequence of the children's education and of a different way of conceiving one's own life space.

The Emergence of Deduction as a Psychological Function: Some Reflections on Netz´s Historical Reconstruction

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Reviel Netz recently published (1999) a book about de "The Shaping of Deduction in Greek Mathematics". This work, in the intention of the author, has been wrote, and can be read, at three different and almost independent levels: first, as a description of the practices of Greek mathematics, second, as a theory of the emergence of the deductive method, last, as a case-study for a general view on the history of science. But all the emphasis relies on his detailed analysis of the practices. Four chapters, devoted to the lettered diagrams and his pragmatics, the mathematical lexicon and the role and use of formulae, constitute, no doubt, the core of the book and probably the most appealing contribution of the author to the subject. Two other chapters develop "the shaping of necessity" and "the shaping of generality", the two conditions deduction meets. These two, stated as a consequence of the previous, are the place where the theory of the emergence of deduction as a cognitive achievement better and finally appears.

Netz´s book is primarily a research about the early history of mathematics (geometry, in fact). And research into the history of disciplines does not need to be at the same time research about cognition in a psychological sense. But in the case of Netz, it is this way (the subtitle of the book is "A study in cognitive history" and reflects well this aspect). So, the historical reconstruction proposed by Netz entails, and more, it accurately develops, a psychologically relevant theory about the first appearance of systematic practices of deduction. Here, to stay the interest of this paper as soon as possible, what I try to do is to accept the invitation I understand Netz is doing, and analyse his study (his pieces of evidence, above all) as a psychological theory. The first question here is then, which one is Netz´s psychological theory about the emergence of deduction? The reason for this question is not a particular one for the case at sake, but a much more general one: the enormous difficulties each meeting between history and psychology finds –and the equally enormous interest these meetings have, in my opinion. A thesis I would like to defend is that Netz´s historical contribution is psychologically more relevant than he even thinks, but for reasons very different than those he is managing.

One of the points of departure for Netz is the need to stress the importance of the practices of knowledge against the concentration on propositional knowledge: "Shared beliefs are much less common than shared practices" (p. 2). The development of this idea supports a positioning that takes distance from the "kuhnian history of science", most propositionally oriented. The other point of departure, the one I would like to concentrate on, is defined with respect the Fodor´s thesis about the modularity of mind. Here, the position Netz adopts is, briefly exposed, that while there has been noticeable advance in the study of Fodorean modules, the same can not be said about central processes: "Clearly, it is very difficult to develop a cognitive science of central processes". But, continuing with Netz´s words, "It simply means that, instead of a cognitive science of such aspects of the mind, we should have a cognitive *history*" (p. 6). In this way the responsability for the construction of that such science of the central psychological processes changes from the hands of the cognitive scientist to that of the historian. "I am not a cognitive scientist (and this study is not an `application´ of some cognitive theory)", Netz affirms. Maybe, if one understand that to be a cognitive scientist necessarily implies to be a fodorean scholar. Obviously, it is not true. Perhaps, Netz is simply doing a wrong election of the psychological landmark he chooses to characterise, and contrast, his work. From the point of view of the vigotskyan tradition and the cultural psychology there is nothing more necessary than to try to reconstruct the paths of development of the higher psychological processes. The point of this paper is to try to show that this is exactly what Netz, maybe inadvertently, does with his "The Shaping of Deduction in Greek Mathematics". Most of the key concepts of the vigotskyan and sociohistorical tradition in psychology appears and play an explanatory powerful role in Netz reconstruction. First of all, and in a nuclear way, the importance of the lettered diagram as a semiotic device that makes possible the emergence of deduction, a form of higher psychological processes socially originated and historically datable, as Netz magisterially shows.

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Sante De Sanctis, a pioneer of psychology in Rome at the turn of the 19th century

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Sante De Sanctis (1862-1935), a psychiatrist and psychologist, is generally regarded as one of the "pillars" of Italian scientific psychology. In contemporary psychology literature he is considered one of the main founders of the discipline as well as one of its main makers between the end of the 19th century and the first thirty years of the 20th. His wide and extraordinary rich scientific production (which includes more than 300 works) was associated to an uninterrupted institutional activity. In this sense, De Sanctis played a leading academic role as holder of one of the first three Italian professorships of experimental psychology (1906), as founder of the first laboratory of experimental psychology in Rome (1907), as one of the founders of the Italian Society of Psychology (1910), which he directed several times, and, finally, as commissioner of the main contests of experimental psychology.

Moreover, De Sanctis is regarded as one of the most important Italian psychologists of the second generation who, with Binet, Külpe, Münsterberg, Stern, Claparède, Ebbinghaus, enlarged Wundt's classic experimentalism (Ash, 1988), by coming out of the laboratories parochial logics in order to point to the inquiry of the superior mental phenomena and to psychology applied activities (Danziger, 1990). As exponent of the second generation of the European psychologists, he adopted a pluralistic methodological approach that united the laboratories methods to the clinical and differential ones (Cimino & Lombardo, 2004; Lombardo & Cicciola, 2006).

In the fundamental treaty he dedicated to Experimental Psychology in 1929-1930 a modern conception of psychology emerges clearly. It includes unitarily both the generalist side (studies on psychophysical proportionialism, on the mimic of the thought, on dreams, on attention, on emotions, etc.) and the applicative one of psychopathology, work psychology, pedagogical psychology, criminal psychology, judicial psychology, all seen in a general experimentalist perspective.

The present paper aims at singling out how Sante De Sanctis's scientific and institutional role at the Roman medical faculty paved the way to a relevant institutional space where experimental psychology could be cultivated according to an original interdisciplinary attitude. This aspect also emerges from a correspondence of more than 800 letters in which the author established a thick net of relationships with the major Italian and European scholars of the 19th century. In particular, De Sanctis, beyond his epistolary relationship with the great psychologists of the period, such as Freud, James, Binet, Janet, Dumas, Claparède, etc., also established a number of scientific collaborations with some anthropologists (Sergi), pedagogists (Montessori and Credaro), naturalists and psychiatrists (Lombroso, Morselli, Tanzi e Tamburini), men of law (Ottolenghi and Ferri) and psychologists (Benussi, Kiesow, Ferrari, Gemelli and Ponzo).

Modernizing workers, transforming minds. The psychological production of workers as psychological subjects in the South.

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After the end of the Second World War psychological knowledge became one of the ways of "modernizing the mind" of the inhabitants of the, then recently created, "third world". Framed within the discourse of development, the world of work has been a central concerned for modernizing theories and strategies, which strongly have assumed that "transforming the mind" is the vehicle whereby productivity is increased, well being is achieved and, as a consequence, it becomes the appropriate path to national development. This paper examines the relation between psychology and the world of work in terms of the constitution of the "citizen" as a modern employee and its Latin American (Asian or African) counter-face as the "poor", or the other that needs to be modernized as a chapter of the historical construction of global modern subjectivity. These particular constructions are carried out not as an imposition from North-Atlantic societies, but as a collaborative scientific enterprise in which psychologists in the South actively got involved. Far from disappearing these constructions are present in today's rhetoric of both globalisation and social intervention programs.

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Coercion and social sciences : Lessons from the Palestinian case

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The following proposal deals with the issue of social sciences under coercion, and demonstrates how these two instances are not always contradictory : in the Palestinian case, coercion, the Israeli-Palestinian conflict and its internationalization both permits and constraints the development of local social sciences. This paper would then assert on the methodological level that history of human and social sciences is not only a history of ideas or institutions, but also one of individuals.

This proposal departs from my PhD fieldwork on the Palestinian social scientists of the Occupied Territories. The data was collected during a two-year ethnographic immersion in the West Bank and Gaza Strip until 2003, followed by a complementary field research at the end of 2005. It consists in original documents, notes and comprehensive interviews.

One important question I raise deals with the historical and political conditions for the emergence of social sciences. While common sense as well as many works would assume – even implicitly – that a liberal and sovereign state is a primal condition for the genesis and very existence of social sciences, the Palestinian case constitutes a blatant contradiction. Neither freedom nor statehood defines the Palestinian polity for decades ; rather internal ethnolocalism, external coercion and land colonization. Todays situation is much more that of spatial and political fragmentation than political sovereignty. The long lasting Israeli

occupation, its land control orientation, have nevertheless let room for an indigenous national space and narrative, notably through a nearly autonomous academic life.

Palestinian social sciences were born in the intersection of the 1948 diaspora, educated abroad, and a new generation of social scientists located in the Occupied Territories. That is to say the primal nationalist drive of the Palestinian social scientists. While the first generation of actors and productions would raise the question of "what is Palestinity", the last generation of social scientists is broadly concerned with the question of "how to cure Palestine". On the institutional level, this therapeutic turn occurring at the end of the 80's goes along with the growing externalization and NGOization of the social sciences, with the development of entrepreneurship and expertise market outside of the universities. Though redefined, the tension between an outside Global and an enclaved Local still remains and characterizes the shape and state of the Palestinian social sciences.

This specific chronology is quite rare in the Arab world, and could be opposed to the North-African colonial experiences were the French administration set its own research centers and colleges for the European colons, and secondly for a selected indigenous elite chosen for second rank administrative tasks. Social sciences there are born colonial, and the stake for the newly independent states would be later to nationalize those sciences. At least and in some respects, the Palestinian case could be compared to the Egyptian experience in its chronology. What mainly differentiates these two cases is the scale and date. In the Palestinian case, social scientists and their institutions arrived too late at maturity, and in too little quantity, to expect any effect on social transformation. What remains today is very specific : dozens of social scientists without institution nor autonomous field, in a strange context : an actual situation of - albeit specific - colonialism, in a post-colonial time and trend.

After these developments on the collective and macro-historic levels, the way is paved for paying more attention to individual biographies of Palestinian social scientists. In so doing, I apply one of the major findings of the new social studies of sciences, born after Thomas Kuhn's works : their programs consider the actors of science as important as their works and institutions. Indeed, this emphasis to individual socio-analysis and biographic levels of science history was seldom applied. In the Palestinian case, I would demonstrate how the professional involvement of social scientists can be distinguished into three models : therapeutic, political, and scientific. Since these models coexist within the same persons in various sets, I would nevertheless show why the therapeutic model seems to be dominant : the humiliated trajectory of both the Palestinian community and its every member leads to consider social sciences as both a therapy for the collectivity and the social scientist, who regains some sort of self-confidence for him and his country.

Psychological type processes constitute an epistemic framework for psychological systems

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Various conceptual schemes can be profitably applied to understand the similarities and differences in the psychological systems-- structuralism, functionalism, psychoanalysis, behaviorism, Gestalt, and humanistic psychology (Coan, 1968, 1979; Watson, 1967, 1977). For example, Coan's conceptual scheme applies bipolar meaning dimensions (e.g., Subjectivism-Objectivism, Holism-Elementarism, Personal-Apersonal Orientation, Qualitative-Quantitative, etc.) to a conceptual analysis of the psychological systems. This

paper proposes that psychological type theory (Jung, 1921/1971) provides a coherent, parsimonious meaning framework to examine both the conceptual schemes applied to psychological systems--and the conceptual variations in the major psychological systems. The eight psychological type processes (Extraversion-Introversion, Sensing-Intuition, Thinking-Feeling, Judging-Perceiving) reflect primary qualities of consciousness that constitute a foundational epistemic framework that underlies cognitive constructions such as the psychological systems. This analysis proposes that primary qualities of Extraversion are reflected in the Objectivism and Exogenism that characterize functionalism and behaviorism; while the primary qualities of Introversion are reflected in the Subjectivism and Endogenism that characterize structuralism, psychoanalysis, Gestalt and humanistic psychology (See Table 1). Further, primary qualities of Sensing perception are fundamental features of the Elementalism and Quantitative Orientation that characterize structuralism, psychoanalysis, and behaviorism. Conversely, primary qualities of Intuitive perception are basic characteristics of the Holism and a Qualitative Orientation that characterize functionalism, Gestalt, and humanistic psychology. Basic features of Thinking judgment are found in the Objectivism, Apersonal Orientation, and Quantitative Orientation that characterize structuralism, functionalism, psychoanalysis, and behaviorism. Conversely, basic features of Feeling judgment dominate the Subjectivism, Personal Orientation, and Qualitative orientation that characterize Gestalt and humanistic psychology. The results of the Judging-Perceiving orientation is reflected, respectively, in whether the system is characterized by a Static, mechanistic metaphor, as in structuralism, psychoanalysis, and behaviorism; or whether the system is animated by a Dynamic, organismic metaphor as in functionalism, Gestalt and humanistic psychology. Each psychological system is evaluated in detail and a function type and psychological type was tentatively assigned as follows: Structuralism-ISTJ, Functionalism-ENTP, Psychoanalysis-ISTJ, Behaviorism-ESTJ, Gestalt-INTP, and Humanistic psychology-INFP (See Table 2).

Table 1

Relationships between psychological type processes and Coan's conceptual dimensions

Coan's Factor I	Subjectivism Introversion Feeling	Objectivism Extraversion Thinking
Coan's Factor II	Holism Intuition	Elementarism Sensing
Coan's Factor III	Personal Orientation Feeling	Apersonal Orientation Thinking
Coan's Factor IV	Qualitative Orientation Feeling Intuition	Quantitative Orientation Thinking Sensing
Coan's Factor V	Dynamic Perceiving Intuition	Static Judging Sensing
Coan's Factor VI	Endogenist Introversion	Exogenist Extraversion

Table 2
Typological classification of psychological systems and theories

ISTJ Structuralism Psychoanalysis Cognitivism; Trait Psychology (Cattell, Eysenck)	ISFJ	INFJ	INTJ
ISTP	ISFP Gestalt therapy (Perls)	INFP Humanistic Psychology Analytical Psychology (Jung)	INTP Gestalt Psychology Personal Construct Theory (G. Kelly)

ESTP	ESFP Feminine Psychology (Horney)	ENFP Individual Psychology (Adler)	ENTP Functionalism Ego Psychology (Erikson, Fromm); Sullivanian Psychology
ESTJ Behaviorism	ESFJ Transactional Analysis (Berne)	ENFJ	ENTJ Social Learning (Bandura, Rotter); Interpersonal Diagnosis (Leary)

Note: In Table 2, titles of psychological systems are bolded; titles of theories are in standard type.

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6.00-8.00 RECEPTION

TUESDAY 26TH JUNE

Main Auditorium

Historiography of the Human Sciences	
9.00-9.30	Kenneth Feigenbaum, A History of Psychology Culled from Professional Journals Written in English (1982- 2006)
9.30-10.00	Matthew Dunn & Jim Capshew, The Present Past: Publication Patterns in History of Psychology, 1998-2006
10.00-10.30	Sally Swartz, Confusions of Mind and Excitable Speech: Dilemmas in the Interpretation of Colonial Lunatic Asylum Case Records
10.30-11.00	Alexandra Rutherford, Toward a History of North American Feminist Psychology: a Prolegomenon

Seminar Room 1

Symposium: Freud Workshop	
9.00-9.20	Ruud Abma, The Reception and Historiography of Psychoanalysis in the Netherlands
9.20-9.40	Jaap Bos, A New Freud for Historians
9.40-10.00	David Lee (Convenor), Switzerland in the History and Historiography of Depth Psychology
10.00-10.20	Gavin Miller, Psychoanalysis in Two "Minor" Nations: Scotland and New Zealand
10.20-11.00	John Burnham (Discussant)

Seminar Room 2

18th & 19th Century Human Sciences	
9.00-9.30	Anne Rose, Puberty and the Passions: Ethnographies of Adolescence in French Anthropological Medicine
9.30-10.00	Edward Rafferty, Early Anthropology and the Origins of American Conservation: WJ McGee and the Washington Intellectual Community, 1870-1912
10.00-10.30	Woodruff Smith, Corruption and the Mapping of Social Science in the Late 18th c
10.30-11.00	Bettina Wahrig, Determining Poisons: Toxicology and Experiment in 19th c Germany

Main Auditorium: Historiography of the Human Sciences

A History of Psychology Culled from Professional Journals Written in English (1982- 2006)

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In 1987 the British Journal for the History of Science published an article by Richards (Richards, 1987) with the title "Of What is history of Psychology a history"? In the article he conceptualized the 'History of Psychology' as a vehicle for justifying the acceptance

psychology as a science by both the natural and physical sciences and by public opinion in general. He also raised the issue of whether the "history" is a history of the discipline, or a "history of psychology".

In between 1987 and 2007 many answers to his question have been proposed to the question raised by Richards (Brock.A.C.et al 2004) (Danziger,1990,1994) (Harris,1997) (Mayr,1990) (Smith,1998) (Furumoto,1989). In 2006 Lovett wrote an article entitled "The New History of Psychology: A review and Critique" (Lovett, 2006) in which he discussed two ideal types of the history of psychology: "the old history" and "the new history". The "old history is characterized by: 1) An internalist perspective which lacks the context of the political and the social. 2) The reliance on presentism for interpretation. 3) Whiggishness– over time psychology progresses as a science.

The new history is characterized by: 1) A tendency to be critical rather than "ceremonial". 2) An increasing reliance on source rather than second hand data 3) The histories are embedded in the thoughts and general perspective of the period in which the events occur. 4) The new histories are written by those with more formal training in history. This "ideal type" analysis will be subjected to a critique and then the empirical section of my paper will follow

This paper will present a history of psychology as judged by both the titles of the article and a sample of summaries of the articles from 1992 to 2007 in the following sources:

1) The meetings of Cheiron
2) The Journal of the History of Psychology
3) The European Journal for the History of Science
4) The Journal of the History of the Behavioral Sciences

The paper provides an update and an expansion of the data sources employed by Lafuente & Ferrandiz (http://psychology.dur.ac.uk) in their review of the papers presented at Cheiron-Europe from 1982-1991. Their paper mainly concentrated on who wrote what and what countries they came from. The methodology for the present study consists of multiple sorts of the titles and an analysis of the abstracts.which eventually resulted into five categories. A second reader also sorted the cards, which will be available for anyone who wishes to do so at Cheiron/ESHH so that inter-rater reliability can be established.

The categories are named:
1) The lost hero or heroine (Old Psychology)
2) We were bad girls or boys! (Critical psychology and New Psychology)
3) Our crowd: nnova;schools:History of the discipline (Old Psychology)
4) Thieves-he stole from her...Intellectual History of Ideas (mixed)
5) It's the time and place that counts stupid! : Historical Context (New Psychology)

The paper mainly presents a descriptive analysis of the content of the articles reviewed in terms of what was written rather than why, or what variables acted upon the historian in writing the article. Analysis of the data will be related to the concepts of new v.s. old history. Additionally, the issue raised by Roberts as to whether there is a continuing history of psychology being written to prove it's scientific status will be addressed.

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The Present Past: Publication Patterns in *History of Psychology*, 1998-2006

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Founded in 1998, the journal *History of Psychology* is the quarterly organ of the Society for the History of Psychology (Division 26 of the American Psychological Association). The journal publishes a broad spectrum of papers that address historically motivated questions ranging from intellectual history and biography to social, institutional, and cultural history. In this paper we present a content and citation analysis of the entire publishing run of *History of Psychology*. The goal of such an analysis is primarily historiographical: to identify and map trends in the demographic patterns of authors, in the content of the papers, and citation relationships within the journal's ambit over the last nine years. We are also interested in providing a basis for comparing history of psychology with other subdisciplines or specialties in the history of science and to other historical disciplines generally. We hope to address such questions as: Has there been a shift towards non-Western topics? Away from intellectual history and towards approaches that take in more contextual factors? A renewed focus on cognition in psychology?

We will collect demographic data for the authors of each article published in the first nine volumes of *History of Psychology*, noting gender, institutional location, occupation, professional age (i.e., time since highest degree), and number of authors. In order to analyze the content of the articles, each one will be classified according to historical period, geographic area, psychological subfield (clinical, developmental, comparative, etc.),

keywords and descriptors, and historical approach (intellectual history, biography, social history, etc.).

After scoring each article for demographic data and content criteria, raw frequencies for both will be compiled and correlations within and between them will be explored. Trends, anomalies, and other items of interest will be reported using graphical methods.

The PSYCHINFO database will be employed to perform a preliminary citation analysis of the journal within the psychological literature. Unfortunately, *History of Psychology* is not indexed by the Institute of Scientific Information, and thus does not appear in the Web of Science and other citation databases published by ISI, preventing a broader-scale analysis of the citations in the general scientific literature. PSYCHINFO has abstracted the journal since the first issue appeared in 1998, and includes citations for each article within the psychological literature. Examining the citation data for each contribution, we will determine where the articles are being cited and which papers are most often cited, correcting for publication date. These two variables will then be correlated with the demographic variables and content classifications.

Our paper concludes with a discussion of what knowledge can be gained by this analytic procedure, noting also its limitations. First and foremost, it can provide a window on recent publication patterns in the main English-language journal solely devoted to scholarly publication in the history of psychology. We believe that our report will be of general concern to historians of psychology, and might stimulate interest in the question of how scholarly publication refracts knowledge and understanding of the field. Like most historiographical techniques, it contributes to an exploration of the context of goals, assumptions, and methods of scholarly practice.

Confusions of mind and excitable speech: Dilemmas in the interpretation of colonial lunatic asylum case records

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This paper describes an extensive archive of records written by doctors about their lunatic patients, inmates of colonial lunatic asylums in the Cape Colony, between 1890 and 1910. The records document patients' states of mind and physical condition on admission to the asylum, and regular notes trace the progress of their illness, sometimes over decades of incarceration.

Engagement with these historical case records and documents raises a number of issues for historians of psychology attempting to understand the complex relationships between class, race, gender and mental illness. Doctors' writing reveals much about their routine practice and knowledge, but frustratingly little about the patients of whom they wrote. A number of questions remain unanswered. Did patients' racial classification, class and gender affect their diagnosis and treatment? Did the colonial context – which included racialised forms of oppression – play a part in patterns of mental illness? Did culture shape the form taken by the illness?

The aim of this paper is to explore the limits of these patient records as a source of information about the inmates of colonial asylums. It examines the representation of patients in colonial social histories (Jackson, 2005; McCulloch, 1995; Vaughan, 1991), and contrasts this with both postcolonial and clinical representations of the mental life of the colonial insane (Swartz, 1999, 2005). It suggests a method of reading patient case notes aligned with

the historiography of subaltern historians (Spivak, 1993), that attempts to avoid colonization of patient voices.

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Towards a history of North American feminist psychology: A prolegomenon

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In 1974, Maxine Bernstein and Nancy Russo reminded their American colleagues of psychologists' general lack of awareness of women's contributions to the field (Bernstein & Russo, 1974). Their remarks, in synergy with the pioneering work of women's historian Gerda Lerner (Lerner, 1979), the cultural momentum of the second wave of the women's movement, and the readiness of a number of historian-psychologists to address this deficiency, presaged 30 years of scholarship that has resulted in an invaluable body of literature on the history of women in psychology (e.g., Bohan, 1995; Furumoto, 1992; Klein, 2002; Milar, 1999; O'Connell, 2001; O'Connell & Russo, 1983, 1988, 1990; Scarborough & Furumoto, 1987; Shields, 1975; Stevens & Gardner, 1982°, 1982b; Wentworth, 1999). This literature provides both biographical documentation and rich contextual analyses of women psychologists' involvement in a historically androcentric discipline. Scarborough and Furumoto's 1987 volume on first-generation American women,"Untold Lives," is a classic work in this genre. Comparable historical research and analysis continues on second-generation women (Cameron & Hagen, 2005; Johnson, 2005; Johnston, 2005), and historians are beginning to document the feminist psychologists who nnovatore psychology's institutions, theories, and practices around the second wave of the women's movement in the 1970s. So what is the history of North American feminist psychology if not the stories of these women, their activities, contributions, accomplishments, and nnovatore to mainstream psychology?

In this paper I argue that nnovat this rich and growing body of scholarship, a synthetic history of North American feminist psychology that weaves together biographical, social, political, cultural, intellectual, and institutional factors to illuminate the full depth and breadth of the feminist project remains to be written. I outline some of the historiographic nnovatore in defining what a history of feminist psychology would look like. For example, as Unger (2001) has pointed out, the history of *women* in psychology can be viewed from a number of different angles, nnovator the history of women as the agents of psychological

research, the history of women as the subjects of theory and research, and the history of organizations concerned with the psychology of women.

In grappling with the question "Of what is the history of feminist psychology a history?" I propose that a history of feminist psychology should include the stories of those women who have intentionally used psychological research to challenge or debunk sexist assumptions about women (early examples would include Leta Hollingworth's work on the nnovatore hypothesis and functional nnovatore , and Helen Thompson Woolley's work on sex differences); it should include the history of the development of research and theory about women derived from feminist frameworks, that is, a history of what Graham Richards would call "small p" feminist psychology; it should be a history of the formation, struggles, and activism of explicitly feminist organizations in psychology; and it should be a history that embeds all of these internal developments in the cultural contexts of first- and second-wave feminism.

I then discuss the historiographic nnovatore implicit in writing such a history include using presentist definitions of feminism to identify feminist psychologists throughout history (i.e., nnovato what work and which psychologists retrospectively count as feminist); the complexity of meshing internal and external history rather than nnovatore them out for separate analysis; the lack of archival material on which to base a history of feminist psychology; nnovato on the relative emphasis nnovat to personal versus professional expressions of feminism, and others.

Finally, I reveal my own historiographic assumptions in the early stages of my efforts to conceptualize such a history. I argue that the history of the psychology of women or the history of women in psychology is not the same as the history of feminist psychology, and I describe my efforts to uncover material in the archival record and to create material through oral history interviews with which to construct a history of North American feminist psychology.

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Seminar Room 1. Symposium: Freud workshop
Convenor: David Lee

This workshop shall wrestle with the past and future of historical writing on Freud, his followers, psychoanalysis and other depth psychologies. The point of common departure is John Burnham's June 2006 article "The 'New Freud Studies': A Historiographic Shift" (*Journal of the Historical Society*) which argues that we have arrived at the dawn of a new age in the study of Sigmund Freud specifically and depth psychology in general. Four young scholars will reflect on Burnham's vision of this historiography in the twentieth century and his predictions for its future from national/regional perspectives. We shall also discuss how these individual national/regional histories of depth psychologies have impacted our historical treatment of this common past. If Burnham is correct and we stand on the threshold of a new era of comprehension and insight, how then can and should it effect our work? These papers will open discussions on historiography in the history of science as well as on the import of and tension inherent in micro- versus macro-histories. We expect not only to provoke discussion, but to build consensus on future historiographic priorities.

Reception and historiography of psychoanalysis in the Netherlands

Ruud Abma
University of Utrecht

From the stages described by Burnham in The "New Freud Studies", mainly the second episode, the "Freud Wars", has been visible in Dutch historiography. There has not been much original historical work done on Freud and psychoanalysis in the Netherlands, but from the 1980s onward there have been a dozen studies on the history of the psychoanalytic movement in the Netherlands, and more broadly on the reception of Freud's ideas in history,

the human sciences and philosophy, and in mental health theory and practice. Most recent, attention has been drawn to the translation and editing of Freud's works in the Netherlands, which can be seen as a gauge of existing interest in psychoanalysis, but also as a cultural practice that produces interest in Freud's ideas. The earliest translation dates back to 1912, and last year saw the appearance of a complete new Dutch Freud edition. In the paper, I will present an brief overview of the reception of psychoanalysis in the Netherlands, as a context for Dutch historiography on the subject. Iwill also try to explain why Dutch soil is not very fertile for the growing or even the reception of "New Freud studies".

A new Freud for new historians?

Jaap Bos
University of Utrecht

The historiography of psychoanalysis had long been invested with Freudianism itself. Thus Freud's own historic accounts are not merely polemical but also strategic pieces, meant to fence off, protect and outline psychoanalytic property. Indeed, the history of psychoanalysis was used as a weapon in its struggle in the so called 'Freud wars' (Burnham, 2006) which served both to defend and attack psychoanalysis. As we have gained temporal and authoritative distance from Freud it has now become possible to write a historiography for and against psychoanalysis, to use Stephen Frosh's (1997) book title.

I will discuss two problems with regard to the 'new Freud studies' thesis. The first has to do with the question of whether history needs to rewritten in the face of changing interests and perspectives, and the second with the question: if so, how? Put in its simplest form, the issue at stake is what happens when historiography is no longer in service to a particular discipline, but becomes a discipline in itself? Do we need a new Freud or does Freud need new historians?

Using a 'dialogical perspective', this contribution argues that that 'new' psychoanalytic historiography implies not a rewriting of Freudian history, but rather a focus on the process of writing and reading of history itself. The paradoxical effect of a redefinition of psychoanalytic historiography is that on one hand psychoanalytic history distances itself from psychoanalytic practice, while on the other it gives it new impetus, or at least new ways of understanding.

Switzerland in the history and historiography of depth psychology

David Lee
Independent Scholar

Switzerland's ethnic makeup and history made it a most unlikely place to have given birth and played host to many of the twentieth-century's most powerful psychological ideas. How and why so many enormously influential figures in the history of the mental sciences were Swiss has long fascinated historians within and outside Confederation borders. I shall trace not only the fractured nature of scholarly writing on this history, but also the (sometimes tortured) purposes to which it has been put in order to demonstrate how, in contrast to most other national histories, the Swiss situation remains particularly fractured as various narratives have resisted centralizing tendencies. This may then metaphorically reflect a

national character after all. Establishing priorities for how to best write this history in the future will not be easy, but here more than elsewhere I argue it is necessary.

Psychoanalysis in two 'minor' nations: Scotland and New Zealand

Gavin Miller
University of Glasgow

Scotland and New Zealand integrated and transformed psychoanalytic ideas according to their own national traditions and needs. In Scotland, Freudian psychoanalysis was modified by an intellectual life that saw communion as the essence of a rationally developed Christian heritage. What emerged was a distinctively Scottish psychoanalytic theory that emphasized the importance of communion between mother and child, and argued that such personal relations did not 'lean upon' drive satisfaction. After World War Two, Scottish psychoanalytic ideas were transplanted to another small country: the New Zealand Association of Psychotherapists was founded on a manifesto that promoted communion between mother and child, and that opposed the child-rearing practices of experts such as the New Zealander Truby King. The psychoanalysis of 'communion' also remained effective within Scotland during the same period: its influence can be traced in anti-psychiatry, attachment theory, object relations theory, and developmental psychology. To appreciate the history of psychoanalysis properly, we must therefore begin to integrate and understand these histories from non-hegemonic nations and regions.

Discussant: John Burnham
Ohio State University

Seminar Room 2. 18th and 19th Century Human Sciences

Puberty and the Passions: Ethnographies of Adolescence in French Anthropological Medicine

Anne Christina Rose
University of Oklahoma

During the first half of the nineteenth century, medical research on puberty flourished in France. Several physicians defined puberty as the developmental moment when the child's delicate economy, newly energized by a vital force, was most vulnerable to effects of the passions. Some physicians described the potentially harmful effects of passions in terms of gender; certain nervous afflictions and persistent maladies were attributed to the onset of menstruation, for example. In 1816, Auguste Lalourcey presented his *Dissertation sur les phénomènes de la puberté chez la femme.* Arguing that the sexual differentiation apparent at puberty resulted from impressions communicated from the nervous system to the genitals, Lalourcey described the physiological transformation in girls as a "a type of revolution in the entire economy," and one that is never experienced without "derangements of health." The vital forces, Lalourcey observed, concentrated around the uterus during puberty, strengthening its "cellular tissue" and increasing sensitivity in the genitals. Puberty was a "shock" to the female economy, exposing a girl to strange and unknown functions.

Lalourcey compared the "shock" of puberty and menarche in terms of ethnicity and geography. He noted that girls in equatorial zones, including much of Asia and Africa, entered puberty at around age nine. In "the states of the Grand Mogol" girls married at age eight, he observed, and while they were nubile at the same age in Arabia and India, where they could become pregnant at nine, in the northern regions, the ordinary age of puberty was eighteen, but girls in those regions remained fecund for longer than their counterparts in areas south of the equator. Independent from the influence of climate were factors such as ignorance and superstition, he contended, which, in the "hot countries" inclined girls to marry at the first hint of pubertal change. Lalourcey also examined the "vulgar prejudice" associated with certain cultural attitudes toward menstruation. The opinion that menstrual blood was malignant was traceable to Arab physicians, he believed, who had transmitted it to Europeans. He described how Jews and Africans sequestered menstruating girls and prohibited them from doing domestic work.

Lalourcey's work is one example of a comparative anthropological approach to the study of human development. In this paper, I show how that approach evolved over the course of the nineteenth century, and I pay special attention to the ostensibly biological category of puberty, the cultural category of adolescence, and the mediating category of pubescence. Besides Lalourcey's work and contemporaneous vitalist research on puberty, my sources include ethnographies by the naturalist Julien-Joseph Virey. The larger purpose of my paper is invested in defining the ways in which anthropological medicine shaped our understanding of generation as a moment in historical time embodied by a cohort of developing individuals with shared sensibilities.

Early Anthropology and the Origins of American Conservation: W J McGee and the Washington Intellectual Community, 1870-1912

Edward C. Rafferty
Boston University, USA

Although the history of conservation and environmentalism in the United States has received frequent comment from historians and environmental activists, there has been surprisingly little examination of the intellectual origins of the conservation ethic. The most important history of American conservation, Samuel P. Hays's Conservation and the Gospel of Efficiency: The Progressive Conservation Crusade, 1890-1920 (1959), ignores the intellectual history of conservation. Hays argued that the rhetoric and language of conservation advocates was less substantial than the institutional and policy making apparatus of the federal government bureaus that they established. Conservation was primarily about efficient application of scientific expertise to natural resource problems. But historians have recently begun to undertake a substantial re-thinking of the conservation ethic in American public life including the impact of conservation policies on local groups (including Native Americans in the west), and the role of conservation in re-shaping American liberalism in the nineteenth and twentieth century.

One area of that has received little attention is the role that early anthropology and ethnography played in the origins of the American conservation ethic. The nation's earliest anthropologists and ethnographers were clustered around a number of important bureaus and private intellectual organizations in the intellectual community of late-nineteenth-century Washington, D.C., such as the Bureau of American Ethnology and the Anthropological Society of Washington among others. The leading thinkers in the nation's capital included John Wesley Powell, an explorer and ethnologist as well as the founding director of the

Bureau of American Ethnology, Lester Frank Ward, the "founding father" of American sociology and a leading contributor to the scientific work of the new organizations, and W J McGee, the youngest of the three men, a self-trained and self-educated geologist and ethnologist.

McGee occupies a central place in the origins of American anthropology but he also occupies a central place in the history of the conservation ethic in the United States.5 By the end of his life, and as a result of his work in ethnology, McGee grew increasingly interested in man's relationship to nature. His essays and reports on water use among Native American tribes, for example, became the start of a late career in American conservation; he always argued that conservation was really "applied anthropology" since the insights he gained from his study of indigenous communities in the American Southwest became central to his understanding of the nation's use of its natural resources. By the time of the Theodore Roosevelt administration, McGee had become the leading advocate for better policies toward the natural resources of the country. He was, even according to Gifford Pinchot, "the scientific brains of the movement" for conservation.6 It was McGee, for example, who coined the phrase that became the defining characteristic of twentieth-century conservation policy: preserving the nation's natural resources for the greatest good of the greatest number for the longest time.

This paper examines the role that anthropology and ethnography played in McGee's understanding of the conservation ethic and the ways the nation might preserve and protect its natural resources. This important role deserves attention from historians because it sheds light on the origins of conservation and anthropology in the United States, and it sheds light on the role that indigenous communities played in the governing of natural resources. Both McGee and the Washington intellectual community in general contributed to an important dialogue about the dwindling resources of the nation and a dialogue about the people that occupied the land that conservation advocates sought to save.

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[5] McGee always wrote his name without the periods in his initials; it lead to an unfortunate nickname as "No Points McGee" since some of his essays can ramble off topic. See Emma R. McGee, *The Life of W J McGee* (Cedar Rapids, IA: n.p., 1915); Lacey, "The Mysteries of Earth-Making Dissolve," and Curtis Hinsley, *Savages and Scientists: The Smithsonian Institution and the development of American Anthropology, 1846-1910* (Washington, D.C.: Smithsonian Institution Press, 1981), who examines both Powell's and McGee's role in the Bureau of American Ethnology.

[6] Gifford Pinchot, *Breaking New Ground* (1947; new edition, Washington, D.C.: Island Press, 1998), 324-325.

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Corruption and the Mapping of Social Science in the Late Eighteenth Century

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A key aspect of the construction of the early sciences of society was the laying out of conceptual and discursive spaces within which such sciences could operate. This aspect intersected with others: defining epistemologies, identifying acceptable methodologies, and so forth. It is, however, very revealing to focus specifically on the mapping of the notional arenas of human behavior and interaction in which the kinds of rational motivation assumed by the founders of the early social sciences could be represented as working. Among other things, important relationships between central features of the public discourse of the eighteenth century and the formation of theoretical social science can be seen in this way. The most obvious example of such an arena is the "market" of classical political economy, which essentially excludes all forms of behavior and motivation that do not conform to a restricted version of the model of the rational consumer and producer. A great many practices that are essential features of "real" markets are thereby effectively externalized. The process of delineating and externalizing establishes both the conceptual framework for classical economic reasoning and the facts of theoretical, as opposed to operational, knowledge in the field of political economy.

The proposed paper will focus on one important way in which the spaces of early social science were mapped out: by the use of the category of "corruption" as a means of identifying items of operational knowledge (that is, observable practices in politics, commerce, and other forms of social intercourse) that were to be considered out of bounds, that need not – indeed, must not – be taken into account in constructing theoretical knowledge. In some instances, this simply allowed theorists to exclude phenomena that did not correspond to their assumptions about the grounds of human behavior. In the cases which the proposed paper will examine, however, the role of corruption was more complex. Its construction as a category helped to define a crucial relationship between evolving patterns of discourse in the public sphere on the one hand and theory (and sometimes policy positions explicitly based on theory) on the other, a relationship that accommodated significant parallels but also created discursive boundaries and conceptual distance. Corruption

employed as an externalizing category also contributed to dealing with the complicated problem of morality – especially the difficulties that arose when the idea of positive moral laws consonant with universal natural law could no longer be sustained. When the term "corruption" was used to refer to certain patterns of behavior rather than to the more traditional notion of a progressive decline of institutions and moral character from an ideal model, it became possible to claim that the conceptual space from which corrupt behaviors were to be excluded was derived from empirical observation alone, without presuming the existence of moral laws. The resulting theoretical analysis, however, was necessarily suffused with implicit moral judgments – an essential feature if it was to have any chance of success in the public sphere.

The paper will develop its argument with reference to a number of texts, but because of the necessary limits of a conference presentation, it will focus on ones connected to a single case: the discussion in Britain in the last quarter of the eighteenth century of Britain's new imperial involvement in India. The paper starts with Adam Smith's use of corruption as one means of mapping the space of economic analysis in The Wealth of Nations (1776), with particular attention to his analysis of Asian commerce and the operation of the East India Company. It continues with the economic part of Edmund Burke's attack in the 1780s on the Company's rule in India and statements of the economic concepts that underlay the reform policies of the Cornwallis administration in Bengal in the 1780s and 1790s. These documents illustrate the ways in which aspects of public discourse informed economic categories and the manner in which employing the category of corruption led to the exclusion from theoretical knowledge of a very large amount of existing operational knowledge of the South Asian economy. The paper then considers concurrent discussions of political aspects of British rule in India and their relationship to politics in Britain itself, making similar arguments about corruption in the mapping of the space of political analysis. "Corrupt" political practices (which were largely ones that led to the individual accumulation of power for its own sake and for enhancing material gratification) were excluded from political theory and taken as objects of reform, both in Britain and in India. Finally, the paper will describe the somewhat less-distinct mapping of a theoretical space for an ethnology of India under similar circumstances.

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Determining poisons: Toxicology and experiment in 19th century Germany

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1785 was the year in which the term 'toxicology' was first used in a German textbook, published by Joseph Jacob Plenk. [7] This marked a first peak of medical and forensic interest in poisons at the onset of modernity. Plenk inserted the notion of poison into the context of classical natural history. However, he also referred to a large field of cultural connotations which are represented in the frontispiece of the textbook. The engraving pictures a hermaphrodite and centaur teaching each other their secrets, and the Latin motto is: "ingens sub minima mole latet malignitas" (immense malignity is hidden within the smallest amount of substance).

The association between a hidden and almost invisible substance and a moral evil is a constant motive in discourses on poisoners and poisoning from antiquity to 20th century. This association is linked to the notion of the criminal, the secret, and the sublime, which have undergone changes, but which also have a very powerful and long-ranging (*longue-durée*) history. Hence, images of the substance and its potential user have always been closely linked.

In this paper, I will argue that the combined motive of small amount/immense malignity is the expression of a fundamental ambivalence which does not come to an end in the development of modern, experimental science. Moreover, discourses on the criminal mind, i.e. on the poisoner and on his/her substance, have constantly overlapped. In this sense, I would like to analyse in more detail the relationships between 19th century toxicological experimentalism and its cultural representation. I will argue that there is a close link between attempts at stabilizing the different poisonous substances (e.g. by chemical proofs or by botanical identification) and stabilizing their effects (e.g. by experiments on animals). This was due to the above-mentioned ambiguity and to the fact that poisons constituted "boundary objects" (Löwy).[8]

The overlap between the substance and its users can be analysed by examining case studies on criminal poisoning with arsenic and by the discussion on the arsenic eaters in Styria. In the latter case, the question arose whether those members of the rural population who had the habit of eating small amounts of arsenic on a regular basis were criminals, impostors, primitives (comparable, for example, to Indian bethel chewers), or bearers of a personal, secret knowledge which science had yet to explore.

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7 Joseph Jacob Plenk: Toxikologie, oder Lehre von den Giften und Gegengiften, Wien 1785
8 Ilana Löwy: Unscharfe Begriffe und förderative Experimentalstrageien: die immunologische Konstruktion des Selbst. In: Die Experimentalisierung des Lebens, ed. H.R. Rheinberger, M. Hagner, Berlin 1993, pp. 188-206

Around 1850, when this phenomenon first stirred the scientific and the general public, attempts were also made to bring the subjective experience of intoxication onto the scientific stage. While Moreau de Tours wrote on the comparability between mental alienation and the consumption of hashish, his experimental subjects (Baudelaire and others) explored the episteme of "artificial paradises" and praised each other as heroic self-experimentators. The well-known "physiologist of love,"[9] Paolo Mantegazza, praised coca as a "nerve nutrient". His findings, which were partly based on self-experiments, were hailed in the German pharmaceutical press.[10] Gustav Theodor Fechner cited experiences with ether in order to explain his notion of a "threshold of sensation".[11] In the 1870s, Emil Kraepelin worked on a fusion of Wundt's reaction time experiments with the impact of narcotics on mental performance. He even consumed a number of these substances himself and measured his own reaction time. Kraepelin tried to compensate for the instability of the substances and their doses by stabilizing the mental phenomena in his experiments and by adoperating a rigid pattern of experimentation. In this sense, both psycho-physiological and toxicological experiments hinged on the precariousness of toxicological substances. If they were, by definition, highly effective in extremely small doses, then their effects were always in danger of eluding control.

The paper will argue that modern experimental patterns emerging in the field of psychopharmacology and toxicology were driven by a dynamics of precariousness. These patterns have simultaneously propagated and denied the ambivalence of the substances they explore. They may be understood as articulations and de-articulations of the malign and the sublime, as a secret signal of the uncontrollable.

11.00-11.30 COFFEE

Main Auditorium

Historiography of the Human Sciences (Continued)	
11.30-12:00	Gordana Jovanovic, Dilthey and an (Im)Possible History of Psychology
12.00-12.30	Jefferson Pooley, The Mnemonic Entrepreneur: Wilbur Schramm and the Four Founders Myth of Communications Research

Seminar Room 1

Histories of Psychoanalysis	
11.30-12:00	Cecilia Taiana, Reading Lacan in Buenos Aires: The A(r)mour of Words in Lacanian Psychoanalysis During the Last Dictatorship in Argentina, 1976-1983
12.00-12.30	Robert Rieber, From the Pharaohs to Freud: Psychoanalysis and the Egyptian Magical Tradition

9 transl: New York 1936, first publ. 1875

10 Ueber die Einführung des Coca, eines neuen Nerven-Nahrungsmittels, nach Europa. In Allgemeine Zeitschrift für Pharmacie, Pharmakologie und Toxikologie 10 (1861), 103-111

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Seminar Room 2

Measuring Attitudes in Wartime	
11.30-12.00	Uta Gerhardt, A Transatlantic Mission: Change in German Attitudes from Authoritarian to Democratic as Reflected in American Government Survey Reports
12.00-12.30	Johann Louw, A Forgotten Liberal Moment: Attitude Surveying in the South African Army during WWII

Main Auditorium. Historiography of the Human Sciences (con'td)

Dilthey and an (Im)Possible History of Psychology

Gordana Jovanovic
University of Belgrade, Belgrade, Serbia

The general aim of this paper is to try to elaborate one piece of an alternative history of psychology based upon the resurrection of a neglected heritage. This heritage refers to the beginnings of psychology, and its resurrection is supported by new accounts in the social sciences that are usually described as an interpretive turn (e.g., social constructionism, discourse analysis, narrative analysis).

The task will be firstly to examine the neglected pluralistic cultural context in which psychology began its scientific development and secondly to reconstruct ways in which a naturalistic model has repressed alternative hermeneutically-oriented accounts. I will then append to this historical analysis a review of the contemporary interpretive turn in philosophy and the social sciences. This turn could be partly described as a kind of a "return of the repressed" and therefore also as an invitation to a comparative analysis between contemporary hermeneutics and the original modern hermeneutics in nineteenth century.

This is the framework in which Dilthey's conceptualizations of psychology will be discussed. Dilthey himself exemplified the tension between "two cultures" in psychology and moreover the resolution of this tension. Paradoxically enough, Dilthey had a forerunner in this regard - the father of scientific psychology, Wundt himself (I am referring to the personal and historical status of Wundt's "*Voelkerpsychologie*"). On the hand, Dilthey had an extensive and deep knowledge (and even appreciation) of the "explanatory" psychology of the second half of the nineteenth century. This is clearly demonstrated in his numerous lectures on psychology, delivered at different universities from 1867 to 1894 (in Basel, Kiel, Breslau and finally in Berlin) where he dealt, among other things, with the scientific achievements of Herbart, Fechner, Wundt, E. H. Weber, J. Mueller, Helmholtz. On the other hand, he was engaged in a grand, lifelong (and never finished) project of "A Critique of Historical Reason ", which meant an epistemological, logical and methodological founding of the human sciences on a descriptive psychology oriented toward understanding (*Verstehen*).

His seminal work published in 1894. *Ideen ueber eine beschreibende und zergliedernde Psychologie* provoked a full range of reactions from positive to negative. The Dilthey-Ebbinghaus controversy was historically resolved in favour of psychology based upon a natural science model. Instead of being a foundational science for all human sciences, psychology became subsumed under natural science. That outcome has substantially shaped the history of psychology. Dilthey himself is very rarely even mentioned in traditional histories of psychology.

Nevertheless, the starting controversy was not silenced; on the contrary, it remained alive. Moreover, the defeated side – which came to be called hermeneutics - gained support from insigts developed within analytic philosophy of science, though the latter originally served as a legitimation for (natural) scientific knowledge alone. Other important insights came from a linguistic turn already underway, or later from science studies. Generally speaking, we can see a move toward recognition of a necessary social, subjective, hermeneutic basis for science. In our hypothetic rewriting of a first stage in the history of psychology to which also Dilthey belonged, it can be argued that the once Dilthey's program of an epistemological foundation of the sciences of man, society and history is again on the agenda. Moreover, the more radical implications include investigations of the natural sciences too.

The status of hermeneutics itself within Dilthey's project is another topic for analysis which can contribute to the proposed rewriting of the history of psychology. Dilthey's program of a descriptive, analytical psychology was based on a conceptual triangle of lived experience (*Erlebnis*), expression (*Ausdruck*) and understanding (*Verstehen*). Understanding was then locus for hermeneutics in Dilthey's theorizing. However, several controversial interpretations of the status of hermeneutics in Dilthey come into play. There is a tension in the status of hermeneutics in Dilthey. Hermeneutics itself is a battlefield with the naturalistic, positivistic approach on the one hand, and the hermeneutic approach in its proper sense, on the other hand. If we rely upon a Dilthey interpretation given by Hans Georg Gadamer, the most hermeneutic philosopher of the twentienth century, then the fate of hermeneutics in Dilthey's philosophy repeats the resolution brought about in the general controversy about the status of psychology.

Hermeneutics may have finally lost out even in Dilthey's large hermeneutic project of understanding the human historical world. Meantime, it has become universal hermeneutics, or "a theory of ineluctable historicity and linguisticality of our experience."(Grondin)

To conclude, it can be argued that this small vignette in the history of psychology proves that both psychology and history are explicable only within a hermeneutic framework, which is by definition historically changeable.

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The Mnemonic Entrepreneur: Wilbur Schramm and the "Four Founders" Myth of Communication Research

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In its postwar institutional infancy, American mass communication research badly needed a history. This paper traces Wilbur Schramm's self-conscious and successful effort to supply such a history in the form of an origin myth, complete with four putative founders. He was, I contend, a mnemonic entrepreneur who took scraps of memory lying about in the postwar social scientific landscape, and assembled these into a coherent, and self-validating, narrative for the would-be discipline. My argument is that the emerging field of "communication" was, by the early 1960s, flush with the resources that other, more established disciplines covet—research funds, students, and faculty jobs, all in abundance. But the field lacked legitimacy, and this deficit threatened all of its material riches. In a series of nearly identical papers beginning in 1963, Schramm drafted four prominent (and unwitting) social scientists from existing disciplines—Paul Lazarsfeld (sociology), Kurt Lewin (psychology), Carl Hovland (psychology), and Harold Lasswell (political science)—and labeled them the field's "founders." This "four founders" storyline was adopted in most mass communication research textbooks soon after, and it remains today a widely repeated account of the field's past. Schramm had, quite effectively, used disciplinary history to help narrow the gap between communication's institutional gains and its lowly status.

Schramm (b. 1907) began his academic career as an English professor at the University of Iowa, where he had earned his Ph.D. in 1932. His focus was American literature, though he taught and wrote fiction as well—and played an important role in the establishment of the famed Iowa Writers Workshop. Shortly after the U.S. declared war in late 1941, Schramm joined the hundreds of social scientists relocating to Washington to work on propaganda analysis and design. There, Schramm was enmeshed in several of the extraordinary networks of social scientists that had formed around various federal agencies—networks of contacts, friendships, and acquaintances that proved, after the war, to be of importance to many disciplines and to various lines of scholarship. Schramm carpooled, for example, with Gabriel Almond and Margaret Mead, and chaired a monthly dinner and discussion on interdisciplinary social science with Mead, Rensis Likert, Ralph Casey, and Ernest Hilgard among the regular attendees.

Most crucially, he came into repeated contact with the cluster of sociologists, political scientists, and psychologists that had, since the mid-1930s, worked in public opinion research—Lazarsfeld, Likert, Wilson, Lasswell, Hadley Cantril, and Sam Stouffer, among many others. Most of these figures had already collaborated on a variety of initiatives funded by the Rockefeller Foundation in the lead-up to U.S. entry to the war. While in Washington, Schramm came across the 1940 report of the Rockefeller-funded "Communications Seminar" ("Needed Research in Communications"), co-written by Lazarsfeld, Lasswell, and Cantril, among others. In his memoirs (1997), he claims that the Rockefeller report and his conversations with many of its authors gave him the idea for a stand-alone field of "communication." When Congress slashed the OWI budget in 1943, Schramm returned to Iowa City, but not to the English department. Instead, he accepted the directorship of the university's School of Journalism, and months later formally proposed a Ph.D. program in "mass communications."

Schramm would go on to establish similar programs and institutes at the University of Illinois (in 1948) and Stanford (in 1955). By the end of the decade, Schramm, along with former students and a number of allies, had successfully re-cast a many prominent schools of

journalism, especially in the Midwest, as "Schools of Journalism & Mass Communication," complete with Ph.D. programs. He had, that is, managed to establish his social science discipline, by colonizing a series of professional schools that succumbed, in part, for the sake of their own legitimacy in the postwar university.

For all of Schramm's institutional successes through the 1950s, the field of "communication" had little intellectual coherence. The most significant postwar media research, moreover, had been conducted elsewhere, by scholars who did not use the "communication" label. In Richard Whitley's (1974) terms, the field had achieved a measure of social institutionalization before ever establishing cognitive institutionalization—in reverse of the typical pattern. As a remedy, Schramm's 1963 "four founders" account was a self-conscious attempt to supply the field with a shared past and the beginnings of disciplinary self-consciousness.

Schramm's 1963 history renders the anointment in the passive voice ("Four men have usually been considered the 'founding fathers'...")—an act of audacious creativity that comes off as mere reportage. Lewin, Hovland, Lasswell, and Lazarsfeld—all eminent bearers of scholarly capital—were invoked as pre-disciplinary forerunners. Though these figures, all polymaths, worked on "communication"-related problems at least occasionally, none would recognize himself in Schramm's communication pantheon—each had either died or moved on to other questions by 1963.

Schramm's founders story is taut, airbrushed and Whiggish. And the essay, which he was to publish in revised form at least five more times over the next three decades, was embraced by an insecure field. This paper argues that Schramm's entrepreneurial cunning, normally linked to his institution-building alone, in fact helped shape the field's memory of its self.

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Seminar Room 1. Histories of psychoanalysis.

Reading Lacan in Buenos Aires: The A(r)mour of words in Lacanian psychoanalysis during the last dictatorship in Argentina (1976-83)

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The turbulent decade of the 1970s in Argentina culminated in a bloody dictatorship installed by a military coup in 1976. This regime lasted until 1983 and was characterized by a growing polarization of intellectuals. Many intellectuals, among them practitioners and students of psychoanalysis, died or disappeared; others were forced into exile and those who remained were subject to rigid censorship. During this period there were as many as 50 study groups reading the texts of French psychiatrist and psychoanalyst Jacques Lacan. They operated in the shadows, outside educational institutions and gave a place of refuge and safety to many intellectuals who did not leave Argentina during this period. This phenomenon appeared at a

time shortly after the military coup-d'état, when the universities ceased to be places of intellectual freedom. Thus, an underground university of sorts began to develop in a social environment of increasing violence and censorship, an environment in which theocratic notions of "man" (including women and children), "fatherland" and the "purity" of a Western-Christian ancestry created an escalating hostility towards progressive, pluralistic and secular ideas.

In the 1930s, when psychoanalytical discourse and anti-fascist sentiment in Argentina together united a group of intellectuals, artists and scientists, Freud's theories provided a place of refuge from the increasingly fascist national environment at home, and a connection to the anti-fascist struggles unfolding in Europe. In this article, it is similarly argue that this place of protection was also offered to Argentinean intellectuals in 1976-83 by the interdisciplinary roots of Lacanian psychoanalysis. It could be said that, with many variations, Lacanian reading groups created their own *Reading Lolita in Teheran.*

The hypothesis is put forward that reading Lacan was a refuge, a space of safety where intellectual activity (questioning) was allowed to continue to exist and to explain the hostile environment. This study offered an opportunity for the members of these groups to examine what Lacan's texts offered to them that was so important. Moreover, it examines the role of language in the creation of alternative, protected places and undertakes a critical discourse analysis of the texts put forward by the military regime, such as speeches, media interviews and government documents. Among other questions, the author poses the following: What constituted the identity of an Argentinean subject in the discourse of the military regime? What features were central, as opposed to merely accidental, attributes of the Argentinean subject? Specifically, what sort of ideal "man," "woman" and "child" was the military regime promoting in their official discourse? What categories resulted from this ontology? How are they different in psychoanalysis?

An exploration of this discursive universe exposes the antithetical counter-culture to the military regime embodied in psychoanalytical thinking. Such an analysis reveals a pattern in Argentinean history with regard to authoritarian/fascist preferences in philosophical ideals that have been commonly opposed to the often "subjugated knowledge" of the autonomous, pluralistic, secular "man," who struggles with his/her Argentinean identity.

The author concludes that the military regime in Argentina saw itself as protecting not only a distinctive doctrine, but also promoting a way of life and a way salvation from the corrupting influence of communism and modern, secular ideas. For the members of the dictatorship who idealized the army as an organic representation of the fatherland, the Argentinean identity was Catholic and Hispanic and rested on the foundations of Western Christian civilization.

Although the Lacanian study groups did not deliberately set out to politically oppose the regime, they opposed by implication. Psychoanalytical ethical and philosophical principles promote an individual who is autonomous from hierarchical orders and is secular in his/her worldviews. Whilst rooted in a Western intellectual tradition, psychoanalysis aspires to understand the universal in human beings and encourages a pluralistic society in which members of minority groups maintain independent traditions. In this sense, it not so much political opposition that this article studies but the contrast between two ontologies by examining the contributions of psychoanalytical thinking, which is characterized by the questioning of obedience to rigid hierarchical authority and the promotion of an autonomous and secular human being.

Argentinean intellectuals were engaged in a well-known fight, shared by many countries in different historical periods where the situation of intellectuals was grounded in their political reality as they forged the path to anti-authoritarian, secular and pluralistic societies. It is during these times that the imbrications of resistance and creativity offer a

place for language to speak and create a different "house of being." Furthermore, the tension between authoritarian and anti-authoritarian and between theocratic and secular discourses has great significance in today's political climate around the globe.

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From the Pharaohs to Freud: Psychoanalysis and the Magical Egyptian Tradition

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One can search the literature of psychology and psychoanalysis in vain for any discussion of the influence of ancient Egyptian mythology on the pioneering work of Sigmund Freud. To be sure, Freud didn't make it easy to trace the connection. However, his library contains over forty books devoted to the subject, suggesting that he was far better acquainted with ancient Egyptian cosmology and mythology than he ever acknowledged. Given the exposure of the

ancient Egyptians to the culture and language of the Israelites in Canaan (and vice versa) it isn't surprising to find that elements of Jewish mysticism also played a significant role in the genesis of Freud's theories. A strong case can be made that Freud's theories of libido, aggression and the death wish would never have been developed the way they had if he hadn't fallen under the spell of Egyptian mythology. In my paper I will also demonstrate that such fundamental Freudian concepts as his delineation of the oral, anal and phallic phases of sexuality, have their roots in ancient Egyptian mythology. Even a controversial theory such as penis envy has its antecedents in Egyptian myth. And while Freud's theory of incest explicitly pays homage to the Oedipal myth, it should be noted that what was a tragedy to the Greeks would have been business as usual for the ancient Egyptians whose mythology positively teems with incestuous deities and whose history is crowded with incestuous royal couples. In one version of the Egyptian creation myth, for instance, the demiurge produced the whole of creation out of his own seed, masturbating to create a pair of deities: Shu (god of dryness) and Tefnut (goddess of humidity) who then mated to produce the first humans – a brother and a sister naturally.

Freud's investigation of personality, too, reflects the overriding preoccupation of ancient Egyptians with the 'double' or the doppelganger. For the Egyptians the survival of the personality was assured by the preservation of the other self, embodied in Ba -- the 'soul' or 'self,' regarded as "a kind of reflective ego or voice for mental or spiritual matters" in the words of L.D. Handkoff – and in Ka, a vital force or spirit which was a guardian and provider of the individual in the afterworld as well as his invisible duplicate in life. In such conceptions we can see the glimmer of Freud's own theories of personality.

The mythology of third millennium Egypt even informs the theory and practice of early psychoanalysis. In Exodus Moses refers to Egyptian priests as 'wise men, sorcerers and magicians.' In a more candid moment Freud might have admitted that such a job description could as well apply to the analyst. I will argue, too, that such techniques as free association owe their origin in large part to the power that ancient Egyptians invested in words, in language, and above all in names. An individual's name (*ren* in Egyptian) on a statue was viewed – to quote Handkoff again -- as "a very real part of the dead person and a participant in all of his needs and procedures." Indeed, an individual's name holds considerable magical significance "as an extension of its bearer, a source of his power, and a possible route to the inner being." I'm not sure that Freud could have put it any better.

Perhaps Freud wished to take sole credit for discovering concepts that actually had been introduced thousands of years earlier, although he obviously reshaped them to fit his own purposes. Perhaps he thought that by acknowledging the influences of ancient Egyptians and Israelites he would be giving additional ammunition to his critics. In either case, it's high time to bring Freud's own other self out of the shadows and into the light of day.

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Seminar Room 2. Measuring attitudes in wartime

A Transatlantic Mission: The Change of German Attitudes from Authoritarian to Democratic as Witnessed in the Survey Reports of American Military Government, 1945-1946 – 1949

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This paper investigates the changes in German attitudes as they were being measured in survey research conducted by Information Control (ICD) of OMGUS (Office of American Military Government in Germany) after the end of World War II. The 191 survey reports have been the subject of a four-year research project now completed at Heidelberg University, and our findings have been that these survey reports documented the social change that took place in postwar Germany in the course of the transformation from National Socialism to a democratic society, in the time period between 1945/1946 and 1949. In other words: There was no mere lingering-on of pro-Nazi attitudes, although here was no wholesale acceptance of democratic ideas either. The Germans recovered over a period of years from their state of shock after the collapse of Nazism, and their attitudes changed noticeably only after changes had taken place in the social structure of postwar German (American zone) society as the Germans also underwent re-education and re-orientation.

I argue that although the change of attitudes was slow – indeed it was slower than the changes in the class structure that were also being ascertained as they were in the surveys and survey reports –, the change in attitudes away from pro-Nazi tendencies did happen as the transformation of Germany took shape. Eventually, the reorientation of public opinion became visible in the data as they were reported on by ICD.

My paper focuses on the transformation of Germany after WWII in terms of Weberian sociology, looking at the transformation from charismatic-traditional to rational-legal authority. My analysis of post-totalitarian society and its changes is based on the data procured by American military government survey research, and I aim to relate such empirical material to the sociological theory of system change (particularly, apart from Weberian, Parsonian thought). I have argued elsewhere that American military government

followed a unique conception of how to deal with German society in the "system system change." I argue in this paper that American sociology in the postwar peri documented the changes in mentality as well as social structure that took place as (We Germany was on the way to develop into a stable democracy.

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A forgotten "liberal moment" – attitude surveying in the South African Army during the Second World War

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It is generally accepted that the Second World War boosted research on "social attitudes" to a prominence previously unheard of. The best known of this work was the research done by the

Research Branch of the Information and Education Division of the US Army, supervised by Samuel Stouffer, culminating in the four-volume series, The American Soldier.

The impulse for these studies generally was to find out "what the soldier thinks." There was another, parallel impulse during the War, to educate soldiers in a number of areas, not the least being "democracy", or "what we are fighting for". The British, Australian and South African armies all had an Army Education Scheme (AES) in place since 1941.

In the South African attitude research to be discussed in this paper, these twin impulses of finding out what the soldiers thought, and how they ought to be educated about various aspects of the war and about society, coalesced in a survey conducted among 7000 white South African soldiers in May 1944. It was published in 1945 as "What the soldier thinks".

Why is this little-known piece of research of interest to historians of psychology?

To start with, it demonstrates the concurrent development of social surveying methodology and the measurement of social attitudes, and how the latter depended on the former. The rise of the sample survey is particularly noteworthy since 1940 (Desrosieres, 1991).

It is yet another instance of Foucauldian governmentality. The need to establish the opinions or attitudes of soldiers, and to educate them, was almost explicitly stated in such terms. It was accepted without much argument that the war-time management of soldiers' opinions and attitudes was important, and that more needed to be known about them. Knowledge was essential, and the attitude survey provided the technical means to produce knowledge. In his chapter on attitudes, Danziger (1997) remarked on this "pervasive modern trend" (p. 147) as well.

War facilitates social science. Jennifer Platt (1996) identified the opportunity the US Army research gave to conduct a large number of empirical studies to test alternative methodological strategies as one of its most important features. In addition, the War made communication between scientists easier, and powerful social bonds were established between the researchers, that lasted well beyond the war years. This indeed was the case in South Africa as well, where the men (almost exclusively) who formed part of these research efforts, took up important positions in the social science research community in this country.

Locally, the survey provided evidence that contradicted the results of South African surveys on race attitudes until then. The responses were encouraging to the version of South Africanism that the AES officers were promoting, a non-racial, liberal democratic South Africa. 47% of respondents agreed with the statement that "Natives ought to be given education of the same kind as Europeans"; 42% agreed that "Natives should be given more political rights than they have at present, but only gradually as they become more civilized"; and 54% that "The gold mines ought to be completely owned by the state and run purely for the benefit of the people as a whole".

The political context in Britain, Australia and South Africa was a significant backdrop to Army education, and to the surveys. When news leaked out of some of the findings of the South African survey (e.g. about state control over mines, and racial matters), the AES officers were accused of socialistic and communistic ideas. MacKenzie (1992) noted something similar in Britain, and that Tory concern about the left-wing character of the Army Education Corps was not entirely without foundation. Dymock (1995) quotes one source to say that Australian Army Education was "an obvious point of communist concentration" (p. 120).

Finally, Shula Marks argued that "worlds of possibilities" for reform existed in South Africa after the War, as it released left and liberal energies. The vision of a common society, of a more inclusive South African citizenship, open to the inclusion of Africans, seemed possible, and was indeed central to the curriculum of the AES. Saul Dubow (2005) is of the

opinion that there is plenty of evidence that the Army education as a "formative intellectual experience had an enlightening effect on many young soldiers". But quickly, in 1948, that became a "world of impossibility", as South African politics wasted this liberal moment in the country's history.

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12.30-1.30 LUNCH

Main Auditorium

Cold War Psychology	
1.30-2.00	Sandra Schruijer (University of Utrecht), A European Association of Social Psychologists in the Cold War
2.00-2.30	Jill Morawski (Wesleyan University), Robert Rosenthal's Studies of Experimenter Bias: From Cold War Anxiety to Civil Rights Democracy
2.30-3.00	Ian Nicholson (St Thomas University), "Shocking" Masculinity: Milgram, Obedience, and the Crisis of Manhood in Cold War America

Seminar Room 1

Psychology of Language: Historical Approaches	
1.30-2.00	Colm Kelly (St Thomas University), The Construction of a Scientific Object and the Institutionalization of an Academic Discipline: the Cases of Durkheim and Saussure
2.00-2.30	Tord Larsen (Norwegian Institute of Science and Technology), Defining Moments in the History of Meaning
2.30-3.00	Michael Carhart (Old Dominion University), Travel Reports and Comparative Linguistics: Malayo-Polynesian

Seminar Room 2

Child Development	
1.30-2.00	Edward Morris and Kathryn Bigelow (University of Kansas), Child Rearing as the Behaviorist Views It: Watson's Advice in Historical Perspective
2.00-2.30	Werner Deutsch and Christliebe El Mogharbel (Technical University of Braunschweig), A Breakthrough in Scientific Collaboration: Clara and William Stern's Project on Developmental Psychology
2.30-3.00	Anton Yasnitsky and Michel Ferrari (University of Toronto), Early Vygotskian Psychology after Vygotsky: the Kharkov School of Psychology

Main Auditorium. Cold War Psychology

Political and intellectual considerations in founding a European association of social psychologists during the Cold War

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In my paper I present my latest results concerning my study into the American motives to help found a European association of social psychologists. The European Association of Experimental Social Psychology (EAESP) was conceived and formally created during the Cold War and initially sponsored by the Ford Foundation, Social Science Research Foundation and NATO. It was intended to help create a European social psychology that was to function besides an American social psychology.

In the USA, the first decades after the war were characterized by a strong belief in the importance of the social sciences. "Social sciences seemed to hold special promise for addressing the challenges of the postwar era" (Bender, 1998). The funding of psychological research came from the government, the military, the SSRC, and various foundations. Mark Solovey (2001) has coined the term 'politics, patronage, social science nexus' to refer to the collusive relationship between politics, funding agencies and the social sciences. There is reason to believe that the USA was especially interested in influencing European left–wing non-communist intellectuals, so as to be able to keep a check on Western Europe and its political developments (Stonor Saunders, 1999; Jachec, 2000).

The EAESP was informally born in 1964 and became official in 1970. Its official aims are: "… to promote excellence in European research in the field of social psychology" … "… it contributes to the scientific communication among European social psychologists as well as between Europe and Social Psychology in the world at large".

In this paper I report on the more than ten interviews I have conducted with the European pioneers of this association (members of the first committee and later presidents) and present their thoughts concerning the relationship between American and European social psychology, between the American and European interest in developing EAESP and to what extent they are aware of any political considerations and their attitude towards them. Also, I have consulted EAESP's formal archives based at the university of Louvain.

The EAESP can be called a Cold War baby. Not only because she was born during that period, but also because her existence was serving the American international policy agenda well. Through the EAESP contacts could be made with Eastern Europe and Latin

America. Through getting the European (Eastern and Western) psychologists organized, their possibly political activities, could get explored. Key actors within social psychology were likely to be unaware of the fact that a perfect alignment of agendas of the American government and Western European social psychologists existed.

Robert Rosenthal's Studies of Experimenter Bias: From Cold War Anxieties to Civil Rights Democracy

Jill Morawski
Wesleyan University

Within the community of experimental psychology, Robert Rosenthal is generally regarded as a methodological hero, credited for discovering "experimenter effects" or "experimenter expectancy" in the 1960s and then empirically demonstrating the pervasiveness of such expectancies both inside and outside scientific laboratories. Rosenthal is recognized for observing that experimenters can bias the outcome of experiments at various stages of research, from the tacit cueing of subjects in the experiment proper to nonrandom computational errors in the final tallying of experimental data. Even today, a half century later, Rosenthal's name often appears in research methods texts, eponymously signaling the potential intersubjective problems of human experimentation and marking as well, technical prophylactics for preventing such social contamination of laboratory events.

This conventional history of the experimenter expectancy is partial in several respects. Rosenthal was not the first researcher to address the problem of intersubjective relations in the experiment or even the experimenter bias: from William James on, a number of psychologists have attempted to address such problems (Morawski, 2005). Further, although Rosenthal's empirical demonstrations are represented as path breaking research of the 1960s and generally associated with the liberalized intellectual thinking of that decade, his first observations were made in the 1950s (Rosenthal, 1956) and were just one of a many sites where experimenters were seeing and worrying about intersubjective dynamics of experimentation. More significant, perhaps, is the general disregard for the experimental problems raised by Rosenthal and others: despite homage in methods textbooks, the matter of experimenter intersubjectivity goes largely unattended. Many researchers have shared social psychologist Edward Jones pronouncement that such methodological concerns are overblown if not hypochondriac. Jones offered a different history: "A unique convergence of events in the late 1960s brought about an exacerbation of self-criticism that became identified as 'the crisis in social psychology.' To some extent this despairing rhetoric fed on itself" (1985, p.47). Such narrative smoothings as well as textbook homage to the "Rosenthal effect" ignore the vibrant history of experimenters' struggles to create trouble-free experiments where, in the scientific spirit of standardization and mechanization, it is assumed that subjects attend only to the scientifically selected variables -- to the "stimuli" -- and to nothing else.

Rosenthal's early research on experimenter "bias" serves as an exemplary case study of experimental psychologists' keen concerns and consequent ritualized management of the dynamic social world of the human psychology laboratory. This paper examines Rosenthal's research program from its inception in the mid 1950s through the 1970s, beginning with its emergence in a generally unacknowledged climate of anxious malaise among American experimental psychologists. Taking the approach of biography of a scientific object (Daston, 2000), this historical examination locates the origins of Rosenthal's first study (his Ph.D. dissertation) and his psychoanalytic interpretation of experimenter expectancy (initially called "experimenter unconscious bias") in a pervasive Cold War climate of suspicious and

anxious regard of others. Historical excavation of the early years of experimenter bias also reveals the generally unnoted relay of psychoanalytic ideas among otherwise staunchly positivist, behaviorist-inclined psychologists. Following this scientific object through Rosenthal's subsequent studies shows how this one empirical demonstration of the dynamic social life of experiments served, for some, as a libratory symbol of human agency. Thus, the humanist psychologist Sydney Jourard (1968) lauded the subjects' psychological complexity and call for a more "democratic" experimentation where subjects as well as experimenters demanded honesty and openness. For others, however, Rosenthal's work demanded response: those committed to standardized experimentation needed to address and ultimately remove the anxious idea that experimenters were somehow not entirely "objective" or that they somehow unknowingly contaminated the pure science transpiring in the laboratory. For these researchers, experimenter expectancy effects had to be technically controlled if not eliminated. Rosenthal's (1969, 1998) eventual renaming of the scientific object, notably removing the terms "unconscious" and "bias", aided the project of technical containment, as did his shift from observing experimenters to studying teachers, employers and other authority figures.

Experimenter bias or what came to be called experimenter expectancy entail an instance of recognizing what Steven Woolgar (1988) has called the "methodological horrors" of reflexivity: realization that the scientist's very being is an constituent part of scientific observation. The human sciences, experimental psychology in particular, have long struggled to avoid, evade, or dismiss reflexivity, and the history of Rosenthal's experimenter bias research stands as a significant case of such regard and disregard.

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'Shocking' Masculinity: Stanley Milgram, Obedience, & the 'Crisis of Manhood' in Cold War America

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Stanley Milgrim's study Obedience to Authority is the most famous psychological experiment of the 20th century. First published in 1963, the experiment remains a staple of

undergraduate education in psychology, but its renown extends well beyond academia. For example, during the famous 1974 trial of Patty Hearst, Milgram's research was cited to explain how the kidnapped heiress could come to obey the directions of the Symbionese Liberation Army. More recently, Milgram's experiments have been mentioned in relation to the behavior of American prison guards at Abu Gharib prison in Iraq (Lemov, 2005). The study's sizable legal and political profile has been mirrored in popular culture. The obedience research has been made into a Hollywood film with no less a figure than William Shatner - Star Trek's Captain Kirk – playing the role of Milgram.[12] The study has also appeared in comic books, a song by rock musician Peter Gabriel and it has been alluded to in an episode of the television show The Simpsons. As social psychologist Lee Ross has noted, the Obedience experiments have become "part of our society's shared intellectual legacy – that small body of historical incidents, biblical parables, and classic literature that serious thinkers feel free to draw upon when they debate about human nature" (cited in Blass, p.283).

Controversial from the outset, Milgram's study generated a voluminous secondary literature when it first appeared – and it continues to provoke discussion within psychology and without. Debates about the ethical propriety of Milgram's work have a particular vitality, but there have also been extensive discussions concerning the validity of the obedience effect, methodological issues, and the theoretical underpinnings of obedience (Blass, 2000; Miller, 1986). Notably absent from this extensive commentary, are sustained attempts to situate the study within the cultural and political context of the environment where the study actually occurred: early 1960s America. The reason for this omission is not difficult to discern: Milgram himself provided an extraordinarily compelling social context to explain what the study was about. From the outset, Milgram linked the obedience experiments to the Holocaust:

> Obedience, as a determinant of behavior, is of particular relevance to our time. It has been reliably estimated that from 1939-45, millions of innocent persons were systematically slaughtered on command. Gas chambers were built, death camps were guarded, daily quotas of corpses were produced with the same efficiency as the manufacture of appliances. These inhumane policies may have originated in the mind of a single person, but they could have only be carried out on a massive scale if a very large number of people obeyed orders (Milgram, 1963, p.371).

Although some scholars have disputed the validity of the obedience research as an 'explanation' of Nazi genocidal behavior, most Americans saw the connection. The experiment attracted a lot of attention, much of it framed in terms of the Holocaust. This pattern continues right into the present day. Psychologists have been particularly inclined to see take Milgram at his word and see the Obedience research in terms of the Nazi genocide. In a recent survey of psychology textbooks, 86% made explicit reference to Nazi Germany and 19% specifically mentioned Eichmann (Saltzman, 2000). My intention in this paper is not to refute the holocaust-Milgram connection, but to supplement it with a detailed consideration one of the study's most compelling and hitherto unacknowledged features: its gendered, cold war character. Milgram himself noted that the obedience study was primarily a study of American men: of the nearly 1000 subjects in the study all but 40 were men. However, in a process well documented by feminist scholars such as Donna Haraway (1991), the idiom of natural science rendered the politicized, masculine gender construction of experimentation invisible. Following established discursive practices in psychology, Milgram presented the obedience research as a test of culturally inert 'subjects' in the face of a

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[12] Entitled "The Tenth Level," this made for TV movie aired on CBS in August 1976. Milgram served as a consultant on the film and Shatner starred as the Milgramesque character 'Stephen Hunter.'

generalized 'authority' thereby implying that the immediate political context and gender construction of the participants and the experimenters were largely irrelevant. The move enabled Milgram to position the obedience study as part of grand, universal discourse on 'human nature' and thus applicable to such diverse historical contexts as Nazi Germany and the war in Vietnam.

In the present study, I draw on unpublished materials from the Milgram Papers to suggest that the Obedience research was not a 'timeless' experiment on 'human nature,' but an historically contingent interrogation of American masculinity at a time heightened male anxiety (Gilbert 2005; Kimmel, 1996). Far from being a bold or dramatic challenge to American understandings of human nature, the study was in fact a continuation of a decade long lament over a seemingly lost or enfeebled American masculinity. I argue that this gendered context invested the Obedience experiments with an extraordinary plausibility, immediacy and relevance. Immersed in a discourse of masculinity besieged, Americans read the Obedience not as a fanciful study of laboratory brutality but as confirmation of their worst fears. American men were losing the individual agency and self possession that had made the country great. If something was not done, vigorous, freedom loving, American men might well become docile obedient Nazis or mindless, drone-like communists.

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Seminar Room 1. Psychology of language: Historical approaches.

The Construction of a Scientific Object and the Institutionalization of an Academic Discipline: A Textual Analysis of Emile Durkheim and Ferdinand de Saussure

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Durkheim and Saussure have been compared on a number of occasions over the years, but never very systematically. The proposed paper will compare their work in three related ways. 1. The role of the concept of the *conscience collective* in constructing, in each body of work, the scientific object under investigation, namely 'society' and '*la langue*' respectively. 2. The role of each body of work in helping to institutionalize their respective disciplines of sociology and linguistics. 3. The role in each body of work of a subversive stratum which destabilizes the object of scientific investigation, namely Saussure's research on Latin anagrams, and Durkheim's little discussed but crucial concept of contagion.

An analysis is presented of the steps by which Ferdinand de Saussure attempted to define the object of the science of general linguistics. It is shown that this object, the language structure (**la langue**), is a structure in so far as it is stored in the **conscience collective** of the community of language speakers. Saussure's argument thus has a sociological basis, even a 'Durkheimian' one. (However, there is no evidence that Durkheim influenced Saussure.) Saussure, however, cannot rigorously determine the boundaries of the langauge-structure. As he shows himself, signs, and even dialects and languages, blend and mingle into each other, in a chain of "differences without positive terms." Unable satisfactorily to define the object of general linguistics, Saussure does not publish his thoughts. The Course in General Linguistics is published by colleagues after his death, and Saussure's name becomes linked to the attempt to establish general linguistics on a secure scientific basis.

Durkheim's central concern was the nature of social institutions, and of the *conscience collective* and the *representations collectives* at their basis. Durkheim was more certain than Saussure of the coherence and stability of his object of study, and he vigorously promoted the science of sociology. Despite this certainty on his part, his writings demonstrate that, like Saussure' appeal to the fiction of the *conscience collective*, he implicitly had to appeal to that self-conscious and enlightened society, that ideal society, which in his discourse is the *telos* of the real society, in order to secure 'society' as a stable scientific object.

Prior to and during the years in which the *Course in General Linguistics* was offered, Saussure carried out research, which was never published, on anagrams in Latin poetry (Starobinski, 1979). Saussure discovered that the lines of the poems repeat in a systematic pattern the phonemes of the theme, usually a proper name (Starobinsky, 1979: 11-12). Saussure speculated that the practice might have originated in the repetition of an invocation, prayer or hymn, incorporating the syllables of a divine name (Starobinski, 1979: 42). This explanation should be compared to Durkheim's thesis concerning the contagiousness of the

sacred. The idea of a force residing in a sacred name, and of an identity between force and name, derived from Durkheim, may complement Saussure's hypothesis. More important, however, is the fact Saussure's 'subversive' research on the anagrams, and Durkheim's 'subversive' concept of contagion, destabilize the supposed objects of general linquistics and sociology respectively, and as a result are never given prominent attention in the history of their respective disciplines.

The main method to be used is a close reading of the relevant texts, with reference to the French originals, and to manuscript versions where relevant, and also with some reference to well-known facts concerning the roles of Durkheim and Saussure in establishing their respective disciplines. The theory is a deconstruction-influenced analysis of the underlying precariousness of supposedly stable scientific objects.

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Defining Moments in the History of Meaning

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During the 20th century the human sciences were obsessed with the problem of meaning, and "the linguistic turn" triumphed in discipline after discipline. In my own field of anthropology the turn was named "the shift to meaning", and three of its herolds were Clifford Geertz, Marshall Sahlins and James Fernandez. The preoccupation with hermeneutics and interpretation has an unbroken tradition in American anthropology since Franz Boas. In Britain, the work of Evans-Pritchard is seen as a turning point in the transition from functionalism to interpretative anthropology. And in France, it was the Saussurean revolution in linguistics which set off structuralism in anthropology, mainly associated with the work of Claude Levi-Strauss..

The new century, on the other hand, has seen the resurgence of naturalist approaches in the human sciences. Efforts to identify the biological basis of morality, naturalistic approaches to the study of religion, cognitive science and various new schools of linguistics tend to challenge – or at least complement – the existing varieties of "verstehende" human and social sciences.

So, it is perhaps time to take stock and gain a perspective on these developments by looking at "the history of meaning", or rather, the history of efforts to understand humans as sense makers. It all began, we may say, with Plato's dialogue *Cratylus*, in which Socrates debates the nature of names with Hermogenes. The debate between the Greek dialecticians and rhetoricians centrally concerned issues of meaning and communication, and Aristotle's *Rhetoric* and his pioneering study of metaphor are still with us when we debate these matters.

The tripartite division of the Greek *logos* into rhetoric, logic and grammar was carried over into the Middle Ages, which – beginning with St. Augustine's *De Doctrina Christiana* – saw a proliferation of treatises which investigated the relationship between world and word. In some medieval texts, it is apparent that the Christian theme of the incarnation and the Aristotelian distinction between form and matter adumbrate modern, secular theories of meaning.

A radicalization of semiotic issues came with John Locke and his *An Essay Concerning Human Understanding* which paved the way for Enlightenment thought on signification (e.g. Condillac), before the Romantic revolution (e.g. Rousseau and Herder), cleared the ground for a new role for language as constitutive of our world. This view came to fruition in the linguistics of Wilhelm von Humboldt which generated the principle of linguistic relativity and a thoroughly modern concept of culture, which was developed further by 20th century cultural theorists like Ernst Cassirer and anthropologists like Edward Sapir and Bejanim Lee Whorf.

The work of Gottlob Frege marked the linguistic turn in philosophy and logic, which was taken a step further by the Vienna Circle and the early Wittgenstein. Wittgenstein's later work on meaning, together with Peirce's semiotics and Saussure's structuralism had profound effects on the human and social sciences, challenging different versions of positivism and giving a communicative twist to social and political thought (e.g. Habermas). The philosophy of language had replaced the philosophy of the subject and the philosophy of consciousness.

The paper cannot possibly survey this vast field of learning. Rather I would like to identify and characterize key moments in "the history of meaning" and examine their relationship to theories of society and human nature: a) the scientific revolution, which designified nature, b) the romantic revolution and the romantic crisis which replaced the episteme of representation with the idea of a linguistic constitution of reality, c) the present situation in which we witness a new naturalism which strives to reinstate a biological vocabulary as the key to our understanding of man as user of language and maker of symbols.

I would also like to propose a revitalization of Giambattista Vico's program for the study of forms of signification and their relation to political, social and technological realities. Theories of meaning do not take place in an autonomous realm, separate from social life, but are constituted by – and constitutive of – the social reality they articulate.

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Travel Reports and Comparative Linguistics: Malayo-Polynesian

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While the conventional narrative places the origin of modern comparative linguistics in the study of Asiatic languages, particularly the Indo-European language family from which are descended the Sanskrit, Romance, Germanic, and Persian languages, much of the important work took place in the study of a separate world region, the Pacific. Employing the same methodologies as Jones, Rask, and Bopp, linguists like William Marsden, J. C. Adelung, J. S. Vater, and Wilhelm von Humboldt investigated the relationships between the Pacific populations, attempting to discover how human beings, equipped with only the barest maritime technology, could have undertaken and survived voyages hundreds of miles into the uncharted unknown, from New Zealand to the Marquesas, Easter Island, and Hawaii. Europeans themselves, equipped with compasses, star charts, and ships that could tack upwind, only reached these places in the 1770s. From these Pacific investigations came the

invention of another major language family, the Malayo-Polynesian, which is entirely distinct from any language originating on the Asian mainland.

In the 1770s, Jones in India and Marsden in Sumatra compiled vocabulary lists and learned the grammatical construction of the languages with which they were in contact. A generation later, Rask travelled to Iceland for the purpose of linguistic fieldwork. But most of the linguistic theorists - including Bopp, Adelung, and Humboldt who puzzled together the major language families - never left Europe. Instead they were dependent on the observations of world travellers, who supplied them with empirical data. That is, men of action traveled to specific locations and later published accounts of what they saw and experienced. Men of letters appropriated those reports, extracting particular facts from one report and reading them against facts extracted from others. From such compilations of decontextualized data, a scientific narrative of human origins, migrations, and linguistic relationships was constructed.

Seminar Room 2. Child Development

Child-Rearing as the Behaviorist Views It: John B. Watson's Advice in Historical Perspective

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Throughout the 1920s, John B. Watson, the founder of behaviorism, wrote for the popular press on a variety of topics, among them child rearing. His advice was criticized then, as it is today, but the criticism is problematic. It is usually based on selected aspects of his advice and is predicated on presentist norms. To assess the criticisms' validity, we review what Watson actually advised, the criticisms of his advice at the time, and how his advice compared to other advice of his day.

Watson wrote nine articles child rearing between 1922 and 1929, some of which he and his wife, Rosalie Rayner Watson, expanded and published in their 1928 book, Psychological Care of the Infant and Child. Among the topics he addressed were negative emotional reactions, misbehavior, daily routines, and love and affection. His advice about negative emotional reactions was that they be kept to a minimum by maintaining a positive, non-threatening home environment. On the topic of misbehavior, he urged its prevention, advising parents to keep their children engaged in appropriate activities and to use punishment sparingly. As for daily routines, Watson emphasized their importance for promoting desirable habits. About love and affection, he advised that, if evoked too much or too often, they would lead to emotional stunting and over-dependence. To guard against this, Watson and Watson (1928) advised:

> Let your behavior always be objective and kindly firm. Never hug and kiss them, never let them sit in your lap. If you must, kiss them once on the forehead when they say good night. Shake hands with them in the morning. Give them a pat on the head if they have made an extraordinarily good job of a difficult task. (pp. 81-82)

Watson's child-rearing advice about negative emotional reactions, misbehavior, and daily routines was consistent with the norms of his day. His advice about love and affection, in contrast, met with critical reactions. The Housewives' League was ad hominem: "Watson must be a very unhappy man to offer such ideas." Pediatricians blamed Watson for an increase in infant sleeping problems. Dell (1930) argued that Watson was factually in error:

Research demonstrated that withholding love and affection can result in children's failure to thrive. Similar criticisms could be found 50 years later. William Kessen, for instance, stated that Watson's "attitude toward children, his attitude toward parents in the psychological care of the child is, it seems to me, pathological" (Senn, 1975, p. 29).

These criticisms continue today, but the question remains: How did Watson's advice about love and affection compare to that of his day? A review of the 1920's child-rearing literature shows that his advice was not unique, but provided for different reasons and in the minority. West's (1914) Infant Care, for instance, warned parents against kissing babies on the mouth, not psychological, reasons, but for medical reasons (e.g., infection control). Other sources, however, advised against demonstrations of love and affection for reasons closer to Watson's. West also warned parents not to play with infants, so as to avoid inducing a "nervous disturbance" (see also Blatz & Bott, 1929; Holt, 1929; Thom, 1927). Watson's and West's, however, was not the norm. The norm was more balanced. Love and affection between parents and children was appropriate and should be promoted (see Groves & Groves, 1924).

Why Watson proffered such advice is largely unknown and difficult to determine. Among the reasons may be, first, that it reflected a generational shift from romantic sentimentality toward children to modernism's turn toward science and technology. Second, social change may have been influential: Women growing independence might have supported a more detached approach to child rearing. And third, Watson's childhood may have played a role: His strong attachment to his father was shattered when his father deserted the family. Perhaps he wanted to prevent similar effects.

Overall, the child-rearing advice of the 1920s varied across and within those who provided it (e.g., physicians, psychologists) and across and within developmental domains (e.g., misbehavior, love and affection). In his own time, Watson's advice on negative emotional reactions, misbehavior, and daily routines was consistent with that of his day, while his advice about love and affection was not, but not unheard of either. Purely presentist accounts do not provide a nuanced account of Watson's advice – a lesson from historicism. In the end, Watson (1936) regretted some of his advice because it was poorly informed. This is a lesson from science: Child-rearing advice should be empirically based.

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A Breakthrough in Scientific Cooperation: Clara and William Stern's Project on Developmental Psychology

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Anyone visiting Max Planck Institute for Psycholinguistics in Nijmegen, the Netherlands, will come across an ancestral gallery dubbed "Brains on a Broomstick". In front of a large slab of slate on which citations of some classics of psycholinguistics are engraved, a row of bronze heads are propped upon metal rods. With only one exception, all these represent men's heads - persons who have accomplished outstanding things in psyholinguistics, such as Paul Broca who discovered the form of aphasia which was later named after him, and F.C. Donders, the famous ophthalmologist. The only woman in this ancestral gallery is Clara Stern (1878-1945), who hadn't had any academic education but who, together with her husband William Stern (1871-1938), had given developmetal psychology a new look in empirical as well as theoretical terms.

It is exactly a hundred years ago, 1907, that the Sterns' book 'Die Kindersprache' (child language) was published. This volume is still available today, in German. Up to now there is no English translation of this book, for obvious reasons, it being almost impossible to translate German child language into English.

The aim of our contribution is to reconstruct the way Clara and William Stern worked together to let this book come into being. Sprung & Sprung (1996) investigated the paths women took in the 19th and beginning 20th century to reach a prominent position in psychology as a field of research. Of the five models they present, the model that fits best for Clara Sternis the partner model: the woman as her husband's co-worker. William Stern

always pointed out that his wife did not just carry out his plans but that they worked together as equipollent partners. This is shown by their both sharing the same study, sitting together at one large desk.

The book "Die Kindersprache" is based on the Sterns' diary notes about the development of their three children. The diary project seems to have been largely Clara's responsibility. It began with the birth of their first child and was carried on till 1918. In the end it comprised twenty-four volumes or several thousand pages. These diaries have experienced a veritable odyssey from Hamburg via Durham/North Carolina and Cambridge / Massachussets, coming finally to rest in the National and Jewish Library of Hebrew University in Jerusalem.

The Sterns' diary project was begun at a time when the breakthrough of developmental psychology as an empirical science had already succeeded, and the developmental diary was a recognized method which had aided developmental psychology to established itself as an empirical science. From this point of view, development is no longer measured by normative standards determining at what age children should show which abilities, but by the facts acually observed in the child. Development is now viewed from the child's point of view.

The main work in this field is William Preyer's book "Die Seele des Kindes" (1882). Preyer, a paysician of German/British parentage observed his son three times a day from birth to the age of three and gathered a large number of developmental facts. However, he did not offer a general developmental theory. Clara and William Stern took up the thread with their diary project, but they went far beyond Preyer's work in theoretical sophistication due to their philosophical background.

The centre of the Stern's diary project were three individuals, Hilde, Guenther and Eva, and following the personalistic idea a separate developmental story was traced for each child in a sepearate diary volume. As a first step, the developmental history of the child was described in a holistic psychographic way without specializing on any aspect. Only in a second step did the Sterns examine the material with a view to special aspects such as the development of language, of memory or of drawing. These efforts brought forth two monographs, one of which was "Die Kindersprache". This method demands an immense effort over a long stretch of time, which would seem quite impossible for modern researchers. In this light Clara Stern's accomplishment is all the more noteworthy. Today the diary method is no longer the main method of research in developmental psychology, but a special method for special purposes. Episodes of developmental beyond childhood demand methods that are not dependant on a close contact between the observer and the observed person.

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Early Vygotskian Psychology after Vygotsky: An introduction to the Kharkov School of psychology

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Contrary to popular belief, Vygotsky's framework for psychological research introduced into the Soviet Union around the end of the 1920s was not abandoned and forgotten until its rediscovery in Russian and America around the end of the 1950s. In fact, Vygotsky remained the spiritual leader for a group of his former students and collaborators, even after his untimely death in 1934. This paper attempts to reconstruct the intellectual history of Vygotskian psychology at the earliest stages of its development, immediately after Vygotsky was no longer there to direct it.

During the 1930s, Vygotsky and his closest allies Alexander Luria and Alexei Leontiev conducted a wide range of psychological studies in such diverse fields as neuropsychology, the ethno-cultural studies of minorities, human developmental pathology, the psychology of art, verbal thinking and practical intellect in children, the development of memory and attention, concept formation, and educational psychology. Lev Vygotsky was not only an extremely talented and versatile psychologist, he was also a gifted teacher, fostering a generation of young scholars who continued his wide-ranging research. In particular, Vygotsky's students in Moscow, Leningrad, and Kharkov, Ukraine established the so-called 'Kharkov School of Psychology'; this school is of particular interest to the Vygotskian scholars as the most direct and straightforward conduit Vygotskian psychology from its founding to the present day. But surprisingly, the history and the actual early studies done by Vygotsky's students at the Kharkov School remain virtually unknown to Russian and Western scholars alike. This period of the 1930s is still referred to as a "blank spot in the historiography of [Vygotskian] psychology" (Zinchenko & Mescheryakov, 2006), or a "major hiatus in American knowledge about Soviet psychological research"(Cole, 1977).

The focus of this paper is on the work of this group of Vygotsky's students who left Moscow in the early 1930s to establish a research centre in Kharkov, in the Ukraine, relatively far from the dangers of life in the capital of the Soviet Union in the Stalinist era—a group typically referred to in the history of science as the 'Kharkov School of Psychology' (Cole, 1980; A. N. Leontiev, 1986; Valsiner, 1988; Sereda, 1994; Zinchenko & Morgunov, 1994; Ivanova, 2002; A. A. Leontiev, 2005; A. A. Leontiev, Leontiev, & Sokolova, 2005). The only systematic account we have of the Kharkov school and of research studies done there during the 1930-s comes from the personal notes of Alexei N. Leontiev—the nominal leader of the school—first presented by his son, a prominent psychologist himself, Alexei A. Leontiev in 1983 (A. A. Leontiev, 1983), and first published in 1988 under the title *Materials on consciousness* (A. N. Leontiev, 1940-41/1988). The impact of this publication was considerable: Leontiev's account has been disseminated widely by a number of later authors (Sokolova, 2001; Ivanova, 2002; A. A. Leontiev, 2005) and seems to be considered virtually the sole and certainly the most authoritative source on the history of the school.

In this paper we critically revise Leontiev's account based on a number of rare, never translated, archival materials written in the 1930s (published and unpublished) gathered from libraries and archives in Kharkov. These documents allow us to explore three main lines of the research done by the members of the school at three main scientific and educational establishments: the *Ukrainian Psychoneurological Academy (UPNA),* the *Ukrainian Scientific Institute of Pedagogy (UNDIP),* and the *Department of Psychology at the Kharkov State Pedagogical Institute (KhDPI)* (which gradually became the sole and the main research center of the school). Diverse studies of animal psychology, human verbal behavior and

practical intellectual activity, knowledge acquisition and forgetting, children's play, and the psychological mechanisms of perception and sensation—along with methodological discussions of the distinctions between psychological and physiological research—are all framed by these scholars as highly interrelated research done within the framework of Vygotsky's unique vision of a psychology of consciousness. Thus, our study of these original archival materials reveals much richer picture of the Kharkov school's research programme than that presented by Leontiev who, after all, was writing in the very narrow context of his own *'materials on consciousness'*. Our analysis also shows that the development of this research programme was continuous throughout the decade of the 1930s. Indeed, the end of the decade appears to be its most productive period, contrary to the view that in 1936 all psychological research substantially decreased and that "this was in fact the end of the Kharkov school" (Haenen, 1996).

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3.00-3.30 TEA

Main Auditorium

Symposium: Crisis? What Crisis? Causes, Contexts, Consequences of the "Crisis in Psychology" in the Early 20th Century	
3.30-4.00	John Carson, Cries of "Crisis" in Turn-of-the-Century French Psychology
4.00-4.30	Annette Mulberger, Kostyleff's Book on the Crisis of Psychology and Its Reception in Spain
4.30-5.00	Uljana Feest, Edmund Husserl and the Crisis of Philosophy
5.00-5.30	Thomas Sturm (Convenor), Bühler's Crisis of Psychology and the Origins of Popper's Critical Rationalism

Seminar Room 1

Psychology and Philosophy	
3.30-4.00	John Rickards, Kant's Cognitive View of Concepts
4.00-4.30	Regine Plas, Theodule Ribot as Reader of Schopenhauer
4.30-5.00	David Leary, GS Hall, the Origins of Pragmatism, and the History of Psychology
5.00-5.30	Hroar Klempe, Kierkegaard and Experimental Psychology

Seminar Room 2

Public Displays of Psychology	
3.30-4.00	Peter Behrens, Popularizing Psychology in the 1930s: Joseph Jastrow and the Media
4.00-4.30	Marcia Moraes, Body and Perception: 19th c Psychology and Early Cinema
4.30-5.00	Sofie Lachapelle, From Stage to Laboratory: Magicians, Psychologists, and the Science of Illusion
5.00-5.30	Francis Neary, Psychology in Museums in Late 19th c Britain: the Museum Visitor as Automaton

Main Auditorium

Symposium: Crisis? What crisis? Causes, contexts and consequences of the 'Crisis in Psychology' in the early 20th century

Convenor: Thomas Sturm

The establishment of the first psychological laboratories and institutes in the late 19th century was frequently connected with the expectation that psychology would, finally, reach the secure path of a proper science. Soon, however, this expectation was counterbalanced by strong doubts and criticisms. Were the results of the new experimental studies to be trusted? Would different experimental approaches converge towards a unified theoretical framework? Could psychology become a respected science like physics?

Beginning perhaps with Willy (1899), one of the key terms under which these and other related questions were raised was the „crisis in psychology". In the following decades it was used frequently and by quite different authors, e.g., Nicolas Kostyleff, Lew Vygotsky,

Karl Bühler, Hans Driesch, a group of Marxist psychologists in Berlin, and Edmund Husserl. The diagnosis became so widespread that, in 1926, Bühler could write that "one can now even read it in the newspapers that there is a crisis in psychology." This debate has several interesting features. First, the diagnoses and suggestions for therapy for the apparently unhealthy state of psychology differed strongly. Second, the various authors did not often mention and discuss one another. Third, the talk of crisis reached a certain peak during the 1910's-1930's, and was then revived in quite different ways during the 1970's - 1980's.

The panel aims at inspiring more research on the „crisis" in psychology. A battery of questions can be raised here: In what contexts -- intellectual, scientific, and social -- did authors come to think that there was such a crisis? What were the causes of and reasons for the experience of the crisis? Was the diagnosis of a „crisis" expressive merely of academic worries about experimental psychology, or did it also relate to broader social concerns? What fundamental problems - -conceptual, methodological, theoretical -- did the authors detect? What solutions did they suggest? Was a similar crisis also diagnosed in other disciplines at the time? What actual effects (if any) did the various diagnoses and suggested therapies have upon the further development of psychology -- and its relevance for neighboring disciplines, such as philosophy, or for society as a whole? Last but not least, what can we learn from the origin and development of this debate for current disputes about the state of psychology?

The panel will concentrate on only some of these questions. It will do so by means of four case studies: First, an examination of Kostyleff's work and the French context in which it originated; second, the reception of Kostyleff's work in Spain; third, Edmund Husserl's path from a diagnosis of a crisis of psychology towards a crisis of European science in general; and fourth, the role of Bühler's *Krise der Psychologie* and the approach of *Denkpsychologie* upon the development of Karl Popper's antipsychologism in his philosophy of science.

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Cries of "Crisis" in Turn-of-the-Century French Psychology

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By the turn of the twentieth century, French high culture in general and French psychology in specific seemed to many to be in turmoil. Celebration of the positivistic and scientistic attitude that had characterized the early Third Republic was now strongly criticized by those proclaiming the "bankruptcy of science" and promoting reliance instead on the personal and arational. From Paul Bourget's *Le Disciple* (1889) to Maurice Blondel's *L'Action* (1893) to Ferdinand Brunetière's *La Science et la religion* (1895), alternative approaches to knowledge, society, and mind challenging the primacy of cold reason and objective science in favor of action, faith, and the spiritual flourished. Undoubtedly the most important representative within psychology of this cultural transformation was the intuitionist philosopher Henri Bergson, a star product of the French educational system. Bergson was initially a committed Spencerian; however, he soon rejected the positivistic aspects of evolution and adopted

instead—as explained in his most famous work, *L'Evolution créatrice* (1907)—an approach to the nature of mind that celebrated the intuitional and spiritual, particularly the *élan vital* (life force). By the early twentieth century, Bergson had attracted an enormous following, although his direct impact on academic psychology was more muted. Nonetheless, in the wake of such critiques of laboratory science, experimentalism in French psychology began to seem less compelling, and interest renewed in introspective techniques, pathological methods, and case-study style investigations.

It was in this context that a *maître de conferences* employed by the Ecole Pratique des Hautes Etudes, Nikolai Kostyleff, announced in 1911 that there was a crisis in French experimental psychology. In his *La crise de la psychologie expérimentale*, Kostyleff attacked much of the work in French experimental psychology for the previous thirty years as fragmentary and overly concerned with individual capacities at the expense of mental phenomena themselves. Praising Alfred Binet's *L'Etude expérimentale* (1903) as exemplifying the proper style for psychological research, Kostyleff declared that "the true goal of experimental psychology . . . [lies in understanding] the nature of psychic phenomena, of their localization and of the ties that bind them to the organism." After the interruption of virtually all scientific research occasioned by World War I, the shift in direction to which Kostyleff pointed became clearly visible within a number of areas of French experimental psychology during the postwar period. In this paper I will examine what it was about French experimental psychology that seemed so troubling, both to Kostyleff and to numerous other psychologists. How had the laboratory, which at one point was deemed the guarantor of objective scientific knowledge, especially within psychology, become a suspect site for the understanding of human mental processes? I will also explore the degree to which it actually had become a problematic space, and the degree to which French psychologists adopted the rhetoric of crisis without being able to jettison the methods that made their work seem like legitimate science in the first place.

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Kostyleff's book on the crisis of psychology and its reception in Spain

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In 1911 the Russian psychologist Nikolai Nikolaevich Kostyleff (1876-1956), then living in Paris, published *La crise de la psychologie expérimentale*. The aim of his work was to evaluate critically the present state of psychology as a scientific discipline. Although in his view psychological science had made some advances, he still found the research to be fragmented, unconnected, without a precise method, and without a clearly defined object. He worried about the state of disorientation he diagnosed in experimental psychology and what he called the missing '*unity of succession*'. With this expression he was refering to the fact that research programs in psychology were started but soon after abandoned, leaving emptiness behind. Referring to these inconsistencies he stated: "*this aspect is especially striking today and makes us think of a real crisis in the development of experimental Psychology*" (K, 1922, p. 6). Be this as it may, what is clear is that Kostyleff used the expression 'crisis' to develop a rather dramatic critique of the psychology of his day in order to gain moral advantage for introducing his own theoretical position. The kind of psychology that would lead the discipline towards a more fruitful future, he contended, should be based on the reflexological approach championed by his master Bechterev.

What now seems to me of interest is to examine the reactions of his contemporaries to Kostyleff's claim for the existence of a *crisis of experimental psychology*. Immediately after the publication of his book, the *Revue philosophique de la France et de l'estranger* reviewed the book very positively. It also gained international attention through a comment in the *Zentralblatt für Psychoanalyse und Psychotherapie*, where Maeder recommended the book, valuing especially its most critical parts. Through Kostyleff's work the discussion about a possible state of crisis of psychology eventually reached Spain, where we can find opposed reactions. Whereas a pedagogue like Rosa Sensat (1923) enthusiastically recommended the reading of Kostyleff's book and a military physician like Galo Fernández-España (1924) made Kostyleff's reflections and approach his own, psychologists such as Emilio Mira (1923), Juan Vicente Viqueira (1918) and Fernando Maria Palmes (1923) rejected it completely. In the view of the latter there was no need to talk about a crisis in psychology. Thus, ironically, the same features recognised as characterizing the development and present state of psychological science were used by some Spanish authors to diagnose a crisis, whereas others interpreted them as signs of good health and social success.

Finally, I conclude by arguing that the fact that Spanish psychologists rejected the discourse on a supposed crisis of psychology in the second and third decades of the 20th century must be understood in relation to the specific development and state of institutionalisation of psychology in the Spanish cultural context.

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Edmund Husserl and the Crisis of Philosophy

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In 1936, the philosopher Edmund Husserl published his article, „The Crisis of European Sciences and Transcendental Phenomenology" (Husserl 1982). The article was based on several lectures that he had given the previous year, one of which (presented in Prague) was entitled, "The Crisis of European Sciences and Psychology", another (presented in Vienna), "Philosophy and the Crisis of European Mankind". In this work, he laid out what he took to be the crisis of science, i.e., that its progress relies on the elimination of subjectivity, and that it therefore fails to be relevant to human life. These various titles are evocative of several discourses about crisis that were going on at the time. One of them was the concern, prompted by the rise of non-Euclidean geometry and modern physics, that the feeling of immediate evidence provided to us by our sensory experiences can no longer be trusted (see Hahn 1933 on the *crisis of intuition*). The other was the concern that experimental psychology, as practiced at the time, was incapable of doing justice to the holistic and meaningful nature of human experience (see Driesch 1925 on the *crisis in psychology*). In my contribution to this panel, I will investigate both threads. I will do so by tracing Husserl's concern with both issues back to his turn-of-the-century work, *Logical Investigations*, where he had famously launched his attack on psychologism in logic and epistemology (Husserl 1900). There he had distinguished between the subjective feeling of evidential certainty and the type of evidence required by logical arguments, relegating the study of the former into the realm of psychology. At the same time, however, the notions of intuition and evidence were central to his own philosophical method (Pieper 1993). This raises questions about the relationship between intuition as a subjective feeling (thereby constituting a subject matter for psychology) and as an objective method (thereby ensuring the autonomy of philosophy).

As has been remarked upon in the literature, Husserl's 1900 and 1910/11 critiques of psychologism are best understood against the background of an ongoing dispute within turn-of-the century philosophy about the status (both intellectual and institutional) of psychology within philosophy (Kusch 1995). These debates already at that point gave rise to the notion that there was a crisis of psychology (Willy 1899), which eventually erupted in the 1913 petition for the institutional separation of psychology and philosophy (Schmidt 1995). While agreeing with this point, I will argue that by the 1920s and '30s, the problematic had shifted some as a result of scientific, social and institutional developments. More specifically, within psychology the crisis no longer concerned the relationship between psychology and experimental philosophy, but rather the question of what was the right way of doing experimental psychology. And this question, in turn, was linked to larger cultural concerns about the alienation of modern science from ordinary human experience (Harrington 1996). This discourse, which I take to be a popularized version of philosophical concerns about the status of intuition, was seconded by another discourse about the need to demolish traditional

ways of thinking and to offer fundamentally reconstructed intellectual systems (Galison 1996).

While Husserl's philosophical approach was, of course, entirely different from that of, say, the logical positivists, his *Crisis* shared with them the spirit of fundamental reconstruction. To Husserl, both the crisis of intuition and the crisis of psychology were expressions of a deeper *crisis of philosophy*, which had failed to provide adequate theoretical foundation for science and rationality. I will contextualize Husserl's theoretical response (transcendental phenomenology) by situating it within the historical contexts of developments in psychology and philosophy, in particular laying out his relations with other philosopher-psychologists, such as Wilhelm Dilthey and Carl Stumpf.

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Bühler's *Crisis of Psychology* and the Origins of Popper's Critical Rationalism

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How did the debate about a "crisis in psychology", and the diagnoses and therapies proposed in it, affect a neighboring discipline such as philosophy? An interesting case here is Karl Popper's critical rationalism in the philosophy of science. As is well known, Popper had studied in the psychology department of Vienna University with Karl and Charlotte Bühler (Hacohen, 2001). His 1928 dissertation, originally planned as an empirical work concerning memory, was ultimately devoted to methodological problems, as indicated by the title, *Zur Methodenfrage der Denkpsychologie*. It mainly discusses the ontological and methodological

views of his two supervisors: Schlick's *Allgemeine Erkenntnislehre* (1918) and Bühler's *Zur Krise der Psychologie* (1927). Popper explicitly states that he aims to defend Bühler's complex methodology against behavioristic and other physicalistic approaches to psychology that had been defended by Schlick and others.

The relation between Popper's later philosophical falsificationism and Bühler's *Denkpsychologie* has been found problematic in different ways. On the one hand, it has been claimed that Popper's 1928 dissertation was not yet really a methodological work, or that it did at least not yet address the central methodological problem of induction (Hacohen, 2001; Gattei, 2004). On the other hand, it has been argued that Popper later downplayed too much the influence of the *Denkpsychologen* Bühler, Oswald Külpe and Otto Selz upon his philosophy of science (ter Hark 1993, 2003, 2004).

My aim in the paper is not to directly refute such views. Rather, I shall try to undermine them by examining more closely the complex Kantian background of the different authors, thereby clarifying also how a specific diagnosis of a crisis in psychology was related to Popper's turning towards philosophy. (1) Bühler defended the core of his methodological therapy for the crisis in psychology – the claim that psychologists must always take the three aspects of experience (*Erlebnis),* meaningful conduct, and the formations of the objective mind into account – by a "transcendental" argument, comparing its status explicitly with that of the system of necessary presuppositions of empirical knowledge Kant had developed. (2) Defenders of *Denkpsychologie* also largely accepted Kant's view that empirical knowledge requires certain conceptual presuppositions, although they tried to show empirically, contra psychological associationism, that human thinking is actually guided by such structures. (3) Consequently, Popper did not need Selz's "deductive psychology" in order to become convinced that induction is problematic as a method of justifying general empirical knowledge-claims, nor Selz's view that learning does not result from simple associative processes of habit formation, but consists in "trying-out behavior" (*probierendes Verhalten*), which requires so-called "schematic anticipations". The problem of induction was noted, of course, already by Hume and Kant – and Külpe or Bühler, through whom Popper learnt his Kant, did not fail to point this out. (4) Finally, instead of only looking for psychological precursors of Popper's critical rationalism, I shall consider (a) whether the empirical studies of *Denkpsychologie* are biased towards their Kantian heritage, and (b) whether Popper did not understand a crucial Kantian doctrine better than the *Denkpsychologen*, namely the distinction between the genesis and validity of knowledge-claims, and the related assumptions about the different tasks of psychology and epistemology.

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Seminar Room 1. Psychology and Philosophy

Kant's Cognitive View of Concepts

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Following Plato and Aristotle ("the soul never thinks without an image"), Locke defined an idea as an image, *albeit* not a perfect image, of something experienced by the senses. This pertained to our ideas of particular attributes or objects we have encountered. Regarding the formation of abstract ideas such as our idea of "man" Locke said, " the mind makes the particular ideas received from the particular objects to become general" by "leaving out something that is peculiar to each individual, and retaining so much of those particular complex ideas of several particular existences as they are found to agree in (Locke cited in Brock, 2003. p. 4)." However, Locke did vary on this position, at times even rejecting abstract ideas calling them "fictions and contrivances of the mind (Locke as quoted in Berlin, 1956, p.124)," and at other times positing a "vague image" in order to account for generalizability. Regarding the abstract idea of triangle Locke said that it "must be neither oblique nor rectangle, neither equilateral, equicrural, nor scalenon; but *all and none* of these at once. In effect, it is something imperfect that cannot exist (as quoted in Berlin,1956, p.125)." Despite these remarks, Locke did posit the abstractive process as the major innate mental operation involved in sensory-based knowledge. And so, however qualified and muddled, Locke's view of abstract ideas was born.

Shortly thereafter, Berkeley flatly rejected the notion that "there can be an idea which is not an absolutely determinate image, since ideas are entities...they must be concrete ideas, and cannot also be abstract, that is not determinate, not having any particular properties given to the senses or the imagination (Berkeley as quoted in Berlin, 1956, p. 19)." Regarding the idea of triangle, Berkeley agreed that "the doctrine of triangle proposed by Locke, is a conflated idea, self-contradictory, for it contains within itself inconsistent properties, e.g., of being at once equilateral, rectangular and scalene, etc. (Berlin, 1956, p.129)." Unlike Locke, however, Berkeley concluded that there are no abstract ideas in our mind, there are only *general ideas*. Whenever we think of a *general idea* we conjure up in our mind a particular concrete idea (an image in our minds of a previously experienced instance) "an idea which, considered in itself, is particular, becomes general by being made to represent or stand for all other particular ideas of the same sort (Berkeley, 1710, as quoted in Berlin, 1956, p. 122)." Thus, to Berkeley, the word "man" was just a general word or a name, nothing more.

Berkeley's argument had a significant impact on the demise of abstract ideas and the rise of nominalism. Consistent with Berkeley, Hume (1748) said, "Let any man try to conceive a triangle in general, which is neither Isoceles nor Scalenum, nor has any particular length or proportion of sides; and he will soon perceive the absurdity of all the scholastic notions with regard to abstraction" (as quoted in Brock, 2003, p. 5). Furthermore, similar to Berkeley Hume argues that when we think of a general idea "the image in the mind is only

that of a particular object, tho' the application of it in our reasoning be the same as if it were universal" (Hume as quoted in Fodor, Bever and Garrett, 1974, p. 157).

Except for J.S. Mill, the British Associationists followed Hume in their rejection of abstract ideas as did some very important American behaviorists, most notably the nominalists Watson ("thought processes" are simply mechanical "muscle movements of the larynx" and "imagery becomes a mental luxury (even if it really exists)") and Skinner (all "mental entities" are merely "explanatory fictions").

Some 200 hundred years later, the philosopher Berlin (1956) countered Berkeley in pondering the question, how can we decide which instance of triangle to use as representative of the concept, presumably all instances are not equally representative, "unless we already know which characteristics are possessed by all triangles as such...that is to say unless we already possess a general [abstract] idea of triangle (p.130)." Similarly, in 1974 Fodor, Bever, and Garrett argued that, a purely empirical analysis which rejects abstractions is not tenable. Since a concrete "image cannot resemble an abstract object," we must posit an abstract rule (or an abstract, general definition) to account for "how the image associated with a 'triangle' represents the abstract property of triangularity (p. 157)."

More contemporaneously to Berkeley, however, in 1781 Kant had assumed a position fully consistent with that expressed above in modern times. Kant (1781) said, "Indeed it is schemata, not images of objects, which underlie our pure sensible concepts [*empirical concepts*]. No image could ever be adequate to the concept of a triangle in general. It would never attain that universality of the concept which renders it valid of all triangles, whether right-angled, obtuse-angled, or acute-angled; it would always be limited to a part only of this sphere.The schema of a triangle can exist nowhere but in thought. It is a rule of synthesis of the imagination, in respect to pure figures in space. Still less is an object of experience or its image ever adequate to the empirical concept; for the latter always stands in immediate relation to the schema...as a rule for the determination of our intuition [sensory experience], in accordance with some specific universal concept (Smith (trans.),1965, pps.182-83)."

Thus to Kant, *empirical concepts* depend on *schemata* not *images*, and *images* depend on *schemata*. *Images* are inadequate to the *empirical concepts,* and so can only be connected to them through their *schemata*. The more abstract and general representation, *schema,* is developed first from a variety of instances (as perceptual *images*) of the class of objects being schematized then the less abstract representation, the *empirical concept* is developed from the *schema* since the *schema* is a "rule for the production of [empirical] concepts (Korner,1955, p.70)." The *empirical concept* can then be used in order to recognize the perceptual image as an instance of a particular concept or to produce an image appropriate to the concept but only through the intermediary of the *schema*. As Korner (1955) said, "the schema is a rule for the production of images (p.70)." That is, the abstract, general schema are "referential rules that link [empirical] concepts to perception (Korner, 1955, p.71)." or they are "rules for image model sequences by the concept of object-in-various-successive-relations-to-perceiver (Sellers, 1978, p. 9)."

In sum, Kant argued that we possess two "concepts" of any sensed object-an abstract *schema* (somewhat similar to Jung's *archetype* or Bartlett's *schemata*) and a less abstract *empirical concept* (akin to the modern *semantic features* view of Collins and Quillian, 1972), as well as a perceptual *image* or an memorial *image*. In addition, there are the very abstract and general *transcendental schema* that are used to apply the *a priori* Categories of Understanding to our perceptions (*phenomena*) at the most general level (Johnson, 1987).

If we view *images* as the "surface structure" and *schemata* as rules as the "deep structure" we have in Kant a model of concept learning that is in the spirit of the modern, more articulated and more powerful model Chomsky (1957) proposed in the language medium. What we have learned from Chomsky's model is that "the only way known for

organisms to 'make infinite use of finite means'-such as they do in recognizing an indefinitely large class of objects as members of the same concept- is to employ 'grammars'...whose rules make use of abstract entities. Only grammars that allow indefinite recursion and employ nonterminal symbols in their derivations of surface structures from deep structure can do this (p. 29)." Upon analysis, it seems that Kant's view of concepts was a precursor to the modern approach to cognitive psychology.

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Theodule Ribot as a reader of Schopenhauer

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Théodule Ribot (1839-1916) is traditionally looked upon as the "founding father" of French scientific psychology, even though he trained as a philosopher. In his first book, *La psychologie anglaise contemporaine*(1870), he presented the English empiricist, associationist and evolutionist philosophers (Hartley, James Mill, John Stuart Mill, and Spencer) as models for the "positive" psychology he hoped and prayed for, as opposed to the French school of philosophy's "spiritualist" psychology. In 1873, he published his French doctoral thesis, which he attended the previous year (at the same time as his Latin thesis about Davis Hartley), *L'hérédité psychologique*, inspired by the work of the French alienists on degeneration and that of Francis Galton on heredity. In 1874-1875 he published, with his friend Espinas, a two-volume translation of *The Principles of Psychology* of Herbert Spencer, whom Ribot admired ardently.

In the midst of these publications on the new "positive" and "evolutionary" psychology, Ribot also produced *La philosophie de Schopenhauer* (1874), which had 14 editions up to 1925. Though mentioned by historians of French psychology in their biographies of Ribot, this work has never, as far as I know, featured in an analysis of Ribot's development as a psychologist.

At first sight, this neglect seems understandable, as Schopenhauer's philosophy seems out of line with Ribot's dominant intellectual preoccupations. In this paper, I will try to understand why Ribot was so interested in Schopenhauer that he decided –or agreed- to write about his philosophy. On the one hand, I will argue that, in spite of traditional French historiographical readings of Ribot, which see him as central to the divorce between philosophy and scientific psychology in France, Ribot remained a philosopher and, in reading

Schopenhauer, found the opportunity to question some assertions presented as certainties of his previous work. On the other hand, Schopenhauer was probably one of the sources of inspiration of the psychology of emotions that Ribot developed in the second – and the most interesting - part of his career. Rather than being marginal to Ribot's intellectual development, Schopenhauer may have been significant to the more innovative aspects of his work.

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G. Stanley Hall, the origins of pragmatism, and the history of psychology

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The story of the origins of American pragmatism, as commonly told, emphasizes a series of papers published by Charles S. Peirce in 1877 and 1878, and an address delivered by William James a full 20 years later, in 1898, at the University of California at Berkeley. This latter address, entitled "Philosophical Conceptions and Practical Results," is said to have revived Peirce's proposal – or rather, his set of related proposals – from the oblivion to which they had sunk and to have initiated the more or less continuous tradition that has run, sometimes quickly, sometimes slowly, right up to our own time, when Richard Rorty and others have placed pragmatism back in the spotlight.

There have been criticisms of this story (e.g., by Fisch, 1954 & 1964, Hollinger, 1980, and Kuklick, 2001), but these critiques have focused either on what happened *before* Peirce's 1877 and 1878 articles or *after* James's 1898 presentation. They have left untouched the question of what happened in the years between Peirce's formal proposal of pragmatism and James's call for the development of pragmatism.

In this presentation, I will discuss what Peirce and James did with regard to pragmatism between 1878 and 1898 and, more significantly, focus on the ways in which G. Stanley Hall drew upon the thought of Peirce and James to develop a distinctive and consequential approach to psychological research. In doing so, I will clarify how this one-time student of James and one-time colleague of Peirce adopted pragmatist ideas and then used them to advance psychology in the late 1880s and 1890s, before James delivered his address at the University of California at Berkeley. Of particular interest will be Hall's 1878 Harvard doctoral thesis; an 1879 article in which he first publicly championed Peirce's early papers on pragmatism; his 1884 inaugural address at Johns Hopkins University, in which he proposed a formal plan for the development of a branch of the "new psychology" that he labeled "historical psychology"; and the subsequent development of genetic or developmental psychology.

Although I will acknowledge that Hall drew inspiration from the folk psychology of Wilhelm Wundt, I will document how his historically based developmental psychology was related to the pragmatic assertions and concerns of Peirce and James. Moreover, I will indicate how Hall and his students pursued this research, with innovative effects, in the late 1880s and throughout the 1890s at the new Clark University.

Interestingly, though his research program was clearly pragmatic in orientation, Hall was convinced, for some reason, that it would be "liable to be injured" if it became "conscious" of this fact (Hall, 1912, p. 426) – a curious statement, both grammatically and otherwise, that may have meant that Hall feared criticism if the pragmatic nature of his research became too apparent. Why that would be so isn't clear, but if that was what he meant, his theoretical quiescence may well have reflected the wariness – and perhaps the duplicity – that plagued Hall's life and career (as documented so fully by Ross, 1972). In any case, whatever the meaning of his cryptic comment, it doesn't reduce the probability or obviate the relevance of what I wish to claim.

Even if time does not allow a very extensive discussion, I will at least point toward the ways in which the works of Ralph Waldo Emerson and Friedrich Nietzsche had an impact on the history of developmental psychology in America through their provision of background and corroboration, respectively, for Hall's pragmatic inclinations.

Finally, beyond coverage of the immediate topics under consideration, this presentation will be aimed at contributing to a possible and appropriate revival of interest in Hall's consistently overlooked *intellectual* as opposed to his widely recognized *institutional* contributions to the history of psychology (see Leary, 2006).

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Kierkegaard and experimental psychology

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The Danish philosopher Søren Kierkegaard (1813-1855) and his existentialism have contributed a lot to psychology, especially during the twentieth century's humanistic psychology. In that sense his contribution to psychology can be regarded as being more indirectly. Not so much focused on, however, is the fact that he also contributed more directly to what he himself called "psychology". In 1843 he wrote a small book entitled: "Repetition; An Essay in experimental psychology" (Kierkegaard 2001). This is a very special essay, and not very easy to understand. The content is primarily focusing on two aspects: a young man's break with his fiancée, and the same man's travel to Berlin. The essay is very much about the young man's thought about this break in retrospect, and therefore it is also a kind of love story, although it is most appropriate to read it as a contribution to Kierkegaard's existentialistic thinking. In short it is very much about different ways to experience time, in which there are primarily two sorts: the cyclical and the linear (Lindhardt 2001). The cyclical implies repetitions, whereas the linear does not. This essay was written the same year as "Fear and trembling" was written, which must be regarded as one of the most interesting contribution to Kierkegaard's existentialistic thinking. Thus there is no doubt about the fact that "Repetition" primarily is a existentialistic essay.

In my paper, however, I will argue that this way of reading is not satisfying. It is ignoring the fact that Kierkegaard mentioned the term "experimental psychology", and the

crucial question is: What did he mean by that? The essay was formulated many decades before Wundt established his laboratory in Leipzig. Nevertheless I will argue that very many aspects of the text will make sense by relating it to some core aspects of the early experimental psychology formulated by Fechner and Wundt. The aspects focused on will be sensation and lower forms of acquiring knowledge, in which feelings and subjectivity plays the most important role. As an argument for how common it was to think in these terms at that time, I will also refer to the Norwegian Philosopher, but first of all known as a poet, Johan Sebastian Welhaven (1807-1873), who in 1841 wrote a small thesis entitled "Metaphysic in hundred paragraphs" (Welhaven 1965). This is a typical pre-Hegelian text, which focuses very much on the Leibniz-Wolffian distinction between higher and lower forms of acquiring knowledge. Already Leibniz made a direct connection between sensing and uniqueness, whereas the general is connected to thinking. Thus the main hypotheses of the paper is that the distinction between higher and lower forms of acquiring knowledge represents a core aspect for understanding the first experimental psychology as based on a not Hegelian German idealism, which units very much of the thinking at that time. By examining the two texts very closely it will be argued that the subjectivity of existentialism is a direct consequence of a rationalistic way of regarding sensations.

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Seminar Room 2. Public displays of psychology

Popularizing Psychology in the 1930s: Joseph Jastrow and the Media

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This paper examines the career of Joseph Jastrow (1863-1945) as he evolved from conventional academician to unconventional popularizer of psychology. From graduate training at The Johns Hopkins University, to faculty years at the University of Wisconsin, to retirement years in New York City, Jastrow's career followed an intriguing trajectory, particularly in the use of traditional and nontraditional media to promote his views. This paper focuses particularly on his work in the 1930's, when he made extensive use of public lectures and broadcast radio for his ideas. This was a time when radio was developing as more than a source of entertainment (Ricks, 1984). In this regard, this paper argues that Jastrow was both behind and ahead of his time.

Born in Poland and raised in Philadelphia PA, Jastrow was a graduate student in psychology at The Johns Hopkins University in Baltimore with John Dewey, James McKean

Cattell, and Christine Ladd Franklin, to name a few. Jastrow received the first American Ph.D. in experimental psychology from Hopkins in 1886. He went on to the University of Wisconsin, Madison, to hold the chair of Experimental and Comparative Psychology for 40 years (Cadwallader, 1987). When the American Psychological Association was organized in 1892, Jastrow was its first secretary under G. Stanley Hall, its first president. The next year Jastrow organized the first public exhibition of psychological apparatus at the Chicago World's Fair and set up a laboratory for visitors for the demonstration of sensory, sensory-motor, visual acuity, color blindness, and reaction time tests. He was also a member of the first APA committee on mental and physical tests, appointed in 1895 (Jastrow, 1930).

At Hopkins and early in his tenure at Madison, Jastrow was an academician in the fullest sense of the term; that is, he was connected to the psychology of the experimental laboratory, the publication of original research (Jastrow, 1910), the review of scholarship (Jastrow, 1905), contributions to the profession (Jastrow, 1906), and the mentoring of students . However, Jastrow was never far from promoting psychology to a larger audience: the American public (Jastrow, 1899; Jastrow, 1911).

To the extent that Jastrow "drifted," it was from an orthodox, scientific psychology to an "everyman's psychology" that addressed the needs and trends of American life and culture. He did this through lectures, popular articles, books, syndicated newspaper columns, and, in his retirement, an extraordinary number of radio broadcasts (Historical New York Times, 1851-2003). But, in this process, he became marginalized and irrelevant professionally. For example, one of his major works which appeared during World War I was The Psychology of Conviction. It was a text with reprinted and original articles (Jastrow, 1918), but decidedly unscholarly. Wrote a reviewer: "Books like this are a survival of a stage of psychologizing that is rapidly passing" (Review, 1919). Other articles published during the Great War addressed the psychological origins of human character in biological and environmental forces, but in popular, not scientific, terms.

Following his retirement from Wisconsin in 1927, Jastrow was in a position to devote considerable time and energy to the promotion of his ideas about psychology and history, psychology and superstition, and psychology and effective living, albeit to the general public (Blumenthal, 1991). He marketed himself through advertisements placed in The New York Times and lectured publicly to civic and religious groups, such as YMCAs and YMHAs. He lectured at Columbia University and held an appointment with the New School for Social Research from 1927 to 1932. An example of a popular lecture was "Superstition, Prejudice, and Fanaticism" to a YMCA meeting in Brooklyn, New York (Jastrow, undated brochure).

Not one to be harnessed by convention, Jastrow found advantageous for his purposes the new medium of network radio. For example, beginning in 1934 he entered into an arrangement with "Woman's Radio Review" which lasted until 1939. The programs expounded his views on psychology, history, war, culture, and democracy. Although many of his broadcasts were based on his previous publications and public lectures, some were original and best understood in the context of the Great Depression (Historical New York Times, 1851-2001).

The historical record suggests that Jastrow was innovative in using popular media for the promotion of his views when his audience was no longer the academic community, but the general public. Furthermore, this paper proposes that Jastrow made the transformation to popularizer at just the time in American history when psychology could serve the interests and challenges of the Depression-era populace through the new medium of radio. This, it is argued, is not unlike the conditions of today that give rise to psychologists and therapists utilizing various media, most recently the internet.

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Body and perception: Notes on 19th century psychology and early cinema

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The body fragmentation was considered as an epistemological problem in the 17th century by the Irish scientist and politician William Molyneux (1656-1698). In a letter to philosopher John Locke, Molyneux asked him: how does a blind born man behave if he suddenly recovers his vision? Many philosophers and scientists studied this problem in the following centuries. The main question of Molyneux was to determine how knowledge acquired by touch could be relayed to another sense, such as vision. In other words, Molyneux was interested on the relations among senses. This question opened a realm of investigation to Psychology and to Philosophy. Rationalists and empiricists philosophers have different points of view about this subject. Discussions concerning Molyneux's problem took a new turn with 19th century Psychology. As is indicated by Jonathan Crary (1990) 19th century Psychology was investigating visual perception as a knowledge that depends on subjectivity. In other words, 19th century visual perception researches was concerned to an epistemological field which was not related to the referent, but which was related to the body of the observer. The body was considered the support of perception. So, the body, with its own properties, was the stuff of knowledge. The observer was an incarnated observer. In this paper we intend to study this epistemological field of 19th century psychology focusing on the constitution of the observer. What are the characteristics of the incarnated observer? The body was considered as a physiological stuff, fragmented in different senses. As earlier suggested by Molyneux's problem, knowledge depends on body. Each sense has its own way of functioning. Visual

perception was articulated with physiological sensation and it was quantified. The incarnated observer was contemporary of the invention of the spectator, particularly early cinema spectator. In this work we intend to indicate the relations between the observer and the spectator of the early cinema. Early cinema occurred in a brief period of time - from 1895 to 1905. In that period movie was not narrative, it was a spectacle based on a movement visual illusion. We consider that it is possible to articulate the incarnated observer, studied by 19th century psychology, and the spectator of early cinema. In 19th century the link between movie and psychology was indirectly established by the physiological experiments about sensation and visual perception. But we consider that we should go further than that and ask: What are the differences and the similarities between incarnated observer and the early cinema spectator? If we consider the relation between movie and psychology in 20th century we should affirm, following Aumont (1995), that movie makers used psychology in order to predict spectator's reactions. Different psychological theories are applied in this way: gestalt theory, behaviorism, psychoanalysis, and others. Investigating these questions our aim is to write a history of psychology in the European cultural and epistemological context of 19th century.

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From the stage to the laboratory: Magicians, psychologists and the science of illusions

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In 1894, Alfred Binet published "La Psychologie de la prestidigitation," an article based on experiments, interviews, and films he had made with some of the most famous magicians of the capital at the time. Binet has gotten Dicksonn, Raynaly, and Georges Méliès, amongst others, to come into his Laboratoire Psychologique at the Sorbonne to produce for him some of their visual illusions and tricks. The magicians agreed to reveal their secrets and even to be photographed by the chronographic apparatus of Georges Demeny, a collaborator of Etiennes-Jules Marey, while performing. With up to thirty photos a second at regular interval, the most effective of tricks of dexterity were revealed : "...the photographic proof completely destroys the illusion...," wrote Binet in his article. He was not the only psychologist showing interest in magicians and their illusions at the time. In the span of a

little over a decade, James Scully, Max Dessoir, and Norman Triplett, for example, all studied the art of conjuring and the psychology of deception as experienced when attending a magic show. By observing willing performers or reading available books, they each hoped to get some insight into the ways in which magicians were able to play their audience with feats of dexterity, clever commentaries, and a great deal of self confidence.

Feats of dexterity and illusions were not new, of course. Magicians could claim to be part of a long and ancient tradition of entertainment, secrecy, and mystery. But by the second half of the nineteenth century, magic had entered its golden age. In France, the Théâtre Robert-Houdin provided Parisians with a stage where the natural and the supernatural appeared to meet every evening. Magicians called themselves "professeur de physique amusante." They believed themselves to be presenting their own scientific experiments coupled with pleasant tricks and entertaining commentary to the crowd gathered in the theater. Outside, anyone could enter the shop of a magic dealer and purchase a book providing a set of instructions to follow in order to perform an array of impressive tricks. All that was required was a certain talent and a lot of practice. In a book addressed to amateurs, De Vere explained that in order to do magic, two things were essential: "...dexterity of the hands and the fingers and confidence in oneself." He went on to provide his readers with a set of advice to follow in their attempts to familiarize themselves with the art of magic. For example, a good magician should never reveal details on a trick beforehand for it could inspire the audience to attempt to figure out the procedure. Also, one should never agree to perform a trick twice but never refuse to do it either, for fear that it could appear impolite to the audience; rather, one should propose to present a similar trick but produce it in a very different way. More importantly, magicians should always distract their spectators with words so that they forget to pay attention to the hands. The magician Richard had further advice for beginners. For example, during your performance, take advantage of any opportunity that falls onto you; and, when you are using a particular trick, try to fool your audience into thinking that you are using another one. In other words, if the trick you are performing involves dexterity, for example, try to appear as clumsy as possible to your audience. Books such as these two were common and the advise given always fairly similar. They suggested that a behavioral code of some sort had been established by performing magicians over the century, one which early psychologists found themselves interested in.

The aim of this paper will be twofold. The first part will consist of a presentation on some of the commentaries written by magicians for other magicians or amateurs on how to behave when performing public feats of magic. The second part will focus on the works of psychologists and magicians in laboratory experiments and in the elaboration of various theories of illusions, conjuring, and deception. In particular, the emphasis will be placed on Binet and his work at the Sorbonne. In this, I will argue that magicians should not be seen as simple subjects of research for psychologists, but as active and willing participants accepting to divulge trade secrets for the benefit of science, a field they already saw themselves as being a part of and contributing to. In an era in which the mysteries of their profession were readily available for anyone who wished to learn them, it seems that magicians became less cautious in their revelations as they entered the laboratory.

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Psychology in the Museum in Late Nineteenth-Century Britain: The Museum Visitor as 'Automaton'

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In the late nineteenth century a specific brand of philosophical psychology based on evolutionary theory entered the museum in a variety of ways, leading to new methods in collecting, displaying and learning. This paper looks at the impact of a cluster of related ideas that centred around treating the average, working class museum visitor as an automaton.

Tony Bennett has identified the concept of 'the new museum' in the late nineteenth century as producing a method of display that was based on the principles of Darwin's theory of evolution by natural selection. These evolutionary principles not only informed the contents and arrangements of exhibitions but also their pedagogical methods. In the 1870s and 1880s T.H. Huxley, Henry Pitt Rivers and others promoted displays that would communicate the development or progress of types of objects by virtue of their arrangements in evolutionary sequences. Pitt Rivers emphasised the collection of ordinary and typical specimens (rather than rare objects) that could be arranged in sequence to trace the ideas of human beings from simple to complex and from homogenous to heterogeneous. He felt that the succession of human ideas could be classified and traced through the products of human industry in the same manner as the products of the animal and vegetable kingdoms. Museums of natural and human history were capable of being arranged according to a similar plan because their objects obeyed the same laws. Huxley's work on displays suggested a clear separation between the curator and the public and he defended a 'transparent but hermetically closed face' accessible to the public with the other side of the displays opening out into a space only accessible by curators. These spatial arrangements in the museum mirrored his political moves to create a middle class scientific elite that possessed the specialised knowledge that privileged them to make pronouncements on scientific, political and ethical matters.

These new, evolutionary displays were meant to be 'autonomous' both in their means of communication (without recourse to guides or reading large amounts of text) and in the way that they treated the working class visitor as an 'automaton', who could soak up knowledge intuitively without the effort of consciousness. They presented an automated learning environment in which the meaning of a collection of objects was rendered auto-intelligible through clear labelling and transparent principles of display. Using object lessons they could educate the artisan mind trained in mechanical matters, which did not have the capacity for reflection that was seen in the highly educated mind. The notion of the automaton mind defining those 'who run rather than read' served as a way of placing the working classes in an evolutionary framework and promoted a political message of self-

progress through gradual educational steps rather than sudden, socialist-style revolutions. According to Bennett, through appropriating aspects of evolutionary psychology the museum was used as an instrument for the maintenance of social order. In the social climate of labour unrest and the increasing influence of socialist organisations in the 1870s, it was conceived to have a role in public education by putting a conservative reading of evolutionary thought into an appealing and easily comprehensible form of display.

However, 'automatism' went far beyond the walls of the museum in the late 19th century: it could be found in many diverse contexts from new philosophies of mind to sites of psychical research to amusements in Victorian parlours, such as automatic writing. The paper will go on to situate 'the museum visitor as automaton' in these wider contexts of automatism in the period. It will criticise Bennett's passive, Marxist notion of the self as too narrow to support the many differing and competing styles of automatism that were forged in this period. Rather than seeing the museum as a place to shore up the conservative hegemony in this period, through describing the multiple uses of automatism the museum will be presented as a site to explore complex transitions in the social and discursive orders and new liberal ideas about the self that emphasised self-direction and self-management. The paper will conclude by reflecting upon contemporary learning styles in the museum to reveal how re-appropriated forms of automatism continue to flourish in some museum contexts and the complexity of the dichotomy between transmission and active-participant communication models.

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7.00-7.45 ESHHS AGM (Main Auditorium)

WEDNESDAY 27TH JUNE

Main Auditorium

Symposium: The Very Modern Homosexual	
9.00-9.30	Peter Hegarty (Convenor), Lewis Terman, Alfred Kinsey and the Heteronormativity of Modern Rationality
9.30-10.00	Nicola Curtin, "A Nurse with a Needle and an Old Cowhand with a Knife":Homosexuality, Castration and Desire in California in the 1940s and 1950s
10.00-10.30	Kirsten Leng, The Homosexual Law Reform Society and the Politics of Post-War Respectability
10.30-11.00	Toni Brennan, Charlotte Wolff's Pioneering Contribution to Research on Bisexuality: a Reappraisal
11.00-11.30	COFFEE

Seminar Room 1

Inventing Subjectivities	
9.00-9.30	Enrique Lafuente and Jorge Castro, "Let's Ourselves Invent!" Reinterpreting the Origins and Functions of Spanish Psychology
9.30-10.00	Aydan Gülerce, Construction of Turkish Subjectivity
10.00-10.30	Ciarán McMahon, Understanding "Attention": An Inconstant and Variable Object Versus a Willful and Diligent Practice
10.30-11.00	Belen Jimenez-Alonso, Self Governance in Sexual Matters at the First Spanish Eugenics Conference, 1933
11.00-11.30	COFFEE

Seminar Room 2

Mental Health in Institutions	
9.00-9.30	Petteri Pietikainen, Neurosis and the Ideal Productive Citizen: Clinics for the Nervously Ill in Sweden, 1920-1940
9.30-10.00	Heather Munro Prescott, Cultivating Mature Minds and Healthy Bodies: Mental Health in American Colleges and Universities
10.00-10.30	Jennifer Bazar, To Preserve or Not to Preserve: What Should be Done with the 19th c Purpose-Built Asylum?
10.30-11.00	Sheila O'Brien Quinn, Lurena Brackett, Laura Bridgman, and Mesmerism at the Perkins Institute for the Blind
11.00-11.30	COFFEE

Main Auditorium. Symposium: the very modern homosexual

Convenor: Peter Hegarty

Like psychology itself, sexuality has often been thought to have a particular relationship with the modern. Within the history of psychology, 'homosexuality,' for example, has been described as constitutive of the modern West's relationships between power and knowledge (Foucault, 1978); a stimulus for distinctly American specious scientism (Terry, 1999); definitive evidence of the social constructedness of 'mental health'(Greenberg, 1997); a minority grouop that has successfully struggled, through research, to be accepted as normal(H. Minton, 2002); an axis along which gendered power operates in disguised form (Kitzinger, 1987); and a 'sexual story' which modern subjects have only recently been able to tell about themselves (Plummer, 1994). The papers in this symposium require the history of psychology to engage with Sedgwick's claim that critical scholarship is compromised to the degree that it fails to attend to the structuring effects of the contradictory constructions of homo/hetero binaries on modern thought (Sedgwick, 1990). Our joint project is a broadly Foucaultian one, to disrupt the received story that we have passed from a period where homosexuality was 'repressed' to a modern enlightened era of sexual liberation where the 'truth' that homosexuality is an untroubling category is recognized by rationalist psychology (and its historians, see Foucault, 1978). For this reason, our papers aim to raise questions about the continuing determinative effects of homophobic thinking on categories of rationality, historical continuities in the power/knowledge relations deployed against homosexuals against sex offenders, homosexual politics prior to the late 1960s moment of 'sexual liberation' and the continual forgetting and deferral of such categories as 'bisexuality' which elude and frustrate modernist attempts to categorize people and put them in their proper psychological places.

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Lewis Terman, Alfred Kinsey and the Heteronormativity of Modern Rationality

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It is common in the history of psychology to link the rise of experimentation in American psychology to the deferral of the recognition that psychology is a reflexive science (Danziger,

1990). Biographers, in contrast the psychologists that they write about, are often enthusiastic about writing about a psychologists' work as an actualization of their private personality. When that category of 'personality' includes sexuality, explanations of authors' works can become quite uneven. As Jones biography of Kinsey shows, we remain within an era a scientist's purported homosexuality can be used to explain her or his work by appeal to folk psychoanalytic logic (Jones, 1997). In contrast, the sexological work of Lewis Terman has resisted a similar biographical impulse. Terman's biographers have acknowledged that his research on marital happiness co-occurred with a period of extra-marital affairs, and a period of marital unhappiness for his wife Anna (H. L. Minton, 1988; Seagoe, 1975), but this obvious relationship between the work and the life has failed to incite historiographical interpretation with anything like the enthusiasm that has occurred, and re-occurred a propos of Kinsey. One way to describe this era would be to say that our history of psychology is circumscribed by living in a culture shaped by Foucaultian 'sexuality' which selectively leads us to psychologizes deviations from the norm of reproductive marital heterosexuality (Foucault, 1978).

Although Terman and Kinsey met only once, their lives and works were intertwined in multiple ways. Their meeting, in April 1953, three years prior to the death of both men, was occasioned by Terman's critical review of Kinsey's first book on male sexuality (Kinsey, Pomeroy, & Martin, 1948; Terman, 1948), which had, in turn, critiqued Terman's own sexological work(Terman, 1938; Terman & Miles, 1936), and psychological research on sexuality more generally. Both Terman and Kinsey had turned to sex research after establishing themselves in an ostensibly unrelated scientific field, and both viewed it as a means of actualizing particular visions of sexual politics. These two were relatively uniquely in that they garnered research for human behavioral studies of sexuality for the Committee for Research on Problems of Sex through relationships with Robert Yerkes(Clarke, 1998; Terry, 1999). Both conducted extensive research on men convicted of sex offenses in California prisons, and – in very different ways – each advocated for prisoners' rights. Both staked out theoretical positions about the ontology of the homo/hetero binary, the relationship of married women's orgasm to family stability, and the vexed question of whether and how sex education should proceed.

In this paper will use these points of biographical intersection and theoretical overlap between Terman and Kinsey to explore how we might correct the heteronormative impulse to use purported homosexuality as an explanation of a human scientists' sexological research, to neglect heterosexuality as an impulse for scholarship on sexuality, and to reduce the field of human sexual relations to these two polar opposites. My concern will be with using this exercise to unroot some of the ways that heterosexuality has been conflated with objectivity in modern thought, in the category of 'intelligence' that Terman defined, the 'objectivity' of methods that Terman and Kinsey debated, and the standards of historical scholarship through which we remember their effects on the shape of the world we inhabit today.

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"A Nurse with a Needle and an Old Cowhand with a Knife": Homosexuality, Castration and Desire in California in the 1940s and '50s

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This paper examines a particular moment in history— California in the late 1940s through the 1950s— and illustrates how contemporary conflations between homosexuality and pedophilia, and beliefs that castration could cure or punish the latter, can be traced to the practice of surgical castration in California prisons. It explores what the castration of "sex offenders" during this time, though rare, might have to tell us about the construction of normative heterosexual male desire and the policing of the body, as well as potential tensions between the state and the medical/psychological community during this time. Furthermore, this phenomenon provides a way for us to understand current discourses conflating homosexuality and pedophilia as well as debates around (chemical) castration, still strong today.

Following World War II there was intense anxiety about the threat of communism, a push to return women who had entered the workforce during the war to the home, and a wave of sex-panic. This panic resulted in increased criminalization of, and attempts to regulate, what were considered deviant sexualities. While frequently framed as concern for the safety of women and children, this terror rather reflected a contemporary interest in regulating and defining normative sexuality, and ultimately concerned itself with the stigmatization of behavior that violated strict norms of heterosexual masculinity. Homosexual men were particularly vulnerable, given the illegal nature of homosexual sex practices and the re-emergence of the conflation between homosexuality, sex offenders and pedophilia during this time. The medicalization of both homosexuality and sex offenders created unique opportunities for both the medical and psychological professions to widen their growing sphere of influence. However, the implementation of surgical castration as a mode of treatment was a contested one, and by examining the different interests of the medical/psychiatric professions and the state in surgical castration, I seek to complicate the notion that the two entities were in easy collusion.

While at first castration seemed to increase the opportunities for medical intervention, it also undermined the power of psychiatrists to treat sex offenders, and the idea that sex offenders were mentally unsound. If the problem could be "solved" merely by altering the offenders' bodies what need was there for mental-health treatment? Many expert proponents of castration were careful to reiterate the necessity of psychological treatment along with, and

even following, castration as a means of insuring a full recovery. Thus, the instances of castration in California illuminate how a phenomena that initially increased psychiatric professional's sphere of influence, also slipped from their control somewhat. Though the use of surgical castration was limited, it illustrates how the state effectively took over, displacing psychiatric expertise. In its attempts to regulate desire and sexuality, the state made use of theories and technologies developed by the scientific and therapeutic community; sometimes, as in the case of castration; defying the terms those communities tried to set forth for its practice. While Foucault and others have rightly argued that psychiatrists and psychologists have long been in collusion with the state, in this instance is perhaps more complicated; control and power between the two seem more contested than simply shared.

Surgical castration reinstituted the body as a site of penal repression and, in some small measure, brought back the "spectacle of the scaffold." Castration as a visible mark of punishment was seen as a deterrent from individual recidivism certainly, but one might theorize that it was also a way of marking those who violated accepted codes of normalcy. In another analysis, however, castration also allows a constant surveillance of the individual and, more specifically, of desire that very much reflects the Foucauldian idea of discipline. Surgical castration was not the necessarily a public punishment; rather, it was "treatment," a means of reforming the individual in such as way as to guarantee constant surveillance of desire through the absence of that which was regarded, in part, the source of male desire. Particularly because at times the discourse did not posit the removal of the testes as eradicating all desire, but in fact claimed that it would restore normative heterosexual desire, one might think of this as a form of disciplining.

Given the differing (and vying) discourses around the practice, it is difficult to consign surgical castration during this period to any one particular purpose as it appears to have served several at once (competing purposes at times, too). However, I hope to illustrate how this relatively rare practice not only illustrates contemporaneous concerns about masculine heterosexuality, and complicates notions of how the psychiatrists and the state functioned in tandem, but also can provide some understanding of current attitudes towards sex-offenders.

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The Homosexual Law Reform Society and the Politics of Post-War Respectability

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During the1950s and 1960s, political organizations concerned with the rights and status of homosexual[1] men and women entered the public arena for the first time throughout the so-called Western World. In Britain, the first stirrings of a politicized movement concerned with the rights and status of homosexual men can be found in The Homosexual Law Reform Society (henceforth HLRS). The HLRS, founded by A. E 'Tony' Dyson in 1958, was established in London one year following the publication of the "Wolfenden Report" on Homosexual Offences and Prostitution[2], which recommended that homosexual acts between consenting males (in private, over the age of 21) be decriminalized. From its inception, the HLRS publicly maintained that its sole aim was a reform of British[3] law along the lines advocated by the Wolfenden Committee[4]; it never organized around a politicized homosexual identity, never sought to create social bonds between homosexually-identified men, and never aimed to legitimate homosexuality as a way of life. Nevertheless, the Society sought to educate public opinion on homosexuality as they understood it, and viewed such work as an essential part of their political strategy.

Upon what grounds did the Society legitimate its claims to rights and demands for decriminalization? Given the stigma surrounding homosexuality during the 1950s, the burden of proof rested with the HLRS to demonstrate that homosexual men were worthy of citizenship and participation. Ultimately, the Society turned to psychiatric discourses to legitimate its claims, and the popular media to convince the public of its cause. This strategy in turn informed the Society's 'politics of respectability', which it mobilized to demonstrate the social and civic worth of its declared constituency.

This paper traces the ways in which the Homosexual Law Reform Society used psychiatric discourses and popular media to support and propagate its political claims; it also analyzes the consequences of this strategy for the Society, and for subsequent sexual rights movements. I will argue herein that while expert discourses enabled Society members to decouple homosexuality and homosexuals from associations with sin and crime, it perpetuated representations of homosexuality as a disability and psychological 'condition' that individuals could not control. I question what kind of authorial political voice such a subjectivity allowed; I also ask what kind of citizenship such a representation of homosexuality would secure. Moreover, I demonstrate that while the popular media may have had the ability to reach a broad audience, it required a dilution or tailoring of the Society's message to make it palatable to the envisaged mainstream audiences. It required a representation of homosexual men as being "just like everyone else," and led to the creation of a dichotomy between the 'model homosexual' and the 'dangerous street queer.' It also led

female reporters to trade on their maternal authority to soften the stigma of male homosexuality.

My analysis is situated historically within the conditions of postwar Britain, and seeks to understand how the Society's mobilization of expert discourses and the media made sense in the changing social, political, economic and legal contexts of the period. The postwar period was one of rapid change and reconstruction; it was also one in which questions surrounding gender roles and sexual freedoms became central to British social and political life. The heightened post-war stress on the importance of the nuclear family, coupled with official panic surrounding the nation's declining birth rate, also threw into greater relief the 'deviant' nature of homosexuality. [5] On the other hand, with memories of a more sexually liberal homefront still intact, demands for a range of sexual law reforms from a range of different social actors became increasingly loud during this period. While the fledgling welfare state may have been principally concerned with the conditions of reproduction—both of its workforce and its own authority—it also had to adapt to new social and sexual arrangements emerging "on the ground." Thus, to satisfy both state planning and social reality, between 1945 and 1970 a series of reforms were passed which on the surface seemed to liberalize sex, sexual relations, and sexuality. [6]

Ultimately, I will argue that what critics have characterized as the Society's expert-dominated bourgeois politics of respectability must be seen in its historical context. I maintain that, given the historical context in which the HLRS found itself operating, a liberal politics of respectability geared towards legal reform proved the most viable political strategy available to them. We must understand their politics as informed by the discursive constructions of homosexuality available to it. The HLRS' politics of respectability was borne of a post-war understanding of the needs of state and social planning, particularly with respect to gender, the family, and the role of expert authority. It was a politics aware of Britain's emerging place in the postwar world.

Notes

1 Throughout the paper, the terms "homosexuality" and "homosexual" are often used instead of other possible terms as these were the words used at the time.

2 Its full, formal title is the Report of the Committee on Homosexual Offences and Prostitution, Cmd 247, HMSO, London, 1957.

3 Here it should be noted that the Sexual Offences Act, the law which finally realized the Wolfenden recommendations in 1967, applied only to England and Wales. The history of homosexual law reforms, and reform advocacy, is quite different for Scotland and Ireland; it was not until 1980 and 1982 that homosexuality was decriminalized in Scotland and Ireland, respectively.

4 The establishment of single issue campaigns—particularly regarding sexual reform initiatives—was a common strategy in Britain during the 1950s and 1960s. A notable contemporaneous example is the Abortion Law Reform Society. For more details on these and other movements, see Lesley Hall, Sex, gender and social change in Britain since 1880 (New York: St. Martin's Press, 2000) and Jeffrey Weeks, Sex, politics and society: the regulation of sexuality since 1800, Second Edition (London; New York: Longman, 1989).

5 Indeed, the Wolfenden Committee debated long and hard over whether buggery should be maintained as a special offence; the argument in favour of retaining it was that it most nearly approximated to heterosexual coitus, and "might therefore be a temptation away from it" (Weeks, 240).

6 Other important sex law reforms include the National Health Service (Family Planning) Act of 1967, which allowed doctors to give family-planning advice and to prescribe free contraceptives, initially to married women only; the Abortion Act of 1967, which permitted

the termination of pregnancy by a registered practitioner subject to certain conditions; and the Divorce Reform Act of 1969, which made it possible to claim of "irretrievable breakdown" of a marriage as the sole grounds for divorce, although it was necessary to prove it in one of five ways (unreasonable behavior, desertion, adultery, two years separation with consent, five years separation without consent).

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Charlotte Wolff's pioneering contribution to research on bisexuality: a reappraisal

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In his informative reader's guide to recent social sciences literature on bisexuality Ronald C. Fox (2004) provided ninety-two pages of references, and, at the beginning of 2007, a search for "bisexuality" on PsychInfo yielded 2586 items. This recent interest in bisexuality contrasts with the dismissal of the existence of bisexuality by early psychologists and sexologists with the notable exceptions of Magnus Hirschfeld (1914, 1926/1930) and

Havelock Ellis (1942). Freud (1905) equated bisexuality with psycho-sexual immaturity and this was the prevalent view until the Kinsey report (1948) acknowledged that sexual behaviour can be conceptualised as located along a continuum from homosexual to heterosexual rather than locked in a dichotomy. However, beyond this recognition, for many years, researchers did not engage with the subject of bisexuality.

This paper examines the pioneering theoretical and empirical contribution of Charlotte Wolff to the understanding of bisexuality. Her research, culminating with the publication of the book Bisexuality (1977), revised and expanded two years later, arguably constitutes the pivotal point in the shift from denial to affirmation of bisexuality, and the first extensive investigation of the subject. The importance of Wolff's opus, which encompasses historical, psychological, social, biological and cross-cultural consideration of bisexuality deserves more recognition than it is currently granted in the history of sexuality and of the social sciences in general. The empirical study with 75 male and 75 female participants who identified as bisexual, apart from being the first of its kind, was methodologically very sophisticated with quantitative and qualitative components, using an in-depth questionnaire devised by Wolff, autobiographical sketches written by participants, and face to face interviews.

Wolff graduated in medicine from Berlin University in 1928 after studying philosophy and medicine at a number of German universities, and practised as a doctor until the rise of Nazism in 1933 forced her, as a Jewish person, to emigrate first to France and subsequently to Britain in 1936, where she worked as a psychotherapist and eventually was granted a licence to practise medicine. Having shown strong attachments to women since childhood, Wolff's interest in bisexuality began while conducting research on lesbianism published as Love Between Women (1972). The flexibility she observed in lesbian sexual behaviour and the influence of Sandra Bem's (1974, 1975) work on androgyny led Wolff to theorize that bisexuality is the underlying basis of human sexuality although some individuals' sexual behaviour will be mainly what is generally categorized as heterosexual or homosexual. Wolff was far ahead of her time in questioning the validity of "orientation" as a meaningful conceptualisation of erotic economies. "Sexual categorization is an artificial exercise...[for example] highly sexed heterosexuals are inclined to 'try everything' in the pursuit of pleasure. Their sexual versatility includes sensual explorations which are special ingredients in homosexual love-making " (1977/1979, p.64). Like the false binary "orientation", dichotomous "femininity" and "masculinity" are always – literally – in quotation marks and seen as roles created by society, rather than characteristics that can be mapped onto a particular "sexed" body, which foreshadows Butler's (1990) account of gender as a copy with no original. In conclusion, this paper argues for a reappraisal of Wolff's work in acknowledgement of her vision in the study of (bi)sexuality.

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Seminar Room 1. Inventing subjectivities.

"Let's ourselves invent!" Some clues for a socio-cultural reinterpretations of the origins and functions of Spanish psychology

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In his influential book *Inventing ourselves*, Nikolas Rose (1996) held that the programme of modern psychology played a supporting role to the model of the subject demanded by Western democracies. Notwithstanding the close relationship that may be found between subjectivity as defined by modern psychology and the agenda of liberal democracies, however, Rose's perceptive critique seems to have overlooked the fact that it was not the same kind of individual that these democracies had always in mind. Significant differences can in fact be observed between them in different times and different places, the notion of "the psychological" differing also accordingly (Castro & Lafuente, in press).

The aim of this paper is to provide some elements for the construction of a genealogy that may be sensitive to the specificity of such differences. This will be attempted through a reinterpretation of the development of psychological fields in the Spanish case, which has been usually considered from the perspective of progress and intradisciplinary development (Carpintero 2005). In addition to an allusion to Rose's work, the title of this paper is also meant to evoke the controversial sentence "Let others (the foreigners) invent!", that was popularized by the well-known Spanish philosopher and writer Miguel de Unamuno (1864-1936) in the early days of 20th century. In Unamuno's opinion, scientific and technological progress should be left in the hands of foreign countries, as Spain had higher tasks to accomplish: those of reflecting on Spanish spiritual being and getting to know its own peculiar character. Only a few years earlier, however, Unamuno himself had admitted that such goal could only be achieved by resorting to the scientific and philosophical tools made available by those very countries. Thus, Unamuno's contradictory assertions may be taken to symbolize the tensions between the autochthonous and the foreign, the scientific and the humanistic, the progressive and the conservative, the natural and the technological, that were characteristic of the emergence and dissemination of psychological knowledge among those Spanish intellectuals actively engaged in the modern project.

The project was actually launched at the end of the 19th century, at a time when a deep national crisis was taking place as a result of the defeat in the war against the U.S. and the consequent loss of the last traces of the past Spanish colonial empire. Soon to be known as "the Disaster of '98", the crisis was but the culmination of one hundred years of social and political uneasiness that gave rise to the emergence of an important critical, ideologically

heterogeneous movement of renewal, the so-called "Regenerationist" movement, to which belonged many of the most significant politicians and intellectuals of the period. As suggested by the name of the movement encompassing them all, they were specifically concerned with what was labelled as "the problem of Spain", that is, the causes of its decay as a nation and the means for pulling it from the state of shock and despair in which the crisis had left it. In going about this task, on the other hand, many regenerationists made use of a psychological framework; and, in so doing, they became a fundamental driving force in providing psychological knowledge with its essential function as a modern science: the management of individual and collective subjectivity. Rather than finding or unveiling the Spanish people's "inner self", however, the reflection carried out by regerationist thinkers constituted a true "invention", the "invention of ourselves" alluded in our title.

In a very real way, the regenerationist movement may be said to have foreseen many subsequent theoretical and practical psychological developments (Carpintero, 1998; Lafuente, 1998; Quintana, 1998; Blanco & Castro, 2005). To say it in a nutshell, the former led to a view of subjectivity that, at a collective level, found expression in an essayistic ethnopsychology, numerous examples of which were provided by regenerationist authors themselves. At an individual level, on the other hand, it found its way into the academic world, where it reflected the prevailing political and ideological projects of social change. As to the practical developments, psychology became transformed into a technology for the making of a new kind of individual; an individual who managed to maintain its historical and cultural roots, while at the same time becoming adjusted to the productive contexts of modernity. Thus, in the regenerationist discourse, psychological technologies formed part of a diffuse set of measures for social reform. With time, such measures split into psychology's three major professional fields: clinical, including the psychopathological and criminological branches for the control of cultural abnormality; industrial, containing hygienical and psychotechnical tools for the improvement of productivity; and educational, encompassing basic pedological and pedagogical measures for the management of citizenship. In Spain, these fields took a peculiar shape in accordance with its own idiosyncratic political-ideological and socio-cultural features.

In short, the crisis of 1898 in Spain provides an excellent opportunity for rethinking the cultural role played by psychology; a role linked to the construction of a model of the subject -and, consequently, of collective life- in accordance to modernity's historical and cultural demands (Blanco, 2002). In this paper, some significant model episodes will be selected and analyzed in order to show the complex set of motives investing psychological knowledge with cultural relevance. On this basis, some clues will be offered for revealing the internal logic of psychological discourse and the resulting institutionalized practices within Spanish society.

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Construction of Turkish subjectivity: Arrested emancipation/modernization/westernization/democratization?

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Over the past decades there has been an awakening within Western scholarship that there is a co-constructive relationship between the subject matter of psychological sciences and the socio-historical context of knowledge production (e.g., Danziger, 1990; Gergen, 1985; Hacking, 1995). Nowadays it is acknowledged, at least in critical circles, that the notion of the autonomous, self-contained and unified self of Western modernity is socio-historically contingent and situated knowledge. Indeed, retrospective and hermeneutic analyses of many literary and philosophical accounts illustrate that various views of self and morality that differ from the modern notion of the human agent have had currency in different time periods within the history of Western thought (e.g., Taylor, 1989). What differentiates modern national subjectivities from pre-modern accounts are their 'imagined communities' (Anderson, 1983) and the fact that they are constructed in order to sustain modern-nation states (Gellner, 1983), even if they appear to have 'natural' (invented) roots in antiquity (Hobsbawm and Ranger, 1983).

Clearly, there are other great civilizations in history that have had originated outside the Western world and have followed diverse trajectories towards modernity. The lives of majority of the world population today are still being formed and are grounded within historical contexts of political economy, cultural traditions, religious beliefs, moral values, philosophical thoughts, discursive systems and the practices of those civilizations and human wisdom, be they invented or not. Thus, historical traces of the diversity of human subjectivity are not necessarily to be found in the past but also can be sought in the present. Perhaps, for a synchronic historical analysis of the present, the making role of psychological subject matter in its mutually constructive interaction with reality is rather limited to re-production, slow or invisible within its own cultural milieu. From a diachronic historical perspective, however, the constructive impacts of the same material on social context are rather vast, drastic, and more obvious when they meet alternative traditions. Thus the co-constructive relationship, which seemingly works in favour of the external conditions of possibility in Western reality, becomes the reverse in so-called non-Western world to such an extent that, ironically, the transformative effects of the subject matter appear as 'destructive' from within the local intelligibilities.

Whatever insight comes from an intellectual exercise or self-reflexivity in the first case becomes an existential issue and a real life experience in the latter. Both, the individual selves and the societies are caught between 'progress' towards (post)modern subjectivity and resistance to change that appears as 'regression' towards tradition and fundamentalism in the developmentally challenged world. Despite the fact that there is a growing interest in

redefining modern psychology as a social, political, historical and reflexive discipline, such re-definitions are unlikely to catch up with the rapid dissemination of the psy-complex and its past content around the globe. Together with all the other technologies of the modern Western self, these exports to the periphery have inescapably made the psychological sciences serve as colonialist/imperialist and globalizing/localizing knowledge and social practices.

My main arguments center around this problem of continuous reproduction and dissemination of the very idea that the ahistorical, elitist, static, abstract, irrelevant, essentialist, and unified Western subject is intrinsic to psychology and is universal. However, in my talk I do not criticize the 'ongoing worldification' (Derrida, 1998) of the Western sovereign subject. Nor do I engage in a debate on whether hegemonic Western thought is able to reflect on and to re-evaluate its own (concept of the) self in isolation -- without a dialogue with the Rest or others -- despite the fact that self-reflexive accounts make invaluable contributions in understanding Western modernity. Rather, I examine the current condition of Turkish subjectivity as an illustration of the entrapment described above. The powerful role of psychological disciplines in constructing the modern individual-citizen with rights, freedom, and choice, and its 'governmentality' (Foucault, 1979), have been valued in liberal democracies (Rose, 1996). Yet it is hard to talk about an official, or any political recognition of psychology in this regard that has been accepted in modern Turkish society over the past century (Gulerce, 2006). However, the diffused and imported content of psychological disciplines and practice have contributed to the socio-political climate of terminological, conceptual, ethical and practical confusion in the country. Thus, drawing from Turkey's rather unique historical context at the border of the Western and the non-Western worlds, I attempt a partial contributions to the discussions on the genealogy of the regimes of self and on Western ontogenetic accounts of the self.

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'An inconstant and variable object versus a wilful and diligent practice – contrasting modern psychological and pre-18th century understandings of 'attention''

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Long before William James (1890) said it, and still today, it seems that 'everyone' knew what 'attention' is. Having been present in the recorded English literature since 1374 and defined first in 1526, the word has a long literary history. As is often said, the purpose of a historian is to make the present strange (e.g. Tyler & Johnson, 1991), and as such, the current paper will illustrate the partiality of the modern psychological concept of 'attention'.

Three contemporary attributes will be contested: 'attention' as a psychological object, with structure and architecture; that object is normally directed and focussed intentionally towards something of interest; and that object is, equally normally, involuntarily directed to an alternative item, beyond the control or choosing of the individual in question. The presentation will give historical illustrations that are at variance with these attributes and thereby highlight certain limits in the psychological theorising of today.

In the first instance, the modern status of 'attention' as some type of metaphysical object with a certain constitution – i.e. 'mechanism', 'system', 'filter', 'deficit' etc. (c.f. Posner, 2004) – should be juxtaposed with its former history in the English language. Right up until the mid-17th century, 'attention' was largely synonymous with a certain type of 'diligence' and was generally taken to mean a certain reverential respect. One of its earliest recorded definitions occurs in Joseph Hall's (1610) The Arte of Divine Meditation and is typical of the era – "A fixed and earnest consideration whereby it [topic of meditation] is fastened in the mind." (p. 160). Not does he not invoke any mention of an underlying sub-structure, but it is clear that Hall's (1610) 'attention' is not any kind of object, but is rather a practice. Similarly, all other uses of the term up until the late 17th century imply an activity, or at the very least, a quality of an activity, not a metaphysical object. This ancient terminology has the certain logical and philosophical strengths over the psychological 'object': it seems to require fewer unobservable hypothetical entities

In the second instance, modern psychology's conviction that the object that is 'attention' is voluntarily directed towards or focussed upon the individual's intended item of interest appears unnecessarily cumbersome when compared with the word's first known definition. In his Pilgrimage of Perfection, William Bonde (1526) writes that "Attencion or intencyon for our purpose here is onely the attendaunce study & diligence yt man or woman gyueth to theyr dede" (p. 160) and in so doing simplifies the entire 'attention' process in bald terminology that some might say is sorely wanting in today's psychological literature. This synonymy of 'attention' and 'intention' is widespread in the English language prior to the 17th century and it is only with John Locke's (1690) An Essay Concerning Humane Understanding that the two are clearly distinguished.

Finally, the prevailing psychological contention that it is normal for the individual to lose control of the object known as 'attention' when distracted or surprised by some other object without choosing to do so seems very pessimistic in comparison to the literature of the 17th century and before. This loss of control, or choice, would have been quite unthinkable in those times as 'attention' was always under the control of the 'will' and 'soul'. For example Robert Hooke (1682/1705) explicitly defines 'attention' as " ... the Action of the Soul in forming certain Ideas" (p. 150) and moreover states that the 'soul' " ...can readily exert its Power more particularly and strongly to this or that Idea, according to the Determination of

its Will" (p.153). Thus, in this period there is no question but that the individual is wholly in control of their 'attention' and wilfully chooses to apply themselves – a theory much more empowering than the assumptions underlying our modern psychological approach.

As such, the way 'attention' is characterised today speaks volumes about modern society and its self-reflective beliefs. The presentation will conclude with a summary of the major influences on the changes in those beliefs; namely, the waning influence of religion on reflexive discourse, the assimilation of scientific and mechanistic language into reflexive discourse and the rise of commercial influence generally, among others. A core purpose of the presentation will be to emphasise the role of the history of psychological language and concepts as a critical tool for contemporary psychology: in enriching its discourse and accentuating the fact of its historical context; hence this research occurs within the tradition of the work of Kurt Danziger, Graham Richards and Roger Smith, with reference to such being made in the concluding remarks.

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Self-governing capabilities on sexual matters in the First Spanish Eugenics Conference (1933): The problem of responsibility

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The aim of this paper is to pursue the connection between eugenics and some of the psychological categories employed in the construction of the modern subject at the end of the 18th century, such as "responsibility". I start from the assumption that eugenics in Spain assisted citizens to the self-regulation of their sexual behaviour (in this sense, Spanish eugenics is closer to the notion of "Latin eugenics" which is proposed by Stepan, 1991 rather than the "mainline eugenics" by Kevles, 1985). The main concern about making people aware of the consequences of their sexual actions headed to certain extent towards what Rose and Novas (2005) calls "biological citizen". In that respect, eugenics has to be related to the governmentality of liberal democracies rather than to be limited to state politics alone (Foucault, 1999; Michman and Rosenberg, 2002).

This study focuses on the psychological discourses of the Primeras Jornadas Eugénicas Españolas (First Spanish Eugenics Conference, FSEC) of 1933, the "official" platform of the Spanish eugenics movement in the Second Republic. The FSEC was largely centred on the development of a new sexual code for body and mind (Glick, 2003): there was an agreement on leaving the catholic sexual morality behind, pedagogy becoming one of its most important operative tools.

The participating authors in the FSEC were conscious that the effectiveness of the eugenic ideal for collective "improvement" depended on each individual in society, on the internalisation of the eugenic knowledge and its in-corporation into daily life. In this respect, two types of eugenics or two stages in the eugenic process could be distinguished. Q. Saldaña (1933) defined them in the following way: 1) cultural eugenics, exposed and instilled into people through propaganda; and 2) prophylactic eugenics, imposed and realised through law. Q. Saldaña considered that personal responsibility had to be transformed into legal responsibility. This is an important step, if we regard that the legal concept of responsibility (moral and social, individual and collective) comes hand-in-hand with the construction of the psychological "inner life", that is to say, certain dimension of subject at the end of the 18th century (for a genealogy of "responsibility", see Villey, 1989).

Nevertheless, even though all participating authors in the FSEC agreed on the need of people's awareness in sexual reproduction, there was not an understanding on the suitability of using some prophylactic measures (e.g., sterilisation), specifically, and about allowing the State to take direct control of citizens' private life, in general. Ultimately, the core issue was in the compromise between civil rights and State action typical of liberal democracies.

Key words: Spain, Latin eugenics, sexual reform, responsibility, governmentality, risk, prevention, capacity.

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Seminar Room 2. Mental health in institutions

Neurosis and the Ideal of the Productive Citizen: Public Clinics for the Nervously Ill in Sweden, ca. 1920-1940

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My presentation examines an essential chapter in the 'Age of Nervousness' in Sweden in the first half of the twentieth century. It was a period during which Sweden underwent rapid modernisation, changing from a predominantly rural, poor and non-democratic nation of the late nineteenth century to an urban and affluent social-democratic welfare state of the mid-twentieth century. As in other western countries, paths to modernity created socio-cultural conditions in Sweden conducive to the dissemination of the language of nerves, and neurosis attained the status of a national malady or 'folk disease' (folksjukdom, Volkskrankheit) alongside such diseases as tuberculosis, syphilis and alcoholism. By the 1910s, the epidemic of nervous illnesses had begun to worry the medical and political authorities, some doctors believing that patients were using their 'ill health' to avoid work and responsibility. After the establishment of Royal Board of Pensions (RBP), the government started to open its own clinics for the care of the nervously ill (and rheumatics) in the hope of restoring the working capacity of the chronically neurotic lower-class people. This presentation focuses on the historical contextualisation of the RBP's clinics.

In 1915, the RBP began to send patients to spas and water-cure clinics, and a few years later it opened its first clinic for neuroses and rheumatism. The state was now actively involved in the fight against nervous illnesses, and nervous illnesses and rheumatism were the two national maladies with which the medical officers of the RBP were especially concerned. This was understandable, since both neuroses and rheumatism were typically afflictions that made people incapacitated without damaging their health completely. Thus the authorities at the RBP thought it worthwhile to invest in the treatment of these two groups of invalids. The RBP wanted to make sure that it did not pay retirement allowances 'needlessly' to persons who were not doomed to chronic invalidism. Neurotics, a large group of potential invalids who might become a heavy burden to the national economy, needed to be provided with effective therapy and then swiftly returned to working life. Thus the motivation behind the RBP's clinics was at least as much national-economic and utilitarian as it was medical. It was this principle of utility, as well as the medical imperative of national health policy (health is a national virtue and an obligation at the level of individual citizen) that characterised both the clinical work at the RBP's clinics and the medical discussion of the proper cure of the neurotics in the decades to come in Sweden.

The therapeutic raison d'être of the RBP was not grounded in the principle that a humane society has to provide health care for the sick, but rather in the belief that the health of (often stigmatised) neurotics and rheumatics must be restored in order to safeguard national wealth. It was not considered sufficient to merely improve the condition of the patients; they had to regain their capacity to work and become useful members of society. Neurotics, especially the more difficult neurotic patients who were in danger of becoming chronic invalids, constituted a national-economic problem that needed to be solved so as to save money and to prevent an unnecessary 'incapacitation' of the population. Those who were thought of as being beyond the pale regarding this fundamental goal were excluded from the category of patients for whom the RBP provided care (e.g. the mentally ill and those who were close to the retirement age). Thus the primary goal of these state-financed clinics was to turn neurotic patients into productive citizens.

In my presentation, I discuss the medical, ideological and socio-cultural context of the RBP's clinics. I examine the therapeutic results at the RBP's clinics (a number of studies of the therapeutic results were conducted between the late 1920s and the early 1940s), as well as the heated debate at the Swedish Society of Medicine in the mid-1930s about the proper institutional care of the nervously ill. I then draw some conclusions about the significance of neurosis as a 'folk disease' in Sweden.

The principal source material for this presentation consists of Swedish and inter-Nordic medical, psychiatric and psychological journals, and of the minutes of the Swedish Society of Medicine, including lectures given at the Society's meetings. Valuable material is also provided by medical manuals, textbooks, popular books on neuroses and nerve illnesses, conference programmes, and the annual reports of the Serafimer Hospital in Stockholm, through which many patients were referred to the RBP's clinics.

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Cultivating Mature Minds and Healthy Personalities: Mental Health Services at American Colleges and Universities, 1920-Present

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This paper will examine the emergence of mental health services for college and university students. It will build on the work of Kathleen Jones on college suicides in order to explain how college health officials first became interested in student mental hygiene as a critical student health issue (Jones 2000). One of the major arguments of this paper is that student mental health services have developed in response to changing notions of the college years as a life stage (Kett, 1977).

Initially, college mental hygienists drew upon the work of the noted child psychologist G. Stanley Hall, who suggested that the college experience prolonged the adolescent stage of life (Hall, 1900). Many of those involved in student health services and student personnel work were inspired by the mental hygiene movement of the early twentieth

century. Like their counterparts in elementary and secondary schools, college mental hygienists became interested in finding ways to prevent and treat mental disturbances in college students before they led to academic difficulties and failure ((Cohen, 1982; Pols, 1997; Jones, 1999). Student health services frequently worked in conjunction with departments of clinical psychology to study the incidence of emotional difficulty among the student body, as well as to design tests that would help predict which students would be most likely to experience maladjustments while in college (Brotemarkle, 1931). Eventually, mental health services became a part of a college or university's role in loco parentis.

Experiences with psychiatric problems during the Depression and Second World War heightened interest in the mental hygiene of young people (Fry, 1942). The 1947 report issued by President Truman's Commission on Higher Education lent further support for improving mental hygiene in colleges and universities. The report argued, "for a satisfying and successful life a person must also be emotionally stable and mature, able to endure the conflicts and tensions, the compromises and defeats, that life is most certain to bring." Therefore, colleges and universities should not only train the intellect, but also "should make growth in emotional and social adjustment one of its major aims" (President's Commission, 1948).

Yet, changing notions about the development of college students, as well as major shifts in the composition of the student body, challenged earlier views about the role of mental health services as parental surrogates. Male students, who already enjoyed considerably more freedoms than women, became more difficult to control after the war. Returning veterans, who were older and had more worldly experience than the average male college student of earlier decades, were especially reluctant to submit to supervision of their activities outside the classroom. Postwar psychological literature warned of the dangers of robbing men of the "deep and perfectly normal masculine drives" permitted them during the war (Bailey, 1999). By the 1950s, college mental hygiene experts began to emphasize the importance of mental hygiene programs in fostering mature minds and healthy personalities among the nation's youth. Experts argued that giving students a greater role in college health programs was a central part of promoting maturity and independence in college students. (Farnsworth, 1954).

This new commitment to student involvement had unintended consequences in the ensuing decade, as students rebelled against the paternalistic structure of American colleges and universities. Some psychiatrists explained incidents of student uproar as extreme signs of late adolescent maladjustment (Farnsworth, 1964). This view was increasingly challenged by a younger cohort of psychiatrists and physicians, some whom had worked closely with students on the frontlines of various social movements of the decade. (Coles and Brenner, 1965). Robert Arnstein at Yale suggested that psychiatrists adopt the role of "change agent," which meant looking beyond the unhappy state of the individual and fixing aspects of "the system" that were causing emotional problems (Arnstein, 1977). Others suggested coining the term "collescent" to describe the developmental status of young people in the college environment (Long and Long, 1970). The student protests of the 1960s culminated in the abolition of parental rules on most campuses. Unfortunately, student activism also prompted a backlash that reduced state allocations and institutional commitment to campus health programs that served "unruly" and "ungrateful" students (Robinson, 1973).

More recently, concerns about a "mental health crisis" on campus, combined with the passage of the Americans with Disabilities Act in 1990, have created new challenges for college mental health services (Kadison and Digeronimo, 2004). Finding a balance between overly protective supervision and utter neglect continues to vex professionals concerned with the mental health of student bodies in the twenty-first century.

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To Preserve or Not to Preserve, That is the Question: What is to be done with the Nineteenth Century Purpose-Built Asylum?

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The nineteenth century, particularly the latter half, saw a boom in the emergence of purpose-built "asylums" for the treatment of individuals who were deemed to be "insane." As Tomes (1984/1994) has highlighted, in contrast to the western world's present day treatment of mental illness, the asylum was the only place in the nineteenth century where physicians treated patients. Institutionalization has become less common in treatment and the physical buildings are no longer considered to be an integral component in treatment. So what has become of the asylum?

In this paper, I will explore (1) the importance of the physical asylum building in the nineteenth century; and (2) the fate of these buildings from the late twentieth century to the present day. My focus will be on the early Canadian asylums, particularly those within the province of Ontario where they were most numerous in number. A brief comparison of the current state of asylum buildings in parts of the United States and Western Europe will also be included.

The importance of the asylum building

One of the beliefs underlying moral treatment in the nineteenth century was the idea that insanity and the environment in which an individual lived were closely linked (see Brown, 1980; Edginton, 1994; Hudson, 2000). The asylum building itself, both inside and out, was therefore considered to be an important element in the treatment of insanity.

Due to the magnitude of the building's role in treatment, many theories of asylum construction and planning were in circulation in the nineteenth century. Books such as Samuel Tuke's 1815 Practical Hints on the Construction and Economy of Pauper Lunatic Asylums, W.A.F. Browne's 1837 What Asylums were, are, and ought to be, C.W.M. Jacobi's 1841 On the Construction and Management of Hospitals for the Insane, John Conolly's 1847 The Construction and Government of Lunatic Asylums, and Thomas Kirkbride's 1854 On the Construction, Organization, and General Arrangements of Hospitals for the Insane with some remarks on Insanity and its Treatment detailed specific aspects of an asylum's environment and the attributed benefits to the cure of insanity.[13] Each of these authors based their books off of their personal experiences as medical superintendents at their respective asylums: Tuke at The Retreat, England; Browne at Montrose Asylum, England; Jacobi at Siegburg, Germany; and Conolly at Hanwell Asylum, England. Asylum planners, architects, and medical superintendents also toured existing locations to determine the manner in which to construct future asylums.

The fate of the nineteenth century asylum

[13] Jacobi's text was originally written in German under the title *Ueber die Anlegung und Einrichtung von Irren- Heilanstalten, mit ausfuhrlicher Darstellung der Irren-Heilanstalt zu Siegburg* and was translated by John Kitching of The Retreat. The translation included an introduction by Samuel Tuke

In Canada the first "Provincial Asylum for the Insane" opened in 1836 in Saint John, New Brunswick (Nielson, 2000)14. Shortly after were the openings of the dominant institutions at Beauport, Québec in 1845 and Toronto, Ontario in 1850 (Moran, 1998; Reaume, 2000). Asylums in the province of Ontario spread quickly during the nineteenth century: the Rockwood Asylum for the Criminally Insane opened in Kingston in 1856, the Malden Branch Asylum in Amherstburg in 1859, the Orillia Branch Asylum in 1861, the London Asylum in 1870. Under the title of "Asylum for the Insane," more institutions opened: Hamilton in 1876, Mimico in 1890, and Brockville in 1894. This trend continued into the early twentieth century as well.

The fate of these buildings has been varied. In the majority of cities where the larger institutions were originally erected, new mental health centres have been built on or in close proximity to the original properties. The buildings themselves have, for the most part, met with one of three varied endings: (1) demolition; (2) abandonment; or (3) preservation. Using case studies from Canada that illustrate each of these outcomes, I will present the debates that have determined what was to become of nineteenth century asylums. To end with, I will present a brief comparison between the illustrated Canadian practices and those of the United States and Western Europe.

Resources for this presentation are derived primarily from archival documents for both the nineteenth and twentieth centuries. This includes government reports, architectural notebooks, asylum construction textbooks, and articles from the popular press, particularly editorials written by members of the public. Images of those asylums discussed as case studies will also be presented as a means of illustrating their initial establishment in the nineteenth century and their eventual fate.

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[14] The institutional management of the insane in Canada can be traced back as early as 1694 when the Hôpital Général de Ville Marie, Québec was founded by François Charon de la Barre (Nielson, 2000). Care continued in the province of Québec under the form of a 'système des loges' where individual rooms were provided for individuals deemed insane (Moran, 1998). Specifically designated institutions for the care of the insane, *asylums* as they were known, were only introduced into what would become Canada beginning in the 1830's.

Lurena Brackett, Laura Bridgman and Mesmerism at Perkins Institute for the Blind

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In the United States, mesmerism first received popular attention in New England in the 1830s (Carlson, 1960; Forrest, 2000). Charles Poyen (ca. 1814 to 1844) generally receives credit for introducing the technique but the people after Poyen who kept the interest in mesmerism alive are often forgotten. In this paper, I trace Lurena Brackett's role in maintaining the popularity of magnetism in Jacksonian America. Examining the facts of Lurena's life gives us some indications of why she developed what would now be called conversion disorder and why she was one of the few young American somnambulists (hypnotic subjects) who was not dismissed out of hand. The publicity about her feats as a somnambulist ended when she enrolled at the Perkins Institute for the Blind but her influence on the history of psychology would continue.

While at Perkins, Lurena Brackett became Laura Bridgman's mentor. Bridgman was the first person to learn to communicate despite being deaf and blind since infancy. She was examined by G. Stanley Hall (Ross, 1972) and would eventually teach Annie Sullivan, Helen Keller's teacher. An unpublished portion of Laura Bridgman's diary, donated to the Perkin's Institute by Bridgman's family in 1968, indicates that magnetism was a part of the treatment received at Perkins and that Laura Bridgman herself practiced magnetism. This is perhaps the earliest documented use of magnetism (hypnosis) in an American institution. Further, Lurena Brackett's discussions of magnetism and her own apparent belief that she could see at great distances without using of her eyes may have influenced Laura Bridgman's understanding of vision.

Readers of New York and New England newspapers were first introduced to her as Laura (not Lurena) Brackett in 1837. Newspaper articles written by Col. William Stone, the enthusiastic editor of the New York Commercial Newspaper, recounted how Brackett could read through lead boxes and send her spirit traveling miles away from her body. Entertaining clergy, physicians, educators, newspaper editors, and university professors, Laura was the toast of Providence, Rhode Island. When Margaret Fuller wanted to visit a somnambulist, she went to Lurena Brackett.

The fact that Lurena had a respectable Yankee background increased her credibility and saved her from some of the slurs thrown at the less-well off somnambulists. There is no indication that the respectable Yankee gentlemen observing her suspected that she had an unladylike desire for attention. Aware of the society's expectations for young women, she took pains to provide unambiguous, in fact somewhat exaggerated, evidence that she was uncomfortable being constantly observed. Because she was well provided for by a concerned father who wanted her educated for a teaching career, she could not be accused for using somnambulism to avoid a life in the mill. Unlike somnambulists of lower social status, Lurena could not be easily dismissed.

The Bridgman's already had three children when Lurena and her twin sister, Lucena, were born in 1816. And two more children were born after the twins. So when Lurena's mother died on 4-July-1822, she left seven children ranging in age from 16 to 2. Lucena, Lurena's twin, died in January1833. In her diary, Laura Bridgman mentions that Lurena had told her about her sister's death. Bridgman does not say that Lucena was Lurena's twin but refers to her as a sister who looked very much Lurena. This is the only indication that they may have been identical twins.

It was about the time of Lucena's death that Lurena developed the symptoms of the conversion disorder that eventually brought her to Dr. George Capron's attention. Thomas

Hartshorne, a magnetist from Providence, included testimonials from Rhode Island and Massachusetts magnetists in the appendix to his translation of Deleuze's (1843) Practical Instructions in Animal Magnetism and George Capron, M.D. supplied his history of Lurena's case. Lurena's symptoms included blindness which was attributed to a blow on the head that she received about 1833. From Laura Bridgman's diary we learn that Lurena was tending a fire when a boy (unnamed) pushed a flatiron off the hearth mantel. According to Laura Brackett, "A very skilled physician put a piece of silver in the hole in her [Lurena's] head to repair it. Lurena's eyes are entirely dead and have not any nerves in them in the least." (Bridgman, N.D., np.)

Conversations with Lurena that Laura recorded in her diary include accounts of Lurena's apparent ability to see with her eyes closed. Further, Laura believed that she and Lurena communicated especially well because they had been frequently magnetized together. The questions that Laura Bridgman had about visions imply that she was confusing ordinary vision with Lurena's clairvoyance (clear vision.)

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11.00-11.30 COFFEE

Main Auditorium

Contexts of Discovery in Psychology	
11.30-12:00	John Burnham, Accident Proneness: A Classic Case of Simultaneous Discovery/Construction
12.00-12.30	David Devonis, The Significance of Garages in the History of Psychology
12.30-1.30	LUNCH

Seminar Room 1

Psychology in Ireland	
11.30-12:00	Geraldine Moane, Contextualizing the Development of Psychology in Ireland
12.00-12.30	Martin Humphreys, The Foundation of Academic Psychology in the Republic of Ireland
12.30-1.30	LUNCH

Seminar Room 2

Psychology & Religion: Historical Perspectives	
11.30-12:00	Jacob Belzen, No Separation between Man and Work: the Making of a Psychologist of Religion
12.00-12.30	Robert Kugelmann, Neoscholastic Cognitive Psychology
12.30-1.30	LUNCH

Main Auditorium. Contexts of discovery in psychology

Accident Proneness (*Unfallneigung*): A Classic Case of Simultaneous Discovery/Construction in Psychology

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For generations, historians of science have been fascinated by instances when scientists have independently and simultaneously come up with the same idea. Thomas Kuhn even used such evidence to show the historical validity of his schema. Now some scholars deny that there is such a thing as "discovery," much less simultaneous discovery. A striking case to illustrate and investigate simultaneous discovery or construction of an idea is furnished by the idea of accident proneness or *Unfallneigung*.

Simultaneous discovery suggests that there was something in the intellectual community or the surrounding culture, either a general Zeitgeist or a point at which knowledge in some way had reached a state that stimulated an inevitable next step (Newton and Leibnitz). No matter that historians have raised many points of doubt and in general want to reject a mystical idea, Hegelian or otherwise, that the course of events is inevitable. The fact remains that independent, simultaneous "discovery" or, if one wishes, "construction" of a concept like accident proneness, excites special curiosity.

The concept of accident proneness, or the tendency of a particular person to have more accidents than most people, originated simultaneously and independently in Britain and Germany in the World War I period. In 1926 both the English term, "accident proneness," and the German, *Unfallneigung* (inclination to accident), were introduced into the technical and general literature—again, simultaneously.

The parallels in the two independent discoveries or constructions of accident proneness are particularly striking. The inventor in Germany, psychologist Karl Marbe, became an advocate. In Britain, another psychologist, Eric Farmer, emerged from a group as the chief advocate. It was Marbe and Farmer who gave the phenomenon its final and definitive names in major independent publications in 1926. Each, however, had already in an obscure and local publication announced the name in, again, exactly the year, 1925. Such persistent coincidence suggests that there was indeed "something in the air." That something was the acute social need to reduce the appalling costs of the human-technology interface. The factor of accident proneness furnished psychologists with a possible lever to do so.

Marbe, a father of the Würzburg school in psychology, was acquainted with psychological research that suggested variations on the statistical normal curve. He was also aware of the work of others on individual differences. Marbe himself, however, traced his thinking on the subject of accident proneness, as such, back to his school days. In school, he noticed that only a few children suffered virtually all of the accidents and similar misfortunes.

By 1916, Marbe had decided that actual accident statistics showed that a person who had an unusual number of accidents could be predicted to suffer further accidents. Or, as he finally put it, the probability of an accident was greater for the person, A, who had had an accident, than for the person, B, who had not. And Marbe meant this as conventional statistical probability. But he published his generalization only piecemeal before 1925.

British investigators first reported their findings in 1919. In an effort to maximize munitions production, the British government set up a Health of Munitions Workers Committee in 1915. In 1919, Major Greenwood and Hilda M. Woods reported individual differences in accident incidence as one of the findings of the British investigators. Greenwood was an epidemiologist (his university title was medical statistician), later a very eminent one indeed. Woods was characterized as a staff assistant. They summarized their findings by writing, "... the bulk of the accidents occur to a limited number of individuals who have a special susceptibility to accidents, and suggests that the explanation of this susceptibility is to be found in the personality of the individual." Archival materials do not reveal much more than the published facts.

It was this work that a new staff member, Farmer, a psychologist, took up and made his own in 1925-1926. And it was he, who apparently through the influence of industrial psychologist C. S. Meyers, finally united the work of the Germans and the British in 1927. Yet investigators in each tradition still tended to take a separate path, depending on the national cultures in which they worked, although often later workers saw them both as originators of a single idea.

The basic strategy that the idea suggested was that if accident prone people could be filtered out of industrial and traffic systems, there would be fewer injuries. In the end, this idea fell away, a further development that suggests that the fact that there were two origins of the idea may well have signaled the existence of a unique cultural moment that was not at all mystical.

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The Significance of Garages in the History of Psychology

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The history of technology is replete with 'garage legends', where technological advances are imagined into existence by dreamers working on a shoestring in primitive conditions (Audia & Rider, 2005). Both the automobile and airplane emerged from garages: Henry Ford's backyard shed in Detroit and the Wright Brothers' bicycle shop in Dayton are ur-garages. Walt Disney's empire started in a garage. The reigning technology of our times, the computer, also gets its start--according to legend--in garages. The birthplace of Hewlett-Packard, at 367 Addison Ave. in Palo Alto, California in 1939, is now California State Historical Landmark No. 976 (Hewlett-Packard, 2006). And Google has just repurchased the garage (along with its attached house) in Menlo Park, California, where for five months at its inception Sergey Brin and Larry Page brought that search engine to life, fueled by frequent trips to the refrigerator and hot tub in the adjacent living spaces (Bogatin, 2006).

Psychology is in many respects technology, but its apparatus, when not paper-and-pencil questionnaires, derives from equipment available in physical or biological laboratories, and the aim has been from the beginning to establish institutional psychological laboratories on a scientific model. Psychology does have some garage-legend analogues: Thorndike's chickens in William James's basement, Raymond Dodge building equipment in his living room, chimps in the Kelloggs' kitchen, Skinner's Minneapolis flour mill (Peterson, 2004), Zimbardo's cobbled-together prison. These spaces compare with technologists' garages as sites of invention for psychologists. Garages per se, however, are in short supply. .

Yet actual garages do figure in the history of psychology, specifically in a discovery context, in a special way. Three examples are offered here, involving psychologists important in the evolution of two psychological technologies in the mid-20th century, behavior modification and psychometrics. B. F. Skinner autobiographically references a garage, as recounted in Siegel's (1996) psychobiography. His grandfather, who never owned a house, was proud of his cars, graduating from a 1911 Ford to a Maxwell. He also stood alone in resisting a strike. Skinner, asked as a young man to take his grandfather's car to be washed, drove it to a garage for the task. The garageman, no friend of strikebreakers, asked Skinner whether that was his grandfather's car, and when he said it was, said he would not touch it. Both Paul E. Meehl and Timothy Leary were profoundly affected by events that occurred in garages. Meehl's father, a successful bank executive, committed embezzlement and, overcome with guilt, drove his car into the garage behind the Meehl home in Minneapolis in January 1931 and never reemerged alive. Leary's wife, preparing to take him and their children to a University of California football game on Leary's 35th birthday in October 1955, also committed suicide by car in their Berkeley garage.

These events were, for all three of these individuals, significant inflection points in their careers. For Skinner, this garage-related event was part of a chain of events that led him to develop an idiosyncratically independent approach which characterized his later work in the field. In Meehl's case, his father was his primary intellectual model as a child, and the suicide led him immediately to Menninger's The Human Mind and a quest to explain irrationality by becoming a preeminent exponent of rationality in psychology (Meehl, 1989). Leary's reaction to his wife's suicide, rather than impelling him toward psychology, forced him away. He rejected his successful career as a psychometrician, a clinician, and an academic and gave full expression to the phenomenology, experientialism, and professional rebellion nascent in his graduate school days (Greenfield, 2006; Leary, 1960). All three developed serious outsider critiques of psychology's internal logic and cultural relevance.

Clearly something other than garages is significant here, perhaps the powerlessness of psychologists to influence the decisions of their relatives, and the effect of those decisions on career trajectories and vocational direction. The point here is that these events did take place in garages, exemplifying, in contrast to the 'garage legends' of technologies outside of psychology, the dual character of the context of discovery in technological psychology. Garages, for psychologists, are not only the provisional, transient spaces for recombination of physical materials into new forms, but are also the locations of significant interpersonal experiences which are essential-for the exemplars here and for psychologists generally-to the development of its hybrid technological and social character.

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Seminar Room 1. Psychology in Ireland

Contextualizing the development of psychology in Ireland

Geraldine Moane
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This paper provides a critical analysis of the development of Psychology as a discipline and a profession in the Irish context from a feminist and postcolonial perspective. The analysis draws on in-depth interviews with a pioneer in the establishment and institutionalization of psychology in Ireland. The experiences and perspectives of this subject highlight the multiple strands and influences which existed in complex interaction over the course of the latter half of the 20th century. In particular, three influences are foregrounded, namely celticism, catholicism and colonialism, which mitigated the drive toward modernity implicated in the development of psychology. Major themes which emerged from the interviews include: the intersections and conflicts arising from the postcolonial socio-political context; the role of voluntarism and the catholic church; the intersections and boundaries

between the rational individualism of psychology and the ambiguities and complexities of celtic traditions.

More specifically, the establishment of professional psychological services in the 1950s occurred at a time of highly contested political structures and historical narratives which shaped both the institutional structures and an emerging consensus. In particular, the relationship between British psychology and psychology in Ireland required formalizations which took distinctly post-colonial forms: on the one hand Irish courses adopted British curricula and were to be accredited by the British Psychological Society, an arrangement which continued until the end of the 20th century; on the other hand, the Psychological Society of Ireland was established on an all-Ireland basis, thus implicitly adopting a "united Ireland" approach (although cross-border cooperation was the conscious intention behind this arrangement).

There were also clear tensions at the time between those wishing to adopt the agenda of US and British psychology and those seeking a more contextualized approach which might better respond to the specificities of Irish history and culture and draw on Irish or celtic language, myth, folklore and tradition to foreground the suppressed and marginalized. In tandem with cultural censorship between 1970s and 1990s the latter view found itself silenced and marginalized, with little acknowledgement that there might be specificities of context arising from Ireland's colonial history, geographical location, monocultural make-up, or the existence of partition on the island. While there were attempts, including by the subject interviewed for this research, to adapt and incorporate local practices, for the most part the field adopted established methods. Thus while the structures adopted a (partially) transnational form which reflected local concerns, the knowledge base and practices were imported with little adaptation to local contexts,

This uncritical and non-reflexive consensus achieved such hegemony that there has been a remarkable absence of critical voices in psychology as evidenced in the *Irish Journal of Psychology,* the *Irish Psychologist*, and proceedings of the Psychological Society of Ireland. There have of course been critical voices and perspectives since the foundation of the field, and many attempts to place social justice issues on the agenda, but the first symposium on critical psychology was presented at the Psychological Society of Ireland in 2005.

In the face of such entrenched interests, those interested in a politicized or critical psychology sought allies in other areas such as community development, emancipatory education, women's studies and critical health. Critical perspectives in and on psychology have been marginalized from the field and from each other, and the field of psychology itself has played a nonexistent role in public discourse over the decades since its foundation. Rather, with few exceptions, public debate in areas of central concern to psychology such as gender and sexuality, childcare and the nature and treatment of mental illness, have been informed primarily by psychiatrists, sociologists and other social commentators. As 21st century Ireland follows globalization trends and shows predictable increases on indicators of psychological distress the field is coming under increasing pressure to be more responsive to social conditions, which will hopefully open a more fruitful dialogue.

The experiences of the subject interviewed for this research reflect multiple contradictions which include being both a founder and a critic of the field, located at a crossroads which is both postmodern and traditional, desiring both a scientifically based and also systemic approach to research and practice. The findings highlight trends which are both global and local, and which will hopefully find resonance in many other contexts.

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The foundation of academic psychology in the Republic of Ireland

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The aim of the present paper is to provide an account of the foundation of Psychology in University Departments in the Republic of Ireland. Underlying the foundation of these Departments of Psychology is an analysis of the 'late arrival' of the discipline of Psychology to Ireland in comparison to other 'Western' countries. There are relatively few histories of psychology relating to Ireland. Accounts tend to be refer to historical dimensions of an area, or a history from the some 'starting point' rather than examining factors involved in the foundation of psychology in context. All that really exists are a handful of brief sketches.

This study uses source material from the archives of the four major universities in Ireland. Other source materials include textbooks, course documentation and papers written at the time of the foundation of Psychology in Ireland. Also referenced are government publications and State Papers with relevance to psychology. One final and important source of material is drawn from interviews conducted by the author with individuals involved in the establishment of academic and applied psychology in Ireland.

The current paper attempts to provide a comprehensive account of the establishment of psychology in Ireland with a particular emphasis on the foundation of Departments of Psychology in Irish Universities from the 1950s to the 1970s. The fours major universities examined are University College Dublin, Trinity College Dublin, University College Cork, and University College Galway. Each university develops a department of Psychology as well as a degree in Psychology within a few years of each other. University College Dublin beginning the process in the 1950s and University College Galway, the last to establish a department, in the 1970s.

The development of psychology in academic departments does not occur in a vacuum, other factors are involved and as consequence are important in the analysis. These factors include the foundation of the British Psychological Society (BPS) and it's Northern Ireland Branch (NIBPS) and the impact these organisations had the development of psychology in Ireland. Other aspects include the perceived need for psychological services in Ireland and the foundation of clinics and services which required the training of professionals in the field of the behavioural sciences. There are also political developments such as the input of World

Health Organisation in the 1950s and government commissions of the 1960s. These developments also reflect the issue of modernisation and its impact on development of psychology. As a consequence, it is necessary to provide an externalist analysis which incorporates a social and historical context. This is evaluated in terms modernity, industrialisation and the cultural context in Ireland. In particular the influence of the Catholic Church within this new State is fundamental to understanding the development of Psychology in Ireland.

The rise and dominance of the Catholic Church in educational, moral and legal domains in Ireland certainly has a role to play in the late development of Psychology in Ireland. There is evidence that the subject matter of psychology would have been of interest to the church for many different reasons, not least the control of the definition and interpretation of the human condition. In particular the religious impact can be seen in the links between philosophy and psychology at university level and a consequent religious dimension to the teaching of psychology.

In this sense the presence of this form of psychology may have hindered the introduction or establishment of modern psychology in Ireland. However when Ireland moves towards modernisation there is a change in perceptions of psychology in terms of academic study and practical applications. As such Psychology moves towards a discipline in its own right.

These new departments actually come from the previous philosophy departments however they are now established as separate entities creating a new discipline of psychology. As these University Departments develop there is the impetus for the foundation of a professional body in Ireland. The early movements begin in 1968 with the establishment of the Psychology Society of Ireland (PSI) in 1970. This marks the formalisation of Psychology as an independent discipline in Ireland.

At the centre of the development of Psychology in Ireland is the foundation of the academic departments. This development was initially hindered by social and cultural factors. As modernisation occurs in Ireland so to does the climate for a profession of psychology, facilitated by the foundation of academic Psychology through the University Departments.

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Documentation from State, university and private archives on the foundation of the Departments of Psychology.
Course outlines for psychology courses before and after the establishment separate university departments.
Articles and books written in Ireland or by Irish authors on the subject of psychology prior to its foundation or establishment as a separate discipline within the University system.
Government publications relating to the need for psychological services in Ireland.
Interviews with individuals involved in the establishment of psychology in Ireland or involved with the foundation and development of university departments.

Seminar Room 2. Psychology & religion: Historical perspectives

No separation between man and work. The making of a psychologist of religion

Jacob Belzen
University of Amsterdam, Nethrelands

The psychology of religion is one of the oldest subdisciplines of psychology, counting virtually all founding fathers of psychology among its practitioners (e.g. James, Hall, Wundt, Freud, Janet, etc.). Oftentimes, following Beit-Hallahmi (1974), the history of the field is depicted as a sudden rise and decline — erroneously, as has been shown in a number of more recent publications (Belzen, 2003; Wulff, 1998). Also in this paper, quite a different picture emerges, when a closer look is taken at the development in the Netherlands.

There are good reasons for making the Dutch situation a topic of research. Although the introduction of the psychology of religion to the Netherlands was quite late and the first contributions only appeared by the time of the Second World War, today the subdiscipline has reached a higher number of academic positions expressly assigned to it than in any other country in the world.

1907 was the year the psychology of religion was introduced to the Netherlands in a twofold way. It was also the year of birth of Heye Faber (1907-2001), who has become the internationally best-known Dutch psychologist of religion. Initially trained as a theologian (also in Germany, studying with celebrities as Otto, Heiler, Heidegger and Jaspers), he has been active in public life in various ways: in politics, broadcasting and church. As he had to hide from the Nazis during the Second World War, he used the time to finish his study of psychology, and even went so far as to defend a second doctoral dissertation in this subject in 1956.

Since that year teaching the psychology of religion at Leiden University, he has been a prolific author in this field and instrumental to its remarkable expansion in The Netherlands. (In the various fields he was active in, he authored more than fifty books and countless articles and book chapters.) Faber was the first Dutch author to publish a psychological treatment of religion (as distinguished from religiosity, on which most other psychologists of religion focus), an original but — as will be shown — premature work. Nevertheless, because of its translations, it has become the internationally best know Dutch contribution to the psychology of religion (Faber, 1972/1973, 1972/1976). Faber was also the author who dealt the last with what probably has been the most important discussion in the psychology of religion in the Netherlands (the so-called 'projection' debate on religion as a projection).

Psychology has not only been Faber's bread and butter in the second half of his life, however: he also deeply needed psychology to understand his own psychic development and problems. Particularly psychoanalytical theories (especially Erik Erikson's developmental psychology and Heinz Kohut's self-psychology) have been important to both Faber's endeavors in the psychology of religion and his self-exploration.

The second half of Faber's life and career provides an ideal chance to sketch the growth of the psychology in the Netherlands is the past decades. The paper proposed discusses his original contributions to the field, showing the intricate relationship between Faber's personality and work. His embracement of psychoanalytic theories reveals clearly the blind spots he retained with respect to his own person and development, as visible among others from his autobiography (Faber, 1993).

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Neoscholastic Cognitive Psychology

Robert Kugelmann
University of Dallas, USA

For the first six decades of the twentieth century, psychologists with a Neoscholastic philosophical foundation paid special attention to human cognition, empirically, theoretically, and philosophically. Although Neoscholasticism, because tied to the Catholic church, was an international movement, there were significant national differences (Carpintero, 1984; Kugelmann, 2005; Piñeda, 2005). This presentation focuses on Neoscholastic psychology in North America where, after the 1920s, behaviorism and statistical analysis became dominant forces in psychology. The most significant contribution to the study of cognition from the

Neoscholastics was Thomas Verner Moore's *Cognitive Psychology* (1939), which has received significant attention (Hampton, 1941; Humphrey, 1951; Kantor, 1940; Knapp, 1985; Rusk, 1948; Surprenant, 1997).

Moore's emphasis was on a sensation-image-meaning schema, derived from Aristotelian psychology, to be sure, but supported by empirical study. Moore (1910; 1932) and his students (Loughran, 1919; McDonough, 1919; Monaghan, 1935), and others (Gruender, 1937; Mailloux, 1942) found fundamental differences between perceptual and cognitive processes. They were much informed by the Würzburgers in their methods and in their theory (see Kusch, 1999, for the appeal of Külpe's group for Neoscholastics). Throughout this period, moreover, many Neoscholastics conducted experimental and statistical studies of intelligence, drawing on Spearman's factor analysis. They understood intelligence to be a factor sui generis, with the statistical studies supporting the Aristotelian philosophical anthropology.

It is a mistake to consider Moore's psychology, which was the template for most of these Neoscholastically-oriented studies, purely Neoscholastic. It owed much to modernist psychology, both in its methods of introspection (perfected at Würzburg, according to Moore) and factor analysis, and in its conceptualization. Especially in his analysis of consciousness did Moore draw on empiricist constructions of the psychological, for which Brennan (1940), another important Neoscholastic psychologist, criticized him. So Moore did not simply apply Aristotelian conceptions to empirical givens in a predetermined fashion. *Cognitive Psychology* asserted that the study of cognition begins with consideration of "personality," that is, the human being "as a living, conscious, thinking substance" (p. 42), a very un-Scholastic, more a Cartesian, definition. Also characteristic of Moore's approach was the inclusion of data from various forms of brain pathology and psychopathology for the analysis of perception and cognition. In this way, we see the influence of the surrounding psychological world in North America playing an active role in Moore's treatment of the subject. He had, of course, also to take notice of the Catholic theological community, as indicated by the *Nihil obstat* and *imprimi potest* that face the title page of the book. In fact, tensions between the psychological and theological contexts, and between the philosophical and applied contexts of the book, are manifest throughout it.

Moore's *Cognitive Psychology* included, after a discussion of various sensations, consideration of what he called "the synthetic sense," a version of Aristotle's *sensus communis*. For Moore, the facts of experience show that not only are the various sensations combined, but also that "past experience profoundly modifies future perceptions." This called for, he claimed, affirmation of this distinct faculty that puts sensory units "in a coordinated structural whole" (p. 238). Beyond this sensory structure, however, was meaning, which for Moore transcended sensation and imagery. The first "interpretation of experience" is perception, which yields meanings, or what in the Middle Ages were called *species intelligibiles* (p. 332). Meanings are a form of knowledge, derived from sensory experience, but not identical to either sensations or images. They are original aspects of psychological life, and they can exist independently of images—again, the Würzburg studies on imageless thought supported his findings. From there, Moore discussed judgment, reasoning, and memory. The book concluded with a discussion of the philosophical issues involved. From this brief account, one sees that Moore's cognitive psychology sought to "strengthen and complete the old by way of the new," i.e., to implement the goal of the Neoscholastic movement, as called for by Pope Leo XIII in 1879.

In addition, there were more philosophically oriented studies (Allers, 1944; Brennan, 1941b) that argued, as did Moore, for recognition of a much-maligned "faculty psychology." Brennan (1941a) argued that factor analysis implied a faculty psychology, "since the ground

. . . [covered] is concerned with the acts and powers, or the performances and abilities of man" (p. 16). There was tension within the Neoscholastic movement over the value of empirical investigation of higher mental processes. If philosophy grounds itself in ordinary experience, perhaps it is sufficient for a description of human mental activity.

The mutual influences among theoretical grounds, available "investigative practices" (Danziger, 1990), and the social contexts of these practices help to unravel the relationships between historical context, philosophical and theological presuppositions, and empirical findings. The paper discusses the investigative practices of these psychologists, raising the question about the relationship between philosophical conceptions and empirical inquiry. Changing conditions, within the Church and within psychology, marked the decline of Neoscholastic psychology in the 1960s, just at the moment when mainstream cognitive psychology came into its own. The paper, finally, addresses relationships, if any, between Neoscholastic cognitive psychology and the more recent one, with its computational emphasis.

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12.30-1.30 LUNCH

Main Auditoium

1.30-3.00 INVITED ADDRESS

The earliest days of autism

Ian Hacking
Collège de France (Emeritus)

We now think of autism in terms of the infantile autism of Leo Kanner, published in 1943, and the apparently related syndrome described by Hans Asperger in 1944. The concept has radically blossomed since 1990, and has become one of the widely known and debated of developmental disabilities, with perhaps the first really solid genetic correlate being published in February 2007. This paper is about the prehistory of these ideas.

By 1908 Eugen Bleuler had associated a type of obsessive self-absorption with schizophrenia, and he called it autistic. He himself connected it with Freud's idea of autoeroticism. He suspected a neurological and perhaps inherited origin for the phenomenon. He was also quite prepared to use the new name figuratively for intellectual and social attitudes of which he disapproved.

Autism – understood along these lines – was often mentioned and sometimes studied during the 20s and 30s. Kanner had trained in Berlin, and was a citizen of the Austro-Hungarian Empire. His medical background was not so different from that of Asperger, a Viennese, so although the latter was younger, Aryan, and separated by a war and an ocean, it is not so surprising that both men introduced similar diagnoses at the same time. Both were deeply influenced by German Heilpädagogik, and one of Asperger's teachers, August Homburger, wrote a textbook (1926) that was one of Kanner's main models in writing the first English language textbook of child psychiatry (1935). When Kanner himself introduced his diagnosis of children, childhood schizophrenia was, thanks to the Bleulerian origin of the idea of autism, in the background. Later, students of autism vigorously dissociated themselves from schizophrenia, but the paper will conclude by asking whether this may have been overly hasty.

Main Auditorium

Symposium: Self Improvement, Rehabilitation, and Human Nature	
3.30-4.00	Benoit Majerus, Healing a Non-Existent Illness: The Aporia of the Anti-Psychiatry Movement in 1960s and 1970s Europe
4.00-4.30	Michael Lumish, The Esalen Institute, the Diminished Self, and the Synthetic Ideal: How Humanistic Psychology Met Asian Religious Practice in 1960s California
4.30-5.00	Greg Eghigian (Convenor), The Rehabilitative Ideal Revisited: Optimism and Cynicism in German Therapeutic Penology in the 1960s and 1970s
5.00-5.30	Mathew Thomson (Discussant)
7.00-7.45	Cheiron Business Meeting

Seminar Room 1

Theoretical Psychology	
3.30-4.00	Graham Richards, Logos or Mythos: Psychology's Perennial Dilemma
4.00-4.30	Henrik Sinding-Larsen, Externality and Materiality as Themes in History of Human Sciences
4.30-5.00	Joel Michell, Bergson's and Bradley's Versions of the Psychometrician's Fallacy Argument
5.00-5.30	Vincent Hevern, Bruner, Mind and Narrative Self: Dramaturgical and Philosophical Influences

Seminar Room 2

Race and Heredity	
3.30-4.00	Frederic Weizmann, Critical Periods, Types and Race: The Work of Charles Stockard
4.00-4.30	Andrew Winston, Did Nazi Atrocities Inhibit Postwar Discussion of Heredity in Psychology?
4.30-5.00	William Tucker, Dwight Ingle and the Segregationist Scientists: Commonalities and Conflicts
5.00-5.30	Rod Buchanan, Eysenck and the Behavioral Biology of Personality

Main Auditorium. Symposium: Self-improvement, Rehabilitation, and Human Nature
Convenor: Greg Eghigian

The 1960s and early-1970s have generally been associated with political protest, social reform, and intellectual experimentation. Historians of the human sciences and medicine, in particular, have noted the ways in which areas such as education, social welfare, and mental health care were reevaluated in response to social movements and public criticism. In the wake of World War II and postwar reconstruction, researchers, clinicians, policymakers, and publics in Europe and the United States studied and debated the extent to which human beings could be helped and their lives bettered. For many, then, what one might call the

"long sixties" was a period of enormous promise about the human potential for change and improvement.

But how did the human sciences and public institutions in the 1960s and 1970s conceptualize and institutionalize self-improvement, therapy, and rehabilitation? This panel will examine this and related questions. Three papers will discuss different historical aspects of the ideals of self-improvement, rehabilitation, and human nature in the sixties and seventies. "Healing a Non-Existent Illness: The Aporia of the Anti-Psychiatry Movement in 1960s and 1970s Europe" explores the writings of prominent anti-psychiatry proponents and their assumptions about psychological treatment. A second paper, "The Esalen Institute, the Diminished Self, and the Synthetic Ideal: How Humanistic Psychology met Asian Religious Practice in 1960s California," is a case study of an institute in the United States that attempted to wed humanistic psychology and eastern religions in order to foster its notion of personal growth. Finally, "The Rehabilitative Ideal Revisited: Optimism and Cynicism in German Therapeutic Penology in the 1960s and 1970s," raises questions about the extent to which the clinical penology movement in West Germany really was, as has been contended, about "healing instead of punishing." A critical commentary will conclude the panel.

Healing a Non-Existent Illness: The Aporia of the Anti-Psychiatry Movement in 1960s and 1970s Europe

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The psychiatry of the second half of the 20th century is characterised by the resurgence of the belief in its power of healing. With the launch of chlorpromazine, considered the first antipsychotic drug, in the fifties, biological psychiatry began again to inflect therapeutic hope within the mental health sector. At the same time, neurological discourse definitely anchored psychiatry inside the medical (i.e. scientific) field, mental disorders being more and more integrated into the classic schemes of illness.

In the 1960s and 1970s however, this medical view was vociferously challenged, yet at the same time consolidated, by the anti-psychiatry movement. Especially in Great Britain, the Second World War contributed to the establishment – even if sometimes at the margins – of social psychiatry, especially through the concept of "therapeutic communities," where the human being was above all considered a social being. The reforms proposed by anti-psychiatry in Western Europe from the early-sixties on had their origin inside this latter conceptual frame, even if the movement radicalised these ideas by leaving the medical field in order to propose a larger, societal model.

By means of the main anti-psychiatric textbooks (e.g., Ronald D. Laing, Jan Foudraine, Klaus Dörner, Franco Basaglia, Thomas Szasz), this paper will focus on an aporia inherent to the anti-psychiatric movement: the apparent contradiction between the denial of the existence of mental illness and the development of a therapeutic device that is strongly committed to the idea that mental illness can be cured.

By emphasizing the social elements in mental illness, the critical psychiatry of the 1960s and 1970s not only dismissed completely the biological approach, but also - in it most extreme forms - denied the existence of mental illness as a medical problem. The label of "mental" was increasingly considered as being entirely socially constructed, a construction by which "society" tries to manage certain categories of people who do not respect the existing societal norms. In the context of "1968," which can be described as a moment of general

questioning of existing norms and hierarchical structures, the state of mental illness was sometimes defined as a positive experience, preferable to "normality" because it allowed the discovering of an inner self that had been alienated through a capitalist and extremely normative society. Mental illness appeared as a chance to discover a new humanity.

But at the same time, anti-psychiatry was also deeply embedded in a modernity that defined itself as the progressive, ascendant development of knowledge. And in so doing, this movement also produced techniques to heal mental illness, ones which were questioned by medical psychiatry, but that still play an important role in the wider field of mental health today: for instance, the creation of therapeutic communities, the importance of "talk" therapy, the awareness of the dangers of institutionalisation, a critical stance toward medications, the emphasis on relational aspects rather than a focus on the individual.

Anti-psychiatry is, thus, clearly a child of modern social psychiatry. Denying the existence of mental illness by proposing a new interpretation of human nature and at the same time proposing a set of procedures to rehabilitate mentally ill patients appear as two sides of a movement that tried to redefine the place of mental disorders in society.

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The Esalen Institute, the Diminished Self, and the Synthetic Ideal: How Humanistic Psychology met Asian Religious Practice in 1960s California

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In the middle of the twentieth century, social scientists such as David Riesman, C. Wright Mills, and William H. Whyte, Jr. suggested that white, middle-class Americans suffered from a diminishing sense of autonomous self-hood. Large institutions of power, they alleged, were creating a new and troubling human type, what Whyte dubbed the "Organization Man" and what Reisman identified as the "other-directed" personality. Humanistic psychologists,

meanwhile, such as Abraham Maslow, Carl Rogers, and Rollo May advanced practices, such as non-directive group therapy, designed to release these Americans from their alleged dependency, conformity, and failure of will. During the same period, Asian religious theorists of western background, such as Alan Watts, Frederic Spiegelberg, and Aldous Huxley, suggested that "mainstream" organized religious institutions furthered this unfortunate cultural trend by eroding the creativity, independence, and spiritual potential of their parishioners and adherents. In response they advocated practices such as sitting-meditation, and the various forms of yoga, designed to liberate the self and bring forth, in the words of Watts, the "practical transformation of human consciousness." To advance such practices they opened, or attended, Asian influenced alternative religious educational institutions, such as the Vedanta Society of Southern California, Gerald Heard's Trabuco College in Los Angeles, and the American Academy of Asian Studies (AAAS) in San Francisco.

In 1962, two students of the AAAS, Michael Murphy and Richard Price, united Asian religious ideas and practices with the insights and practices derived from humanistic psychology under the umbrella of what Huxley dubbed "the human potential." On the rugged cliffs of Big Sur, California, they opened the Esalen Institute and invited numerous intellectuals, artists, scientists, and religious figures to join them. These included not only Maslow, May, Rogers, Watts, and Huxley, but also Paul Tillich, B.F. Skinner, Gregory Bateson, Fritz Perls, Andrew Weil, Joseph Campbell, and many other notables in their respective fields. With the assistance of such individuals, Esalen quickly became a popular think tank, or clearing house, for such methods as encounter group therapy, gestalt therapy, primal screaming, deep body massage, yoga, tai chi, and a whole host of other methods, both sacred and secular, designed to free the individual from his or her social, psychological, or "spiritual" malaise. Advocates of the human potential thus joined the work of the humanistic psychologists with those of the alternative religious thinkers in a quest to liberate the diminished self, or the conformist American, from his or her alleged personal and spiritual stagnation. Liberation of the self and a utopian transformation of western culture were the expressed goals.

Based on archival and secondary sources, this paper makes three basic points: that popular and social scientific notions of a diminished American self were central to the development of the human potential movement of the 1960s and 1970s, that advocates of the human potential linked concepts derived from humanistic psychology with Asian mystical ideals and practices, as understood in the west, and that Maslow's ideas of "peak experience" and "self-actualization" were central to the overall project, as it was understood at Esalen. The story of the Esalen Institute, and of the human potential movement, moreover, is significant to the history of the human sciences in several ways. It demonstrates how humanists following World War II went about creating optimistic ideals of human subjectivity that countered, or sought to counter, the generally pessimistic views of the self promoted by psychologists and social scientists during the first half of the twentieth century. It illustrates the ways in which such efforts were not limited to scientific and clinical projects within formal university or medical institutions, but represent more or less *ad hoc* syntheses of psychology with religion within relatively informal institutions. And, finally, it confirms the argument made by others, such as Harry Oosterhuis, that regimes of mental hygiene in the third-quarter of the twentieth century broke away from earlier, more formal, hierarchical relationships in favor of what Oosterhuis dubs "spontaneous self-development," a trend which emphasized concerns for personal freedom, individualism, and a search for an "authentic" sense of self.

Kurt Back, Sensitivity / Beyond words

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The Rehabilitative Ideal Revisited: Optimism and Cynicism in German Therapeutic Penology in the 1960s and 1970s

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For at least the last quarter century, advocates and critics of efforts to rehabilitate prisoners – on both sides of the Atlantic – have generally agreed on two things: that therapeutic penology has been driven by progressive, humanistic ideals and that the continental European approach to incarceration has been gentler and more sensitive to prisoners than its American and

English counterparts. The history of therapeutic penology in modern Germany, however, presents a far more convoluted picture.

Proposals to reorient criminal justice around the scientifically and clinically informed resocialization of convicts were first outlined in the nineteenth century, with early experiments undertaken in the 1920s, 1930s, and 1940s. But it was only in the 1960s and early-1970s, during a period in which the criminal justice system was fundamentally reformed, that West Germany made the clinical rehabilitation of convicts a priority in the prison system. The hallmark of this new approach was the creation of therapeutic treatment centers – so-called "social-therapeutic facilities" – for repeat sexual offenders.

A closer look at how authorities, researchers, and clinicians imagined and administered rehabilitative programs for prisoners, however, shows that these programs were rarely intended to be for the general inmate population. Instead, therapeutic projects were planned for and targeted only a certain select group of prisoners deemed capable of reforming themselves. A great deal of time and energy was spent on separating out the corrigible from the incorrigible. The much-vaunted rehabilitative ideal of postwar West German penology did not spring, then, from a sanguine, quasi-utopian outlook, but rather reflected a mix of optimism and cynicism about scientific and clinical progress, the criminal nature, and the efficacy of public institutions.

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Discussant: Mathew Thomson, University of Warwick, UK

Seminar Room 1. Theoretical psychology

Logos or Mythos? Psychology's perennial dilemma

Graham Richards

Drawing on the distinction between *logos* and *mythos* as recently deployed in several works by Karen Armstrong (particularly Armstrong, 2000), it will be argued that one perennial difficulty which Psychology has faced is negotiating and reconciling the conflicting demands on it to be both a '*logos*' project (i.e. a pragmatically rational scientific enterprise) and a '*mythos*' project (i.e. a provider of broad frameworks of meaning, value and significance capable of satisfying members of modernist cultures). This issue is, historically, especially apparent in the complex relationships which have characterised the Psychology/Religion (primarily Christian) encounter and in the ways in which psychotherapy has been construed both by its practitioners and the public at large. While this observation is not entirely novel, it will be argued that couching it in these terms gets us closer to the heart of the matter than previous ways of articulating it. K.O.Apel's 1977 Habermasian distinction between three goals or 'interests' of knowledge for example identifies manipulative, communicative and 'critically emancipatory self-reflection' goals. In this scheme Psychology is clearly in tension between the first (orthodox natural scientific) goal and the last (providing routes for self-knowledge). It is also worth observing that Marxism is in a similar bind, psychologically it could be considered in the last of Apel's categories as a remedy for 'false consciousness' and provision of meaning by participating in the class-struggle, yet economically Marxism sought to demonstrate scientifically that the course of events was governed by impersonal economic laws ensuring the ultimate triumph of the proletariat and would fall into the first category. Apel's approach (I grossly oversimplify) however frames the issue as a technical epistemological question, while the *logos* vs. *mythos* schema places it in the context of more fundamental existential dilemmas. Thomas Teo's recent review of the history of critiques of Psychology (Teo, 2005) suggests that most of these have also, ultimately, centred on the same problem. The clarity of the distinction between *logos* and *mythos* will also be examined. It will be suggested that rather than two distinct categories or types of belief system or 'interests' (as in the science vs religion or technology vs ideology polarities) belief systems falling predominantly in either must contain elements of the other, the issue then becoming one of relative ratios rather than pure types. *Logos* not ultimately in the service of a *mythos* becomes pointless, while a *mythos* unratified by *logos* becomes mere fantasy. This will be historically illustrated by cases drawn both from history of Psychology and more broadly, notably the evolution vs creationism controversy. It will be argued, for instance, that the widespread rejection or dissatisfaction with both radical Watsonian behaviorism and Jungian Analytical Psychology is rooted partly in a psychological feeling or intuition that they have, in their opposite ways, aimed to exclude one of these 'dimensions' (for want of a better word), *mythos* in the behaviorism case and *logos* in the Jungian one. Since 'scientific' Psychology is a *logos* type project the behaviorist tradition has commanded more respect among professional psychologists, but, significantly, Jung is almost certainly more respected among members of the general public who tend to want Psychology to provide a satisfactory 'modernist' *mythos*. For their part, natural scientific evolutionists such as Richard Dawkins and Creationist or 'Intelligent Design' proponents are clearly at cross purposes because the former fail to grasp that the evolutionary image necessarily possesses a *mythos* dimension while the latter equally fail to understand that *mythos* considerations cannot over-ride *logos* ones in empirically-based scientific theory construction. Ironically late 19th century evolutionists such as Galton and Haeckel were, misguidedly as it turned out, quite

uninhibited in exploiting evolution as a *mythos*. Again, the resolution must rest in each party coming to understand and appraise the repressed component in its own thinking. Evolutionary thought cannot provide a satisfactory *mythos* type 'philosophy of life', while religion cannot, as a *mythos* of the highest order, be converted into science, as Intelligent Design's proponents seek to do. Yet each must acknowledge the role of the other in determining the meaning of what they are about. Finally, one must acknowledge that this very polarity is itself a product of the psyche or mind which Psychology takes it as its task to explore. How is it then that this fundamental division emerges? Why is it that individuals differ so widely in terms of their identifications, affiliations or commitments to one over the other, often to the extent of casting that other as a mistake or illusion? In raising this question we are, in a fashion, returning to the popular mid-20th century *topos* of 'modern man's' lack of 'spiritual' direction following the Nietzschian death of God. But with the added twist that in addition to the Jungian 'Modern Man in Search of Soul' problem we also face one of 'Modern Souls in Search of a Science'.

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Externality and materiality as themes in the history of human sciences

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Tim Ingold's book (2000) "The perception of the environment" has already become a source of inspiration and debate within several disciplines, notably anthropology and archaeology as reflected in Tilley et al. (2006). It is no coincidence that two of the major works of authors that Ingold mentions as his primary sources of inspiration contain the word ecology in their title: Gregory Bateson (1973) "Steps to an ecology of mind" and James J. Gibson (1979) "The ecological approach to visual perception".

The paper will discuss themes within this "ecological perspective" as treated in the above mentions works: 1) the shortcomings of the traditional (Cartesian) division between the materiality of physical objects (body, technology, environment) and the immateriality of mental objects (ideas, meaning, mind), and 2) the related shortcomings of viewing the skin of the body as a membrane between an interior self and an exterior material world.

The starting point in the ecological perspective is not the individual *in* an abstract or generalised environment but the individual *plus* his or her concrete environment as a co-evolving entity. To obtain this new perspective it is necessary to take materiality more seriously than has been done in the human sciences since the 'linguistic turn'.

Several of the contributions in Tilley et al. (2006) "Handbook of material culture" emphasize that there has been a recent shift in focus from a hermeneutic and poststructuralist view on the interpretability of material objects to a view where materiality itself plays an

independent and important part in the meaning and use of objects. Ian Hacking, discussing some material infrastructure notes *"...you may want to call these structures 'social' because their meanings are what matter to us, 'but they are material, and in their sheer materiality make substantial differences to people'"* (Olsen 2006:96 citing Hacking 2001:10).

The importance of 'sheer materiality' is equally a theme in another contribution: *"As a parent it is possible to prepare a child to ride a bicycle by talking about balance, speed and when to turn the handlebars. But the real teacher is the bike itself, which will tune muscles, set up the faculty of balance and provide the social expectations of what other bike riders will do. Textiles, hand axes and fish stews are other vital teachers in human history"* (Godsen 2006:425)

Ingold in his chapter 'On weaving a basket' discusses issues of materiality when showing that the production of basketry "*involves the bending and interweaving of **fibres that may exert a considerable resistance of their own**. [...] The actual, concrete form of the basket [...] does not issue from the [pre-existent] idea [of the form she wishes to create]. It rather comes into being through the gradual unfolding of that field of forces set up through the active and sensuous engagement of practitioner and material. This field is neither internal to the material nor internal to the practitioner (hence external to the material); rather, it **cuts across the emergent interface between them**."* (Ingold 2000:342, my emphasis)

This preoccupation with what cuts across the interface between internal and external is central to the ecological perspective.

The ecological (co-evolutionary) perspective on our bodily involvement with the use and production of material artefacts may also be applied to our mental involvement with signs and descriptions. I will show how musical notation as a semiotic system, much like fibres in basket-weaving, *"exert a considerable resistance of its own"* making the production and use of music an equally *"active and sensuous engagement of practitioner and material."* (Sinding-Larsen 1992). I propose that it is precisely this ability to exert resistance that makes something appear as external and thereby "real", and that this is equally true for both material and mental objects. Materiality may in this perspective be seen as a special (and extreme) case of externality.

Materiality has throughout the history of science been a topic of changing attitudes. The material has been associated with 1) *liberation* (through the enlightenment's study of the material independent of religion and dogma), 2) *oppression* (through the hermeneutic and post-structuralistic critique of materialism), and 3) *liberation* (through the new ecological approach purportedly liberated from both technological (material) determinism and social constructivism).

Important roots to the ecological approach may be found in the new formal (mathematical) theories of information transfer (Shannon and Weaver) and information processing (computing, cybernetics (Turing and Wiener)). These paved the way for a kind of "linguistic turn" in biology (from DNA-genetics to eco-systems) and engineering (computers). In this new "materialist" linguistic turn the code is neither in the mind nor in the 'readers' context but in materiality itself. I will propose to see the ecological approach in human science as an attempt to integrate the new materialist and old idealist "linguistic turns" with a concern with embodied and pre-linguistic experience.

Some problems with this ambition will be exemplified with reference to the book Changeux & Ricoeur (1998).

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Bergson's and Bradley's Versions of the Psychometricians' Fallacy Argument and the Logic of Quantity

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The distinction between quality and quantity is topical, but many are only dimly aware that it has a history in psychology. Controversy peaked a century ago and one of the issues was the so-called 'quantity objection' to Fechner's psychophysics. For example, von Kries (1882) argued that intensities of sensations cannot be measured because while one intensity might seem greater than another, it is never twice, thrice or any other number of times greater. Despite this objection, a consensus was crystallizing (Titchener, 1905), viz., that differences between intensities are quantitative and these are what Fechner's methods measure. The glue for this consensus was the inference I call the psychometricians' fallacy (Michell, 2006), which has it that whenever degrees of an attribute are ordered, the attribute must be quantitative (i.e., possess additive structure). Were it so, psychological attributes (like intensities of sensation or levels of intellectual ability), which we only ever experience directly as ordinal, would possess hidden quantitative structure and in principle be measurable on interval or ratio scales.

That this inference is invalid follows from a result published by Otto Hölder (1901), but still largely undigested by psychometricians. Then as now, their tendency is simply to presume the psychometricians' fallacy valid (hence the name). However, a century ago, some caught-up in the controversy attempted to justify it, in particular, the French philosopher, Henri Bergson (1913), and the English metaphysician, F. H. Bradley (1895).

Bergson and Bradley joined the nineteenth century reaction against scientism (Burrow, 2000) and were critics of the new psychology, Bergson savaging psychophysics and Bradley, associationism. Seemingly, their attempted justifications of an inference used to defend psychophysical measurement is anomalous. However, in each case, the argument proposed unfolds from other features of their respective philosophies.

For Bergson, Fechner's error was projecting spatial concepts, which are intrinsically quantitative, onto immediate experience. Fechner thought that sensation intensities are experienced as ordered and via the psychometricians' fallacy inferred that they are quantitative. Bergson elaborated the fallacy by arguing that were they quantitative, greater intensities would necessarily contain lesser ones, just as greater lengths contain lesser and then objected that because relations of containment do not hold between sensations, these must not be ordered. Hence, they must be heterogeneously qualitative. This conclusion followed from his doctrine that immediate experience lacks structural features like quantity, which, he held, derive from our pragmatic dealings with things.

Bradley's version of the argument follows from his doctrine that every sensation is composed of ultimate units of experience. He reasoned that whenever one sensation is experienced as more intense than another, the greater is composed of the lesser plus something else constituted of the same units. Thus, order entails quantity. He attempted to overcome the objection that sensations are not always experienced as composites of units by claiming that only in the case of spatial and temporal sensations are units 'visible side by side.' In other cases, he said, neither the units nor the manner of composition is open to analysis. This follows from his thesis that in experience the Absolute is only ever incompletely grasped and is always distorted by thought and language.

Bergson and Bradley criticised the science of psychology from a metaphysical point of view, so while acceptance of the inference from order to quantity underpinned psychophysics and guided the later consensus, its presence in their writings suggests that it has a history independent of the traditions of quantitative science. This is confirmed by anticipations of this inference in Aristotle and medieval philosophy.

Furthermore, the arguments of Bergson and Bradley are of contemporary interest: they expose fundamental issues grounding the distinction between quantity and order and they suggest some of the ways in which attributes may be ordered without being quantitative. This is relevant to the debate about qualitative versus quantitative methods in psychology because it reaffirms that the central logical issue is not one of conflict between competing philosophies (e.g., positivism vs. postmodernism), but is the issue of the structure of psychological phenomena.

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Bruner, Mind, and the Narrative Self: Dramaturgical and Philosophical Influences

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Jerome Seymour Bruner began his graduate studies in the Psychology Department of Harvard University almost seventy years ago. He arrived in Cambridge, Massachusetts in 1938 to study with that famously contentious faculty amidst a talented cadre of fellow students. Three years later after completing his thesis (Bruner, 1941), he moved into work on behalf of a government on the verge of war in the fields of public opinion research and various aspects of war information monitoring (Bruner, 1982). After this wartime service, Bruner returned to Harvard in 1945 as a faculty member and remained there until leaving for Oxford in 1972. In 1960 he founded (with George Miller) the influential Center for Cognitive Studies, a venue which closed in 1972. Bruner's turn as the first Watts Professor of Psychology at Oxford University lasted until 1980 when he returned to the United States. He held the George Herbert Mead University professorship at the New School for Social Research from 1980 to 1988. Finally, in 1991, he assumed a Research and, then, University professorship at New York University where he also serves actively as an adjunct professor in the NYU School of Law. His postdoctoral career of teaching, research, and writing has reached the midpoint of its seventh decade.

Despite Bruner's central place in psychology in the second half of the 20th century, there are relatively few published resources approaching his work from the perspective of either biography or intellectual history. No book-length biographical study of Bruner as psychologist or intellectual has yet appeared. Bruner's (1983) own autobiography remains the most comprehensive review of his own life. Standard reference works merely offer brief overviews of his life and contributions (see, for example, Jerome S(eymour) Bruner, 2003; Sheehy, Chapman, & Conroy, 1997). A special issue of the journal, Human Development, in 1990 did offer three analytic essays detailing Bruner's intellectual journey in somewhat condensed or summary fashion (Gopnik, 1990; Greenfield, 1990; Olson, 1990). Bruner's own contribution to that issue (Bruner, 1990b) points to the development of his thought in the years after he left Oxford as he has grappled with the role of culture and narrative as formative of mind. The Emory University anthropologist, Bradd Shore, published an extended but edited conversation with Bruner (Shore, 1997), but a promised volume containing the full set of his interviews with Bruner has never appeared. Finally, two Italian researchers, Ornaghi & Groppo (1998), offer a bibliographical overview of Bruner's published work in a journal accessible to a limited scholarly audience.

The relative absence of critical biographical and critical analysis of Bruner's contributions is compounded by the challenge of a career of such length and scope. In the late 1940s, his contributions to the study of perceptual processing and the "New Look" in psychology (Bruner, 1992) anticipated broadly influential advances in understanding both constructivist influences on cognition and the paths of infant cognitive and language development during the late 1950s through the mid-1970s. By the early 1980s, however, Bruner turned far more explicitly to the role of cultural interaction as it affects the development of mind, and, by the late 1980s, he proposed that the "narrative mode of thought" and, particularly, autobiography were central to the construction of identity and the self (Bruner, 1990a,b; Bruner & Weisser, 1991a). Nonetheless, his move toward cultural psychology and narrative over this past quarter-century has been the subject of relatively little review insofar as it relates broadly to either Bruner's academic past or intellectual development.

In this paper, I will examine two important influences upon Bruner's thinking from outside the social sciences that contributed, in later years, to his embrace of narrative as a fundamental function of mind. The first is impact of the literary theorist, Kenneth D. Burke (1897-1993), whom Bruner first learned about from his roommate, Irvin Dunston, as a senior at Duke University during 1937-1938 (Bruner, personal communication). Burke's theory of dramatism has deeply affected the direction of Bruner's thinking over the last twenty-five years. Burke's Grammar of Motives was widely rejected by contemporary critics when it first appeared in 1945. However, Bruner has found Burke's ideas, particularly his motivational "pentad" (actor/agent, act/action, setting, instrumentality or agency, and purpose) as a particularly apt bridge between literary theory and social science, particularly psychology. Burke's own intellectual movement from a critical aestheticism in the 1920s to profound engagement with diverse social and cultural forces in the 1930s and 1940s (Burke, 1935, 1941, 1945) mirrors or prefigures stances by Bruner (and others) about human persons as intentional agents and their behavior as symbolic actors in complex interactive settings. The second figure for review is Nelson Goodman (1906-1998), whose thought was first introduced to Bruner by Morton White, a professor of philosophy at Harvard in the 1950s and 1960s while Goodman was teaching at the University of Pennsylvania (Bruner, personal communication). When Goodman arrived at Harvard in 1968 to teach philosophy, they began a personal and intellectual friendship which endured until Goodman's death (Bruner, personal communication; Goodman, 1991). Goodman also contributed philosophical critique to discussions at the Center for Cognitive Studies as a Fellow in its closing years. Beginning with The Structure of Appearance (1951) an elaborated less formally in Ways of Worldmaking (1978), Goodman's constructivist epistemology argues for a multiplicity of symbol systems by which individuals construe the world. As early as his 1951 volume, Goodman proposed that "physicalist and phenomenalist systems are distinct, valid constructions of independent interest and importance. Neither is parasitic on the other" (Elgin, 1998, p. 137). This epistemological stance finds itself strongly echoed in Bruner's oft-cited dualism of paradigmatic vs. narrative modes of thought, each of which, Bruner claims, " provid[e] distinctive ways of ordering experience, of constructing reality. The two (though complementary) are irreducible to one another" (Bruner, 1986, p. 11; see, also Bruner, 1991b).

My presentation will draw upon Bruner's published works, email communication between us on this topic, and a direct personal interview with him exploring his memories and judgments about these theorists. I will detail (1) the sequence of Bruner's encounters with the thought of each figure, (2) a synopsis of the their own thinking most relevant to Bruner's contributions, and (3) a summary of how Bruner has deployed their understanding in his more recent writings.

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Seminar Room 2. Race and Heredity

Critical periods, types and race: The work of Charles R. Stockard

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The essential idea of critical or sensitive periods in development is that the effects of an environmental influence depend on the developmental stage at which it is encountered by the organism. It was the embryologist Charles Stockard (1921) who first formalized and provided systematic evidence for the idea. It has subsequently been applied to many areas of development, from brain and sensory growth, to the formation of affectional bonds, to language acquisition. Stockard's work itself, dating from 1906, focussed on demonstrating the vulnerability of developing organisms to a variety of deleterious influences. His later studies on the effects of prenatal exposure to alcohol (e.g. Stockard, 1918) in guinea pigs, revealed various kinds of developmental defects which were passed on to descendents; Stockard's initial interpretation was that alcohol led to a general weakening of the germ-plasm.

Stockard and his work became one of the major sources of scientific support for Prohibition in the US. Much to the prohibitionists' chagrin however (Jones, 1963; Pauly, 1996), Stockard later reported that after several generations, guinea-pigs from alcohol-treated stock were fitter than normal control animals. He now began to emphasize the role of teratogenic agents like alcohol in eliminating the unfit. He did not hesitate to apply the Social Darwinist implications of his findings to humans, claiming that "...some such elimination of unfit individuals had benefited the dominant races of Europe, since all of the dominant races have a definitely alcoholic history ..." (Stockard, 1931, p. 317).

The emphasis on individual selection marked a change in his work (Stockard, 1941, pp. 5-6). A long-time eugenicist, from the early 1920's on, Stockard became concerned with the genetic and endocrinological bases of purported human and racial types Since he saw breeds of dogs as comparable to human races, in the mid-1920s, with Rockefeller Foundation support, Stockard (1931; 1941) began to carry out cross-breeding experiments on dogs which he also applied to humans. In a book published in 1931, he argued that there were two human types, a long-headed, thin or "linear" type and a broad-headed stocky, or "lateral" one, which differed in temperament as well as body type. This is quite reminiscent of Kretschmer's or Sheldon's types, but he does not refer to their work although he and Sheldon were both associated with the Columbia Constitution Clinic which emphasized constitutional factors in disease.

In a later work, left unfinished when he died and published posthumously, Stockard's (1941) claims are much bolder. In it argues that the artificial selection of dogs destroyed the balance of the prototypical ancestors (most closely represented by the German Shepard) and led to deformations comparable to human endocrinological disorders. Although he earlier stated that the various human types occurred in all races, he now emphasized that unregulated hybridization among various human races led to disharmonious combinations. He also argued that indiscriminate mating with former slave races caused the fall of past civilizations. The increased emphasis on heredity and the growing intrusion of his racial beliefs were undoubtedly major factors in Stockard's later work; however, Stockard came from embryology and always had a sense of the organism as a unified, developing system. As he said, (Stockard, 1941, p. 8), he was interested in investigating "constitution in a comprehensive manner", and not the "genetics of isolated characteristics". Stockard saw the endocrine system as the major coordinating and regulating system of the body. He believed that constitution emerged in the course of development, and suggested that the endocrine system played a major role in governing constitution and its expression, (i.e., type). He repeatedly raised the issue as to whether genetic influences manifest themselves through their effect on the endocrines (i.e., as part of an integrated developing system) or directly and locally.

Stockard's view of types incorporated a systemic and developmental dimension, and in part, his later work may be viewed as his attempt as an embryologist to deal with the challenge of genetics. Nonetheless, although Stockard continued to believe that external factors played a role in development, they were clearly subordinate to constitutional factors. The idea of human "types" is much older than the idea of the gene or the discipline of genetics, although some, like Stockard, have attempted to integrate them. Stockard's work illustrates the survival of these older ideas of heredity, even within the context of an epigenetic, interactive view of development.

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Did Nazi atrocities inhibit postwar discussion of heredity in psychology?

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A very commonly held assumption is that immediately after World War II, revulsion engendered by Nazi war crimes and Nazi racial theories strongly inhibited discussion of heredity in academic psychology. The primary source of this assumption for contemporary historians is Carl Degler's (1991) widely read In Search of Human Nature. Degler provided a careful analysis of changes in hereditarian discourse, but he placed great stress on the inhibitory effect of the Nazis. Degler's work has been used to bolster this idea in numerous sources, ranging from textbooks to popular discussions (e.g. Pinker, 2004). In this paper, I will argue that this assumption is highly oversimplified, and possibly incorrect. As the analyses of Cravens (1978), Harwood (1979), and others have shown, the vicissitudes of hereditarian discourse bear a complex relationship to changing social conditions and broader cultural projects. I will attempt to provide a more nuanced view of the relationship between psychological discourse on heredity and National Socialism in the decade following World War II.

Many discussions of this issue have understandably conflated changes in the treatment of racial differences with the treatment of heredity. I will show how psychologists negotiated a discursive disentanglement of these issues, partly with the help of the even-handed Robert S. Woodworth (1941). That is, Woodworth constructed a version for psychologists that maintained the importance of heredity for individual but not group differences.

An additional source of confusion is the conflation of the decline of "mainline" eugenics with discomfort regarding heredity. As one of the sources for his general contention, Degler cited a famous statement by eugenicist (and later magazine marriage counselor) Paul Popenoe that "Hitlerism" killed eugenics. This view was a convenient one for eugenicists: by implication, their program did not collapse from its own deficiencies but from unfortunate, and in their view unjustified, "guilt by association." But historians have shown clearly how the decline of eugenics began in the late 1920s. There is no doubt that Third Reich eugenic policies and their support by some American eugenicists had an important impact, but the rejection of the mainline eugenics program cannot be considered a rejection of the general importance of genetics for psychology.

Discussions of race and racial differences were undoubtedly altered in reaction to Nazi racial theory, and this process began well before the beginning of the war (Barkan, 1992; Richards, 1997; Samelson, 1978; Winston Butzer, & Ferris, 2004). The shift was primarily an abandonment of intra-European comparisons, with Black - White comparisons left as an open question. There was considerable variation by discipline (e.g., see Littlefeld, Lieberman & Reynolds, 1982; Paul, 1985), with anthropology somewhat more univocal than psychology. These issues were particularly important to Franz Boas and his students, who addressed Nazi racial theories directly (Hyatt, 1990). For example, Ruth Benedict's Race Science and Politics (1940) contained a full discussion of the origins of Nazi theory, as did Montagu's (1942) Man's most dangerous myth: The fallacy of race. The 1938 condemnation of Nazi racism issued by the Executive Council of SPSSI constituted one of the few official statements on Nazi race theory, and perhaps the only official statement by a group of psychologists.

Reaction to Nazi horrors had important effects on psychologists' discourse on race and racism, and obviously served as an immediate major impetus for research on prejudice and authoritarianism, as well as later studies of obedience. There were very few explicit discussions of Nazi crimes in psychology journals, although in 1943, the Journal of Abnormal and Social Psychology presented personal accounts of Nazi concentration camp experiences by Curt Bondy and Bruno Bettelheim. I have been unable to find any pre – 1960s discussions by psychologists which linked the study of heredity with Nazi atrocities or racial theories. Despite the prominence of learning theory, discussions of heredity were not banished from psychology after World War II, and the importance of heredity was acknowledged in journals, monographs, and textbooks. I will show how acceptance of heredity as a cause of human differences, viewed in an interactionist framework, was particularly evident in discussions of intelligence, personality, and child development. Leona Tyler's (1947) highly successful Psychology of Human Differences provides a clear example of a prominent place for heredity in psychological theorizing and research. Growing interest in the personality theories of Hans Eysenck and Raymond B. Cattell, who both emphasized the genetics of individual differences, further indicates the place of heredity in postwar psychology. Finally, I will discuss the postwar acceptance of Konrad Lorenz and his work, despite knowledge among some psychologists and biologists of his Nazi affiliations and of the use of his writings by the regime (Deichmann, 1996). The participation of Lorenz in the 1953-54 World Health Organization Study Group on the Psychobiological Development of the Child, alongside Jean Piaget, Margaret Mead, and others suggests that conceptions of the "banishment" or "suppression" of heredity in postwar psychology must be seriously reconsidered.

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Dwight Ingle and the Segregationist Scientists: Communalities and Conflicts

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The controversy over racial differences was particularly acrimonious during the civil rights movement, when a number of scientists made inflammatory pronouncements that were used by defenders of segregation in an attempt to keep blacks in subordinate status in the United States. Historians have paid considerable attention to most of the important scientists involved in this debate, including such well known names as Henry Garrett, Arthur Jensen, and William Shockley. One person who was quite significant at the time but has been largely neglected by scholars is Dwight Ingle.

Though his doctorate was in psychology, Ingle was more interested in endocrinology and, after a dozen years as a research scientist in the private sector, went on to accept a professorship of physiology in the University of Chicago's Ben May Institute, eventually becoming chair of the department. One of the most prominent scientists of his time, Ingle was the recipient of numerous honors, including election to the National Academy of Sciences and Fellow of the American Academy of Arts and Sciences. Concerned with the issues of joint interest to science and the humanities, in 1957 he founded Perspectives in Biology and Medicine as a forum for interaction between the two cultures and edited the journal for most of the next two decades.

Near the end of this distinguished career, Ingle published his autobiography, in which he detailed his personal experiences living in an integrated Chicago neighborhood. Disenchanted by the difficulties he encountered--drug trade, unsafe streets, litter, etc.--he complained about what he called the "forced integration" of neighborhoods, characterized by the "random mixing" of blacks and whites without regard to "behavioral standards." Around the same time, in his own journal and other scientific outlets, he called for research on racial differences in ability as a response to two social trends he claimed were becoming widespread: the tendency to place persons in positions solely on the basis of race, and the belief that the solution to racial conflict was interbreeding. Ingle also came to the conclusion that efforts were necessary to prevent births among those people unqualified to be parents, making the case for such measures in a book titled Who Should Have Children?. Although Ingle himself was not part of the clique of scientists who had opposed both the Brown decision and the subsequent pieces of civil rights legislation, his opinions quickly led to a relationship with this segregationist group that had benefits for each side. In the name of free and robust discussion, Ingle opened the pages of Perspectives to the scientific opponents of integration, thus providing a respected venue for viewpoints that he wished to see publicized without himself having to be identified with them. Believing that they had found a sympathizer, the segregationists made a conscious effort to recruit such a prominent scientist to their cause. However, despite the service Ingle rendered to their desire to keep attention focused on the putative biological shortcomings of blacks, he balked at placing his authority behind certain of their conclusions that, as a scientist, he judged unsupportable. Archival correspondence indicates that this difference of opinion caused a disagreement within the segregationist group concerning how best to respond to Ingle, some still desiring to cultivate him, others dismissing him as gutless.

Eventually the segregationists lost interest in Ingle, turning instead to Shockley and Jensen, two men who also must have harbored doubts about some of the more extreme statements but were less inclined to make them public. In any event, the sour end to the relationship between the physiologist and the segregationists was not repeated with these new sources of scientific authority.

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Hans Eysenck and the Behavioural Biology of Personality

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As an academic psychologist in postwar Britain, Hans Jürgen Eysenck became famous for the audacity of his theorising and the forthright, often controversial views he expressed. In the mid-1950s, the ambitious Eysenck set about laying down what he thought of as the "causal" basis for his newly-minted personality dimensions. Inspired by the eugenic concerns of Galton and German physical morphology, he attempted to anchor neuroticism, introversion-extraversion and psychoticism in corporeal biology. These bio-dimensions could then be correlated with characteristically different patterns of conditioning, performance and problem solving. Eysenck's learning paradigm was classically behaviourist, and never far from its physiological underpinnings. While Hull was taken on board and then discarded, Tolman was brushed aside, and Skinner barely considered at all. Latter-day cognitive approaches were also largely ignored. But Eysenck's affinity with Russian work survived, especially with that of a Nobel laureate who considered himself primarily a physiologist. Eysenck made the misunderstood Ivan Pavlov his intellectual muse - a distant but compelling guide linking material body with immaterial mind.

Cued by Pavlov, Eysenck took the radical step of situating individual differences in behaviour within the functional economy of the nervous system. Personality differences were directly related to differences in the brain that were largely determined by hereditary. Utilising what now appear to be the limited techniques of functional neurology of the 1950s, Eysenck searched for constitutive differences in the study of animal breeding and behaviour. In his landmark 1957 book The Dynamics of Anxiety and Hysteria, neuroticism was linked to an anxiety-related notion of drive, while his famous "typological postulate" mapped introversion-extraversion onto a simplified version of excitation and inhibition. Prompted by criticism and further research, these ideas were revised a decade later in The Biological Basis of Personality. Eysenck's dimensional grid both reflected and predicted differential learning

patterns which, in turn, were the result of differential hereditary propensities. Biologising personality also enabled Eysenck to claim that his dimensions were culturally universal, with a continuum from humans to animals.

Eysenck's reach and ambition set him apart from a wash of American personality trait psychologists. By demonstrating that psychometric descriptions could be experimentally analysed in relation to the control structures of the brain, he became a boundary-crosser - a "biological behaviourist," "personality learning theorist," and then some. Each of these subfields had their specialists, most of whom had decidedly non-neutral attitudes to encroachment. Yet Eysenck treated traditional divisions and intellectual niceties as an opportunity for transgression, the better to make bolder, more novel claims. While many of his fellow personality psychologists were intrigued, few could contemplate such an ambitious program. It was not just an intellectual bridge too far; it ran up against formidable financial and institutional constraints. So how did Eysenck do it? How did he generate and manage the kind of hybrid resources, the equipment and the personnel required? Eysenck had a knack for making a little go a long way. The relatively small number of researchers that attempted to follow in his footsteps claimed Eysenck's approach had the potential to unite the discipline as never before, to reconcile Cronbach's "two disciplines" of experimental and correlational/factorial psychology. Eysenck's final biological model remained the blue-print for future efforts. His precedent dominated research in the neurophysiology of temperament differences in the decades to follow. Nevertheless, Eysenck's integrative efforts encountered significant resistance from those he tried to unite and left traditional boundaries largely still in place. In part this could be represented as entrenched resistance to colonisation, a rebuff to an attempted takeover. But it was not just that.

Eysenck's measurement tools and experimental methods called to mind precision and reliability, stripping away untrustworthy particular and personal aspects. Yet Eysenck's work inspired anything but trust amongst many of his peers. His high-minded style provoked some notable critiques from his co-workers. Likewise, beautiful numbers failed to convince many farther afield, even those willing to give his neurobiological conjectures a sympathetic hearing. Eysenck's version of scientific practice exchanged carefully constructed consensus for partisan output. He was all detachment, all overt formalism, all public performance. Eysenck did not, and perhaps could not, do the behind-the-scenes work to allay the doubts he evoked. Out of this emerged a spectacular dissonance. Eysenck's glittering notoriety was accompanied by quite toxic levels of private cynicism amongst his peers – highlighting the importance of what is left out of public scientific discourse for making knowledge claims and breaking reputations.

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7.00-7.45 Cheiron Business Meeting (Main Auditorium)

THURSDAY 28TH JUNE

Main Auditorium

Symposium: Literature and the Social Sciences	
9.00-9.30	Daniel Becquemont, George Eliot Between Positivism and Evolution
9.30-10.00	Jean-Philippe Bouilloud, Intellectual Autobiography in the Social Sciences, Between Literary and Scientific Production: Relationships, Tensions, Typology
10.00-10.30	Nathalie Richard, Taine, Stendhal and the Jacobin: Psychology, History and Literature at the End of the 19th century
10.30-11.00	Coffee

Seminar Room 1

History of Anthropology	
9.00-9.30	John S. Gilkeson, Eric R. Wolf at Columbia, 1946-1951
9.00-10.00	Gerald Sullivan, Of Margaret Mead and Biology, 1933-1939
10.00-10.30	------
10.30-11.00	Coffee

Seminar Room 2

Psychophysics in Cultural Contexts	
9.00-9.30	Alex Hui, Ways of Hearing, Ways of Listening: Musical Expertise in Sound Sensation Studies of Helmholtz, Mach, and Stumpf
9.30-10.00	Susan Lanzoni, Empathy in the Lab: Kinaesthesis and Gender in Titchener's Cornell Experiments
10.00-10.30	Arthur Arruda Leal Ferreira, Recovering the Losers: the Place of Panpsychism in Fechner's Work
10.30-11.00	Coffee

Main Auditororium. Symposium: Literature and the Social Sciences
Convenor: Daniel Becquemont, Université de Lille III, France

If we try to examine the relationship between literature and the sciences of man, we must consider after Wolf Lepenies the border line which is elaborated and constantly re-shaped from the 18th century on to the present between a literary prospect and a scientific prospect. Sometimes novelists and even poets have resorted to the categories of social scientists or to one particular social theory in order to structure their fictions, sometimes social scientists have not given up any hope of being considered as literary writers. Literary writers may take as their subject-matter social sciences, or one human science more particularly; social scientists themselves may elaborate their own theories about the distinction between literature and social sciences, or resort to "literary" means such as autobiography. Social scientists and novelists may join their writings in reviews and newspapers, political, economic, social or literary.

Yet the border line between the two spheres becomes more clear cut in time : literature appeals to the feelings and sensibility of the readers, and resorts to style in order to move him. Social sciences resorts to reason, and may appear as detached from human

contingencies and as a 'dismal' science, as Carlyle said about political economy. It can also be considered as a 'third' science, a bridge between physical sciences and literature.

A literary approach of the social sciences can be used in order to stress what they are unable to account for, what is left in the shadow, its hidden, libidinal or political dimensions. This workshop intends to present some examples which may contribute to clarify some aspects of the relationship between the sciences of man and literature.

George Eliot between positivism and evolution

Daniel Becquemont
Université de Lille III, Société Française d'Histoire des Sciences de l'Homme

George Eliot, in *Middlemarch,* expressed through the means of narration, theories about nature and society which were deeply influenced by Comte's positivism, and more particularly by her companion G. H. Lewes's special brand of positivism, which against Comte's theory, laid great stress on the importance of psychology. Though less explicit, Spencer's theories, founded on the idea of a slow passage from homogeneity to heterogeneity according to Karl Ernst Von Baer's embryology, influenced her views on development, and more particularly on the psychological development of her characters. The societies in her novels are thus culturally organic, subject to internal laws of causation – Comte's three stages of development and Spence's laws of evolution - and evolving change.

The positivist concept of an organic society subject to laws of causation in George Eliot,s writings drove many critics to speak of a strict determinism. But, according to Lewes's stress on the importance of psychology, the fight - or sometimes the reconciliation - between individual needs and deterministic social laws, enhances the idea of a margin of individual freedom in the forward struggle against determining conditions.

Intellectual autobiography in social sciences, between literary and scientific production : relationship, tensions, typology.

Jean-Philippe Bouilloud
Ecole Supérieure de Commerce-EAP Paris, Société Française d'Histoire des Sciences de l'Homme.

Intellectual autobiographies have taken an ambivalent place in social sciences; they do not belong entirely to the scientist's works, yet they are intimately related to them, since very often they can be used as a tool to explain and justify their author's theories and show the origin of the work proper. We shall examine, through an analysis of the 'topoi' of intellectual biographies and the works of G. Devereux and C. Geertz, the complex links, very often hidden, between scientific production and autobiography. It will appear that intellectual autobiographies are often part and parcel of the scientific work, whose synthesis they establishes under literary and stylistic forms which are different from scientific texts. It will enable us to illustrate in a new way Dilthey's words on autobiography being the model of research in social sciences.

Hippolyte)

~~H~~Taine, Stendhal and the Jacobin : psychology, history and literature towards the end of the XIXth century

Nathalie Richard
Université Paris I, Société Française d'Histoire des Sciences de l'Homme

In his texts focussed on a reflection on history, Taine openly links the latter with psychology. Now the most important of such texts - even if they have been later published separately - are introductions to works whose subject is literature : *L,Histoire de la littérature anglaise* (1864) and the second edition of the *Essais de critique et d'histoire.* These volumes, which deal mainly with history and literary criticism, are a first step towards a reflection on art which Taine develops under a deeper and more articulate form in his *Philosophie de l'art* (1865).

When he planned the writing of his only work considered today as belonging to history, *Les Origines de la France contemporaine*, from 1871 onwards, Taine had already worked out a complete reflection on what history should be and on the nature of literature. He had also just completed his work on psychology, *De l'intelligence*, in which he advocated a new Œexperimental, psychology, freed from metaphysical abstractions and based on a close and controlled observation of facts.

This lecture will rest on extracts of *Les Origines de la France contemporaine* bearing witness to the close relationship in Taine's works between literature, history and psychology.

Seminar Room 1. History of anthropology

Eric R. Wolf at Columbia, 1946-1951

John S. Gilkeson
Arizone State University West

In Anthropology (1964), Eric R. Wolf (1923-1999) noted how, since the end of the Second World War, anthropologists had begun to turn their attention away from the "cultures of primitives" they had studied in the past toward the "development of civilization." Increasingly, anthropologists studied acculturation, peasantries, and complex societies. Indeed, by 1964, "the anthropological point of view" had become "that of a world culture, struggling to be born."

Wolf, who spent his childhood in Vienna and the Sudetenland before coming to the United States as a refugee in 1940, made three signal contributions toward the creation of a "science of man" that would "underwrite the new world culture" he envisioned in Anthropology. First, he reoriented the study of peasantries away from Robert Redfield's view of peasants as villagers whose traditional way of life was threatened by urbanization to a view of peasants as rural cultivators whose surpluses, after the crystallization of power in the state, were expropriated by dominant groups. Second, Wolf pioneered a comparative global history that made the histories of peoples supposedly without history integral to the history of the expansion of European capitalism. And third, Wolf helped to redefine cultures from fixed, unitary, and bounded to series of processes in which cultural materials are constantly being made and remade.

This situates the development of Wolf's thought in the five years that he spent as a graduate student at Columbia University from 1946 until 1951. At Columbia Wolf studied with Ruth Benedict and Julian Steward, joined Sidney Mintz and other veterans attending Columbia on the G.I. Bill in the Marxist-oriented Mundial Upheaval Society, interviewed Austrian immigrants for Benedict's Research in Contemporary Cultures project, and engaged in fieldwork in a coffee-growing community for Steward's The People of Puerto Rico project. Drawing on Wolf's published writings and his correspondence, I discuss why he rejected Benedict's attempt to construct a homogeneous national character as ahistorical, substituted a "cultural historical" emphasis on class relations for Steward's "cultural ecological" emphasis on land use, acknowledged his intellectual debt to yet criticized Alfred Kroeber's natural history of civilizations, and thought that he had perhaps learned more from his disagreements with Redfield than his agreements.

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Of Margaret Mead and Biology: 1933-1939

Gerald Sullivan
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Over the years a pervasive vision of Margaret Mead as a cultural determinist who, further, reduced cultural life to various contrasting stereotypes has grown up in some quarters. Such criticism has limited relevance to Mead's first two ethnographic projects: Samoa and Manus (see Mead 1928, 1930). Maureen Molloy (forthcoming) points out the by the time Mead wrote her underappreciated and largely forgotten study of the Omaha, *The Changing Culture of an Indian Tribe*, published in 1932 while she was in New Guinea, Mead had begun to develop further a notion of temperament, or of innate disposition. Mead's use of this notion, albeit in an undeveloped form, can be traced back to chapter nine of *Coming of Age in Samoa* and further has sources in the work of such important psychologists as William McDougall and Kurt Koffka (see Sullivan 2004, forthcoming b). Certainly, beginning in early 1933, Mead along with her third husband, Gregory Bateson, and over the strenuous objections of her second husband, Reo Fortune, began developing her theory of the squares.

Mead never published an explicit account of the theory of the squares, nonetheless it provides the organizing theoretical framework for two of her major contributions to anthropology, *Sex and Temperament in Three Primitive Societies* (Mead 1935) and *Balinese Character: A Photographic Analysis* (Bateson and Mead 1942). According to Mead and Bateson's research proposals (for the originals, see MMP N5:2, N6:2), themselves consistent with squares theory and written in 1935 and 1936 in search of funds to support their researches amongst the Balinese of the Dutch East Indies and the Iatmul, a Sepik river people from the Australian protectorates in New Guinea, personality arises out of the conjunction of temperament, life accident, culture and the individual's reaction to these three previous factors. Biology was, thus, not been absent from her thought.

Far from being absent from Mead's social theory, biology plays several crucial roles in Mead's understanding of how people become particular the particular sorts of people they become. In the theory of the squares, temperament derived from the temporal conjunction and disjunction of individual heredity and a variety of environmental factors, some ecological and others arising out of political economy; to this point my analysis follows previously available work (see Sullivan 2004 especially). This raises the historical question of what sorts of understandings of biology could and did Mead and Bateson seek out in the mid 1930s. Julian Huxley (1942) had yet to publish his synthesis of Darwinian evolution and Mendelian genetics. The structure of DNA and the human genome project were even further in the future. The general intellectual framework with which we are currently familiar was not yet

in place. Bateson and Mead were developing their theory within and against the currents which brought forth the modern synthesis.

Gregory Bateson was the sole surviving son of William Bateson, who coined the term "genetics" in 1905 and brought Gregor Mendel's work into British biology; Mead's (1972) account of the origin of the squares includes a reference to Gregory Bateson's knowledge of Mendel. Mead, through Ruth benedict if not directly herself, must have known of A. A. Goldenwieser's ([1913]1933) application of the idea of convergent evolution to social forms. Mead read Koffka's *The Growth of the Mind*, with its interest in the development of perception, in 1924 and brought it to the attention of Edward Sapir and through him to Benedict in 1925. In the wake of the discussions which lead to the squares, Bateson, in particular, would find inspiration in Ernst Kretchmer's ([1921]1925) work on physiological types and psychological processes.

During the summer of 1934, Mead began writing *Sex and Temperament* while at the Hanover Conference. There she read a package of materials prepared by Earl Engel, an endrochronologist from Columbia's Medical School, what was known about hormones and gender as well as summarizing research on so-called constitutional types. Mead wrote to Bateson discussing Engel's summaries; Banner (2003) mentions this matter, but does not pursue it. The proposed paper will likely focus on this material prepared by Engle.

I should nonetheless note that later in 1934, Mead visited Ireland with Bateson and his friend Conrad Hal Waddington, a British geneticist and student of developmental biology; they discussed the squares. Waddington was to show how chemical messages influence the development of tissues within the body, bringing out *How Animals Develop* in 1935 as well as *Organizers and Genes* in 1940. Bateson and Mead would begin their Balinese researches in early 1936. While in Bali, they read at least portions of Darwin's *The Expressions of the Emotions in Man and Animals* (1872).

During the summer of 1939, Mead and Bateson enjoyed several days discussions with Joseph Henry Woodger, who Mead (1972:249) describes as "the philosopher biologist." Woodger had already brought out books on morphology, physiology and biological principals (1924, 1929) as well as his *Axiomatic Method in Biology* (1937). By the time of their discussions with Woodger, Bateson and Mead were back from Bali and Iatmul and were beginning work on *Balinese Character* (1942).

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Seminar Room 2: Psychophysics in Cultural Contexts

Ways of Hearing, Ways of Listening: Musical Expertise in Sound Sensation Studies of Helmholtz, Mach, and Stumpf

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This paper will address the sound sensation studies of such psychophysical investigators as Hermann Helmholtz, Ernst Mach, and Carl Stumpf. I closely examine their use of musical instruments, and even music itself, as psychophysical instruments. I show how musical experience became constitutive of psychophysics. Helmholtz, Mach, and Stumpf consciously used these instruments to navigate the worlds of both music and natural science and in doing so the experiments they performed were both musical and scientific. To this end, the role and

meaning of musical expertise in scientific study evolved in relation to changing conceptions of hearing, sound, and music itself.

Psychophysics, definable as a set of ideas and methods formed in pursuit of the relationship between sensory physiology and sensory perception, was in the nineteenth-century both an investigative technique and a belief system, interpreted broadly and to different ends by its many practitioners. Located at the intersection of the physical and the psychical, the practice of psychophysics was troublesome. In particular, the practice of psychophysical experimentation on sound sensation often relied on innovative approaches that included the use of such musical instruments as pianos, organs, and violins.

The young Helmholtz, in an 1838 letter declared that he always appreciated music much more when he played it for himself. Though he frequented concerts and operas throughout his life, and would in 1863 publish *Die Lehre von den Tonempfindungen*, a highly influential work where he attempted to connect tone sensation with music theory, it is likely that Helmholtz's enduring engagement with music reflected this very personal relationship of playing a musical instrument for himself. I will develop this idea, shifting the discussion of Helmholtz's work on sound sensation back to its origins, to his initial relationships with musical instruments, with music itself and examine the role of his material interaction with musical instruments. This is an extension of current work in history of science that promotes, indeed calls for, a "materialized epistemology" relating sensual with ideational knowing. This paper is an attempt to apply some of these ideas to the musical realm, to claim that music-making is knowledge-making. More specifically, I ask how Helmholtz's own musical training, his material interaction with musical instruments, his musical expertise, informed his scientific work and motivated his effort to reconcile his physiological theory of tone sensation with a historicist conception of musical aesthetics.

For Mach, musical experience motivated his historicism as well, and much earlier than 1872, the year in which he published *On the Conservation of Energy*, which is often regarded as his first full articulation of his position on the historicist nature of ideas. Aesthetics were psychophysical. Culture, according to Mach, "stamps its unmistakable traces on the human body," musical aesthetics was an alternative entry point to investigating sound sensation. His historicist conception of sound sensation – that *hearing itself* was historically and culturally contingent - may very well be a direct product of Mach's relationship with the music critic Eduard Kulke and joint efforts to locate the accommodation mechanism of hearing and ultimately reconcile sound sensation and musical aesthetics within a psychophysical and evolutionary framework.

An examination of the polemic exchange developed between Wilhelm Wundt and Carl Stumpf will conclude this paper. What began in the early 1890s as a critique by Wundt of Stumpf's application of the Fechner-Weber law to studies of sound sensation became a vicious debate over the role of musical expertise in science. Their polemic highlights the contours of the changing definition and role of musical expertise in psychophysical studies of sound. From the middle to late nineteenth-century, the role of musical expertise shifted from an implicit motivation to a requirement in sound sensation studies. All the while as the models for hearing, sound, and music were also shifting.

Music was a valid avenue through which to approach and understand the sensation of sound, music and sound were treated as interchangeable investigative objects. This suggests that Helmholtz, Mach, and Stumpf all assumed that their audience was well-versed in music theory and had extensive musical experience – enough to follow and be convinced by the musical passages written on the page. Sound sensation was not only an object of investigative study but also a *means* of investigative study. Not only were musical instruments employed as scientific instruments but music itself was an instrument of science. Further, the frequent use of musical examples also assumed the validity of the audience's

individual experience of music. The use of music, of musical expertise, in science, and the acknowledgment of the specificity of individual experience, contributed to new theories – both psychophysical and musical – as well as a new awareness of the historical and cultural contingency of sensory perception.

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Empathy in the Laboratory: Kinaesthesis and Gender in E.B. Titchener's Cornell Experiments

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In 1909, the German aesthetic term *Einfühlung* was translated as "empathy" by the Cornell psychologist Edward Titchener and it became increasingly common in experimental psychological studies of visual, verbal, and auditory stimuli. The psychological meaning of empathy was closely connected to its German aesthetic origins, where *Einfühlung* (literally, "feeling-into") signified bodily and mental responses to an art object or the natural world, and only secondarily indicated the grasp of the mental life of another person. In late nineteenth century and early twentieth century Germany, the lineaments of *Einfühlung* as an aesthetic phenomenon were debated by art historians, philosophers and psychologists, including Robert Vischer, Johannes Volkelt, Theodor Lipps and the members of the Würzburg school of thought-psychology.

In his 1909 *Experimental Psychology of the Thought-Processes*, Titchener designated empathic experiences as kinds of "attitudinal feels"-- organic and whole body responses to experimental materials and to everyday situations.[15] Titchener adopted the kinaesthetic meaning of the term from the writings of the Würzburg School, a meaning that was distinct from the more mentalistic approach of Theodor Lipps. To make his point, Titchener described how his own organic movements and kinaesthetic representations differed in the process of sitting down at his desk to write a personal letter, as compared to sitting down to think through a lecture. At times empathy marked an actual bodily sensation, but it also could signify merely an image or representation of a bodily movement.[16] Titchener admitted that some of these attitudinal feels operated on a physiological level, and thus could function below conscious awareness, but maintained that under the proper experimental conditions, they could be subjectively experienced and described.

Indeed, over the next decade, in a variety of systematic experimental studies, the Cornell laboratory did just that. Empathic responses to visual and other sensory stimuli were often generalized to become markers of an attitudinal set, and beyond that to an empathic type. In one study, for example, the observer, engaged in the process of introspection, reported a feeling of lightness or heaviness when visual figures on a card were raised or lowered. This was an empathic feeling-response that was also called a participatory

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[15] Titchener, *Experimental Psychology of the Thought-Processes* (New York: MacMillan Co., 1909), 21, 214. Titchener credited the Würzburg school, mentioning Bühler, Messer, Marbe, Orth, Ach and Watt. He cited A. Messer Experimentell-psychologische Untersuchung über das Denken" Archiv für die gesamte Psychologie, Vol. 8, 1906.

[16] Titchener had struggled with the possibility of the existence of a kinaesthetic image without accompanying sensations, and claimed in 1909 that an ideal kinaesthetic image did exist. Edward B. Titchener, *Experimental Psychology of the Thought-Processes* (New York: MacMillan Co., 1909)

attitude.[17] The empathic or participatory attitude was deemed similar to a common sense attitude, and both were differentiated from the descriptive or objective attitudes. Because the empathic attitude lay outside the more systematic, descriptive approach favored by Titchener, it could pose problems for introspectionists.

Researchers at the Cornell laboratory ascribed to a psychology of individual differences and classified experimental observers (or research subjects) as having various imaginal inclinations, one among them being the empathic or kinaesthetic type.[18] In a study of belief conducted in 1910, the most empathic subject was as a woman graduate student, who relied on kinaesthetic sensations and an "intense affectivity" in order to make judgments regarding her belief or disbelief of verbal statements. She reported shifts in her bodily attitude and changes in her breathing as accompaniments of these judgments. For this research subject, a strong feeling of belief or disbelief was signaled by a stiffening of the body.[19] Although another woman subject did not demonstrate a similar empathic kinaesthesis, the researchers were surprised when a male instructor of the verbal type described his own, empathic-kinaesthetic response. Although empathic responses were theoretically possible for all experimental observers, in these studies a typology of empathic responders began to emerge, with women being the most adept. Rather than being one of many possible way of responding, the study of empathic response often became the characterization of a type.
My paper examines the experimental literature comparing research reports (many of which were published in the *American Journal of Psychology*) and manuscript sources to outline the ways in which kinaesthesis, empathy, gender and attitude were linked in the early years of the twentieth century in the psychological studies of the Cornell laboratory. Interest in a bodily-based conception of empathy waned by the 1920s, in part due to the decline of systematic introspection, but also as social psychologists and psychoanalysts began to utilize the term more exclusively to delineate a process of understanding that took place between individuals. The kinaesthetic meaning of empathy rooted in aesthetics and characterized by a sensory resonance is a largely forgotten meaning of the term, and a genealogy of empathy that revives this meaning may be of relevance to current theories of empathic mirroring in diverse fields such as social neuroscience and philosophy of mind.

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[17] M. Zigler, "An Experimental Study of Visual Form" *The American Journal of Psychology*, Vol. 31, No. 3 (Jul. 1920), 286; "Empathic" came to mean participatory for social scientists as well.See R. M. Ogden, "Sociology and Gestalt Psychology" The American Journal of Psychology, Vol. 46, No. 4 (Oct. 1934), 653.

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Recovering the Losers: the Place of Panpsychism in Fechner's Work

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In most manuals of History of Psychology, Gustav Fechner is celebrated as the author who made the difficult and long awaited bridge from the pre-scientific to the scientific stage of Psychology. This would have happened thanks to his psychophysical work, in which not only a psychological measure of sensations was proposed, but also included in a high complex general law, related to physical stimulus. This is the so-called Fechner Law, or as he prefered, the Weber-Fechner Law. Thanks to his contribution, exposed in his *Elements of Psychophysics* (1966/1860), Fechner was considered as a "science superman whose life was devoted to 'hardnosed' research" (Bringmann, Bringmann, Medway, 1987). Nevertheless, if we take his writings as a whole, we notice that his work *Elements of Psychophysics* is an island amidst metaphysical, religious, and aesthetic problems. Without trying to see in the author an unsuspected unity, it is necessary to ask about the relations between *Elements of Psychophysics* and works as *The Little Book of Life After Death*; *Nanna, or the Soul Life of Plants*; *Zend-Avesta*; *Concerning Souls*; *The Three Motives and Grounds of Faith;* and *The Daylight View Opposed to the Night View* (all off these works are edited and translated entire

or partially by Lowrie, 1946). We can include in this list satiric works under the pen name Dr. Mises as *The Comparative Anatomy of the Angels*. Would there be in Fechner works a similar division as the assumed by the epistemologist Gaston Bachelard between a day work (epistemology) and a night work (aesthetic)? In this sense, would there be a "The Daylight View Opposed to the Night View" in fechnerian terms? Fechner gave another meaning to this opposition, inverting the terms of the bachelardian division, because his daylight view represents a spiritual view in opposition to the sterile materialism of the night view. More than a division between science, religion, methaphysic and aesthetic, Fechner tried to constitute a single Cosmovision. However, this division in Fechner's work will be established by a great number of authors in the History of Psychology, as highlighted by Bringmann, Bringmann, Medway (1987:243-244), giving special attention to E. G. Boring (1950) classical work. More than separated, the non-scientific interests are disparaged or omitted in the name of the current status of Psychology as a behavioral science. In Fechner's case, his lifelong interest in psychical research, parapsychology and in supernatural phenomena is openly omitted. Consulting the principal textbooks in the History of Psychology, such as Boring (1950), Klemm (1933), Gardner Murphy (1960) and Brett (1963), only the two latter authors do not place Fechner's non-scientific interests together with biographical curiosity. In some cases, an inversion would take place, as the one performed by William James, clearly against Fechner's psychophysics (James, 1890), and at the same time respecting his religious work (considered a "Thick Philosophy" – James, 1906). For Latour, this operation of scission, derogation, and omission in the history of one science reveals an asymmetrical principle between winners and losers. This principle becomes very complicated in the evaluation of an author's works taken as a whole, as it tends to delete his obscure and non-triumphant conceptions. Without forcing some doctrinaire unity, one must take Fechner's work as a whole, in order to preserve its tensions and problematizations, without imposing ruptures or chasms on it (S. Jaeger, 1987). Inasmuch as one cannot separate the physicist Newton from the alchemist, since his Universal Gravitation Theory was based in the Attraction Theory of the Alchemy, one cannot separate Fechner's psychophysik from his Daylight View. How can these two dimensions be articulated? The aim of this work is to restore the psychophysics in Fechner, breaking apart from the prevailing conception that views it a mere tool serving psychology's scientific rigor. Bearing this in mind, this work
attempts to place psychophysics as an empiric function of Fechner's metaphysical and religious thought, the so-called "Daylight View". For that, such philosophy will be analyzed in its components present in his metaphysical books, as the panpsychism hypothesis, the animist nature conception, the hierarchy of the souls, and the pantheist conception of God. A special case of this works is his "Spiritist Diary" (not published, but partially exposed in Bringmann, Bringmann, Medway), that describe his observations in a series of séances with the American medium Henry Slade. This kind of psychical studies, common in the official psychology of the end of 19th & begin of 20th century, was justified by Fechner comparing the relation of these phenomena to the pathological ones. If the pathological phenomena is important for the understanding of our physiological and psychological life, psychical phenomena is crucial to comprehend our panpsychical existence. Finally, I will discuss the reasons that led to the exclusion of all these aspects in the historiography of psychology, taking into account points of view like that of Despret (2004).

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Main Auditorium

Discrimination, Race, and Social Class	
11.00-11.30	Stephen Berger (Springfield College), Social Class, Socialization, Discrimination, and Violence: How Allison Davis and Robert Havighurst Shifted the Question
11.30-12.00	Fran Cherry (Carleton University), The Peregrinations of Paired Testing: a Brief History
12.00-12.30	Leah Gordon (University of Pennsylvania), "Whether Mature Individuals or the Mature Society are Developed First": Social Theory, Social Action, and the Race Problem at Fisk University, 1944-1954

Seminar Room 1

11.00-11.30	Hank Stam (University of Calgary), Buytendijk and a Unique Phenomenological Psychology in mid-20th century Europe
11.30-12.00	Rene Hezewijk (Open University of the Netherlands), Psychology as an Autonomous Discipline: Johannes Linschoten's Dissertation
12.00-12.30	John Greenwood (City University of New York), Thomas Huxley and Epiphenomenalism

Seminar Room 2

New Perspectives on the History of Behaviorism	
11.00-11.30	Michael Sokal (Worcester Polytechnic Institute), The Major Impact of a Disability: Cattell and the Course of Early American Experimental Psychology
11.30-12.00	David Clark (York University), Edwin Guthrie's Reply to Karl Lashley
12.00-12.30	Marissa Barnes (York University), What was Really Going on at Bartlett's Cambridge University?

Main Auditorium: Discrimination, race, and social class

Social Class, Socialization, Discrimination and Violence: How Allison Davis and Robert Havighurst Shifted the Question

Stephen D. Berger
Springfield College

In the late 1930s, the African-American social anthropologist Allison Davis played a strong role in the studies of "Negro youth" of the American Youth Commission. He wrote the first outline for the study, and was the senior author (with John Dollard) of the one of the four major studies produced (and the one which sold the best): *Children of Bondage* (1940). While these studies were supposedly focused on the question whether minority status was a significant factor in the development of Negro youth, Davis' book focused on issues of family socialization in the context of social class.

Davis had worked previously with the social anthropologist W. Lloyd Warner, first on the Yankee City studies of Newburyport, and then on the study of Natchez, Mississippi in which black and white husband-and-wife teams studied both sides of this segregated community. (*Deep South*, 1941) Warner's particular innovation was the "discovery" that communities were stratified into social classes – a "discovery" somewhat vitiated by the fact that Warner's technique made this true by definition.

John Dollard, older and more well-known, had been Edward Sapir's assistant in the Rockefeller-funded international culture-and-personality course held at Yale University in the early 1930s, as well as participant in all of the Rockefeller culture-and-personality conferences of the time. Impressed with psychoanalysis, Dollard was presumably one of the sources of the emphasis on family dynamics in *Children of Bondage*. He had previously published his own book on southern racial dynamics, *Caste and Class in a Southern Town* (1937), as well as a review and statement for the Social Science Research Council on how to study personality development.

Physicist Robert J. Havighurst had become interested in high school science education. He was hired by the Rockefeller Foundation's head physicist, Max Mason, in 1934, to work on the science portions of the Rockefeller General Education Board's major attempt to transform high school and junior college education in the United States, their general education program. By 1937, Havighurst took over the entire direction of this program, as well as the associated child study research efforts that had been funded. Havighurst took himself around the country to familiarize himself with the child study programs and the people conducting them. He was impressed with Erik Erikson – and with Allison Davis.

When the American Youth Commission proposed the studies of Negro youth, Havighurst was unimpressed. Given that the General Education Board (GEB) had finally noticed there was a continued major depression in the country, and given his awareness that African Americans were discriminated against by both employers and unions, he reported that his people wanted to see an emphasis on jobs, rather than on the feelings being engendered among young blacks. However, since the GEB would fund almost anything having to do with blacks, and since the GEB's attention when the grant came up was on whether to keep funding the American Youth Commission at all, the proposal was funded.

Children of Bondage is a curious book. Supposedly about youth, the authors' interest in early childhood socialization led them to focus more on their subjects' earlier years. Given

Davis' training with Warner, and given black intellectuals' concern that the image of the race was being tarnished by the sexual, alcoholic, family and criminal practices of poor blacks, this interest in early childhood socialization was located within a social class context: blacks engaged in bad practices, so it was argued, because they were artificially forced to remain poor. They taught the culture of these practices to their children, who did not do well in school, moreover, because black teachers were middle-class and were unsympathetic to the culture of their lower-class students.

While the authors claimed that this social class shaping of socialization practices and culture was their discovery, a study of Davis' training manual for his interviewers shows instead that it was an assumption which was brought to the study. A perceptive reader/critic of the draft of their study noted how they had shifted the focus away from the effects of racial minority status towards family socialization. And this despite the fact that the book also includes, though it does not emphasize, a powerful statement regarding the environment of violence within which black people have to live.

When the GEB general education program was winding down and the GEB at the end of its funds, Robert Havighurst left the fund to take a position as educational psychologist in the Department of Education and the Committee on Human Development at the University of Chicago. While he wrote to his colleagues in the southern division of the GEB that he hoped they would strive mightly to keep Allison Davis in the South, Davis, by 1942, ended up in the same department as Havighurst. By 1944, Havighurst would write that the legacy of the child study research programs funded by the GEB was the emphasis on social class, and by 1945 he would collaborate with Davis on studies of social class and child rearing. In 1947 they jointly published a popular book on family socialization, entitled *Father of the Man*, which also treated social class.

Allison Davis published an article on caste and violence in 1945, but the majority of his work remained focused on social class and learning.[1] Robert Havighurst, too, retained a significant focus on social class and socialization, though he noted in the early 1960s that his attitude towards social class had shifted: earlier the use of the concept had carried a critical ring to it, but he now believed that the different social classes needed each other.

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The Peregrinations of Paired Testing: A Brief History

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In the immediate post World War Two period, it is well documented that social scientists had become allied with liberal reform efforts for racial desegregation in the United States (Biondi, 2003; Jackson, 2001; Richards, 1997). The case of *Brown v. Topeka Board of Education* (1954) is often viewed as the exemplar linking research to activist efforts for greater racial justice. Debates over the interpretation of projective techniques used to establish a case for desegregated education have to some extent obscured the wider array of investigative practices developed in the 1940s and 1950s. In the period prior to the mass civil rights era, community self-survey techniques were developed in other equally important areas of discrimination such as housing, employment, commercial transactions and public accommodations (Sanders, 2001; Wormser & Selltiz, 1951). I will focus on paired testing as one of the many community based investigative practices developed to redress discrimination.

Paired Testing for Discrimination

I will argue that paired testing is an overlooked approach to experimentation largely for reasons of its legal and political consequences, although there are ethical and methodological to be considered as well. Paired testing involved the monitoring of responses to trained interracial testers (sometimes called checkers or auditors) in various transactions. The technique engaged social psychologists who had become involved in the action research group established in New York by Kurt Lewin, namely, the Commission for Community Interrelations (CCI) of the American Jewish Congress. It was CCI staff member, Claire Selltiz, who had become familiar with the pioneering community self-survey work of the Race Relations Department at Fisk University in Nashville. She forged a research relationship with the Committee on Civil Rights of East Manhattan (CCREM), an umbrella organization of about two dozen civic groups (CCREM, n.d.). In 1949 and again in 1951, Selltiz (with the resources of Kenneth Clark at City College of New York and Northside Child Guidance Clinic) consulted on a paired testing study of racial discrimination in east side Manhattan restaurants (Selltiz, 1955). Civil rights historian, Biondi (2003) has interpreted the founding of the CCREM as "an indication of the success of African American activists in pushing the issue of race discrimination onto the agenda of white liberals" (p. 81). It is furthermore significant that participatory action researchers like those at CCI developed investigative practices appropriate to the work of such interracial collaborations.

Paired testing in community based settings migrated from psychological social psychology, as laboratory based experimentation became the dominant style of research. The practice of paired testing will be traced through its later usage by social scientists in a variety of disciplines and contexts where there was not as great a reliance on independent variable manipulations in the laboratory. It has been urban anthropologists, sociologists and economists who have developed controlled interventions and observations in a wide range of contexts including restrictive housing and public accommodations, unfair employment practices, and a host of other everyday commercial transactions (Fix & Turner, 1999; Henry, 1989; Henry & Ginzberg, 1984). In one documented instance, when paired testing returned to social psychology, it was challenged on largely ethical grounds despite the use of procedural

rigour and aggregated quantitative analysis. Connor wrote up this attempt in 1999 to have students at her university conduct a paired testing study for the National Center for Case Study Teaching in Science along with teaching notes (Connor, 2000). Paired testing has rarely achieved more than anecdotal usage by social psychologists largely to exemplify gross ethnic differences (Aronson, Wilson & Brewer, 1998; Brown, 1995) while its' use by investigative journalists (see Sawyer & Lakaskiewicz in Aronson, Wilson & Brewer, 1998) and urban advocates (Fix & Turner, 1999) has been more common.

While experimentation has been practiced in a variety of ways over psychology's history (Danziger, 1990) with a particular style becoming dominant in experimental social psychology by the 1970s (Winston, 1988; Winston and Blais, 1996). Paired testing migrated out of social psychology as psychological social psychologists were drawn to the science of hypothesis testing in laboratory settings and a more cognitive orientation to the social psychology of prejudice rather than the social phenomenon of discrimination. This narrative, however, does not acknowledge the diversity of investigative practices developed as part of the post-World War Two civil rights period (Cherry & Borshuk, 1998; Jahoda, Deutsch & Cook, 1951). The documenting of everyday practices of discrimination through systematic paired testing fell outside the boundaries of acceptable social psychological knowledge. A separation of experimenter and research participant, a move from real communities into the laboratory, and the separation of research outcomes from social action and social policy became hallmarks of psychological social psychology. Political as well as disciplinary constraints in the 1950s will be discussed as deterrents to paired testing and action research more generally.

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"Whether the Mature Individuals or the Mature Society are Developed First": Social Theory, Social Action, and the Race Problem at Fisk University, 1944-1954

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At Fisk's second annual Race Relations Institute in 1945, biologist M.F. Ashley-Montagu argued that competing frameworks for studying the race problem should be developed even if they could not be integrated. He told an audience of social scientists, educators, and activists that, since "human behavior is rarely to be explained in terms of single processes," collaboration between specialists was essential. "The great need," Montagu explained "is for human ecologists—liaison officers between the various sciences of man."[1]

Montagu was not the only postwar scholar-activist who recommended linking disparate social scientific approaches to the race problem. Two assumptions permeated discussions at Fisk's yearly Race Relations Institutes (established 1943). Institute leaders assumed both that dialogue across disciplinary boundaries would lead to more sophisticated social science and that social scientific knowledge should inform political strategy. The history of Fisk's Race Relations Institutes (RRI) reveals that wartime and postwar African American scholar-activists often developed political agendas that did not follow from their social scientific findings. Black intellectual networks were more tolerant of analyses of the race problem that emphasized the structures of capitalism or political domination than were prominent white universities. At the same time, scholar-activists at Fisk generally embraced a narrow racial liberalism, calling for anti-discrimination legislation, integration, and anti-prejudice education.

Fierce disagreement about the forces that contributed to racial injustice and inequality—and about the meaning of and best strategy for racial progress—emerged throughout twentieth century African American intellectual and political life.[1] In the interwar period, politically engaged social scientists of race relations—many working at black colleges—had described economic competition and violent political oppression as the source of racial conflict.[1] By the postwar years, however, prominent white research universities, prompted by philanthropic support for the behavioral sciences and religious organizations'

interest in prejudice, increasingly rooted the race problem in white psychology and morality.[1] In addition, wartime and postwar civil rights leaders appealed for justice by emphasizing democratic ideals in the face of the fascist, and then communist, menace.[1] Many postwar Americans, moreover, assumed that Gunnar Myrdal's *An American Dilemma* produced the final word on race relations. As a result, the existing historical narrative maintains, a "liberal consensus on the race problem"—a social theory that saw legal desegregation, anti-discrimination legislation, and anti-prejudice education as key aspects of the struggle for racial justice—dominated postwar social science, activism, and policy.[1]

The history of Fisk's Race Relations Institutes reveals that individualistic understandings of the race problem did not set the terms of debate in all postwar intellectual arenas. The postwar flowering of the psychological sciences certainly impacted African American institutions.[1] Nonetheless, the psychology of prejudice did not drown out sociological, economic, and political analyses of the race problem at Fisk as in many prominent, white research universities. In their discussions of the origins of the concept of race, of prejudice, of racial conflict, and of racial inequality, Institute participants suggested that America's postwar race problem emerged from labor exploitation, the structures of capitalism, and violent political oppression under slavery and colonialism. In fact, throughout the 1940s and 1950s, RRI sociologists and psychologists called—as Montagu did—for reconciling competing social theories. Nonetheless, the contradictions between dispositional, systemic, and relational conceptions of the race problem remained largely unresolved at Fisk's Institutes.[1]

Since social theories with contradictory implications for action were simultaneously articulated at the RRI, one might have expected heated debate about activist strategy. When discussing the best course of action, however, RRI scholar-activists produced a surprising level of consensus. While in the Institute's early years a few participants had demanded full employment and a far-reaching social welfare agenda, Institute participants largely embraced the tenets of racial liberalism, demanding anti-discrimination legislation, legal desegregation, and anti-prejudice education.

A number of factors explain why, although the parameters of social scientific debate were wider at Fisk than at many white research universities, Institute participants translated only some social scientific approaches into theories of action. Patterns of philanthropic support for African American universities as well as the tendency to challenge social scientific universalism were both important. The political realism of the scholar-activists who contributed to the RRI was, however, the most decisive factor distinguishing social scientific findings from activist agendas. Assumptions about the kinds of reform that would garner popular and court support—an understanding strongly influenced by international developments and a long history of frustrated activism—motivated both local and national reform initiatives.[1] Like black activists across the country, RRI leaders and participants quickly took up the language of civil and human rights, as well as the anti-Nazi emphasis on prejudice. Demanding economic redistribution and highlighting interracial competition for resources, however, fit uncomfortably within liberal, anti-communist rhetoric. As a result of assumptions about the politically permissible, Institute scholar-activists called for individualistic social policies despite thoroughly understanding the relational and systemic foundations of the race problem.

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Seminar Room 1. Phenomenological Psychology

F. J. J. Buytendijk and the creation of a unique form of Phenomenological Psychology in mid-twentieth century Europe

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In our ongoing work on Dutch phenomenological psychology of the mid-twentieth century, the role of Frederik Buytendijk is central to understanding the unique formulation of both the program and its resulting demise. Buytendijk, trained first as a physician and then as a physiologist, began a series of studies in animal psychology during World War I for which he received his doctoral degree in physiology. In 1929 he moved to the University of Groningen and continued his physiological research until the end of the 1930s when his work began to take on a general theoretical, philosophical tone. In 1937 he converted to Catholicism under the influence of, among others, Jacques Maritain, Gabriel Marcel and Romano Guardini. A strident anti-Nazi, he was first held hostage by the Gestapo and then went underground for the remainder of WWII. After the war he accepted the chair of Psychology at Utrecht University, an appointment that was not entirely uncontroversial given Buytendijk's lack of formal training in psychology. He quickly developed a reputation for his work on existential-phenomenological subjects and he surrounded himself with a range of colleagues and students, among them Johannes Linschoten, who shared aspects of his vision of scholarship and academic life. Linschoten eventually succeeded to Buytendijk's chair in Utrecht upon the latter's retirement in 1957. Linschoten's early work is especially characteristic of the orientation that Buytendijk brought to psychology. A thoroughly philosophical-anthropological attempt to understand human life as a variation of boundedness and freedom, Buytendijk's vision of psychology was far larger than the experimental psychology of his day. Nonetheless Buytendijk established a psychological research laboratory in Utrecht if only for the reason that the teaching of psychology required that laboratory experience be

available and graduate students generally conducted research there. Buytendijk himself was drawn almost entirely to philosophical or broader, social topics. In this respect his thinking drew on the work of a range of European thinkers especially Merleau-Ponty, for it afforded Buytendijk an opportunity to develop his notion of intersubjectivity that was both corporeal and yet also human and meaningful.

Buytendijk's influence is readily seen in the early work of Johannes Linschoten who wrote a number of phenomenologically inspired works in the early 1950s. These works, including his Master's thesis on movement (which will be discussed in the companion paper by René van Hezewijk), are indicative of Linschoten's struggle to find a unique voice under the domineering tutelage of Buytendijk. Linschoten's eventual apparent rejection of phenomenology (which was influential in undermining phenomenological psychology in the Netherlands) is more a rejection of Buytendijk and the latter's version of phenomenological psychology. Even here Linschoten's debt to Buytendijk is clear for while Linschoten turns to the laboratory and experimental psychology to understand the limits of human thought and the importance of the scientific world view, he cannot help but be concerned still with the nature of human experience. In this paper we discuss the personal and public tensions in the work of Buytendijk and Linschoten in the 1950s and the resulting consequences for psychology in the Netherlands and elsewhere. For the Dutch phenomenological school's influence extended to other locations, especially Duquesne University in Pittsburgh.

Psychology as an autonomous discipline: Johannes Linschoten's dissertation

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In 1956 Johannes Linschoten – one of the members of the alleged phenomenological Utrecht School – presented his dissertation on binocular space perception. He surprised many a colleague with a work of 573 pages plus a booklet of 226 figures, written in German, containing an introductory part, an experimental part and a theoretical part (Linschoten, 1956).

The introduction offered an overview of the theory that was focussed most on binocular space perception at that time (Hering's Theory of Identity), and its follow up versions.

The experimental part reported of 130 experiments. In the experimental part Linschoten not only criticized this theory, but also other candidates from other, more general, theories of perception (Wheatstone, Helmholtz, Gestalt theory). He discussed the Panum-effect in his analysis and experiments, and attraction of image points of the two retinal images involved as an explanation, as well as the role of eye movements.

The theoretical part is an analysis of the role binocular depth perception plays for the organism in the localisation of significant objects in the depth of the "structured spaceness (Räumlichkeit)". He presented a dynamic theory of binocular space perception and concluded with his answer to the question why human beings have two eyes.

Although at first hand this dissertation can be viewed as an excellent combination of experimental *and* theoretical approach to solve a complex problem, and as the opposite of a phenomenological approach that could have been expected from this author, we argue for a different point of view. We will point out that the phenomenological approach in the Utrecht

School of the fifties did not exclude experimental work at all, and will demonstrate that, more importantly, Linschoten's aim with his dissertation was to argue that in any account of binocular depth perception a psychological explanation is inevitable. In other words: psychology is an autonomous discipline. It demonstrably involves explanatory problems that can only be solved by presupposing a *psychologically* active organism.

Continuing our research Linschoten in psychology (Stam & Van Hezewijk, 2004; Van Hezewijk & Stam, 2006; Van Hezewijk, Stam, & Panhuysen, 2001, , 2002) we tend to believe that initially Linschoten believed that only a phenomenological approach could guarantee the autonomy of psychology as a discipline. However, Linschoten changed his views during his short life, to end with the apparent opposite view (Linschoten, 1964), that psychology should be reductive and experimental.

Our argument will include references to Linschoten's recovered master thesis (Linschoten, 1949), earlier work on space and movement perception (Linschoten, 1950, , 1952), and copies of handwritten notes for his dissertation.

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Thomas Huxley and Epiphenomenalism

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Thomas Huxley is generally supposed to have provided the classical statement of *metaphysical* epiphenomenalism in his 1874 address to the meeting of the British Association for the Advancement of Science in Belfast, entitled "On the hypotheses that animals are automata, and its history." According to this doctrine, conscious mentality is merely a "collateral product" of the mechanistic neurophysiological processes that generate behavior, and has no effect on neurophysiological processes or behavior, or any other physical state or process. Huxley claimed that "all states of consciousness [in animals and humans]...are immediately caused by molecular changes of the brain-substance," but denied that "any state of consciousness is the cause of change in the motion of the matter of the organism" (1874, p. 244). According to Huxley, "states of consciousness" are like the "steam-whistle which accompanies the work of a locomotive engine" and "is without influence upon its machinery" (1874, p. 204). He represented this claim as the natural conclusion of mechanistic theories of reflexive behavior developed in the nineteenth century, which justified the extension of Descartes' (1664) conception of animals as mechanical automata to encompass all human behavior.

Huxley did not use the term "epiphenomenalism" to characterize his own theory. The term "epiphenomenal" comes from medicine, and describes a symptom of a disease that plays no causal role in the generation or maintenance of the disease. The term was first used by William James to describe Huxley's "conscious automata theory," and the theories advanced by Shadworth Hodgson (Hodgson, 1870), William K. Clifford (Clifford, 1874) and Henry Maudsley (Maudsley, 1886), according to which conscious mentality is "an inert spectator of the autonomous mechanical functions of the central nervous system" (1890, p. 129).

In this paper it is argued that Huxley did not present arguments in support of metaphysical epiphenomenalism, and that it is very doubtful if he held this view. Rather, Huxley presented a variety of arguments in favor of an *empirical* epiphenomenalism, according to which conscious mental states are mere byproducts of the neurophysiological states that generate behavior, but do have other effects within the nervous system. Huxley's empirical epiphenomenalism was on a logical par with William James' (1890) theory of emotion and Hugo Münsterburg's (1888) theory of the will. James and Munsterberg claimed that emotions and consciousness of will play no role in the causal generation of the relevant target behavior, but did not deny that they have other effects within the nervous system. Thus, according to James, emotions such as fear do not cause flight and physiological arousal, but are merely our consciousness of the behavior and physiological arousal that are our reaction to the objects of our emotions (such as a bear in our path). Münsterberg claimed that so called consciousness of will is not consciousness of an autonomous act of will that determines behavior, but of an automatic sensational response to our physical anticipation of behavior, based upon muscular feedback.

Epiphenomenalism did not follow from the extension of mechanical reflexive explanation to all forms of human behavior, including those mediated by consciousness mentality. Ivan Sechenov, who developed a comprehensive mechanistic and reflexive theory of the nervous system, was neither a metaphysical nor empirical epiphenomenalist. Although Sechenov is famous for having denied that thought is the cause of behavior, all he meant was that thought is merely the *proximate* cause of behavior, which is itself fully determined by external sensory stimulation: "the initial cause of all behavior always lies, not in thought, but

in external sensory stimulation, without which no thought is possible" (1863, p. 322). Sechenov did not claim that thought is a byproduct of the mechanical processes responsible for behavior, but rather that it is an intermediate cog in a mechanistic reflexive causal chain running from sensory stimulus to behavioral response. It is argued that Huxley also conceived of the causal efficacy of the mental states of animals and humans in this way, as "parts of the great series of causes and effects" (1874, p. 244). He treated mentality as analogous to the drive wheel of the locomotive rather than its whistle.

Huxley's empirical epiphenomenalism was restricted to *consciousness*, rather than mentality per se. He claimed that the consciousness that often accompanies mentality and behavior plays no role in the causal generation of behavior, including complex and purposive behavior, since such behavior can occur in the absence of consciousness. In support of this claim, Huxley cited cases of decorticated frogs that continue to engage in complex purposive behavior, and brain damaged patients who engage in complex purposive behavior while unconscious. These cases provided some support for a limited empirical epiphenomenalism about consciousness, they did not support the *universal* empirical epiphenomenalism that Huxley expoused, since these very same cases provided strong evidence that consciousness sometimes does play a critical role in the generation of human behavior.

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Seminar Room 2. New Perspectives on the History of Behaviorism

The Major Impact of a Disability: James McKeen Cattell and the Course of Early American Experimental Psychology, 1880-1904

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In 1880, James McKeen Cattell opened the personal journal he kept while a student in Europe by despairing of ever describing in it what he thought and felt, and concluded that he should simply "better … write[e] what I do." And as Dorothy Ross once commented, "that's exactly how American psychology turned out!" This presentation traces the impact of Cattell's inability to describe (and even observe) his thoughts and feelings, starting with his difficulties (as a laboratory student in Leipzig) in applying Wundt's preferred procedure of inner Wahrnehmung, much less the more formal procedures of Selbsbeobachtung used by other. As Kurt Danziger has shown, this disability led Cattell to introduce new terminology into the lexicon of experimental psychology – he seems to have been the first to use the word "subject" to describe those performing a specific experimental activity -- that remained dominant at least through 1990s. It continues with a brief overview of Cattell's two major laboratory programs of the late 1880s, which he carried out at the University of Pennsylvania with collaborators Georges S. Fullerton and Charles S. Dolley. These two experiments sought – solely by measuring an individual's physical movements -- to redefine the results of traditional psychophysical investigations and to measure the velocity of the human nervous impulse.

After Cattell moved to Columbia University in 1891, he immersed himself in creating a program of (what he called) "mental tests," which he later claimed to be the world's first such system. Nevertheless, in designing this program, he (and his collaborator Livingston Farrand) continued to avoid any attempt to address any traits other than performance and

indeed they limited themselves to measuring human performance of rather simple tasks divorced from any real-world setting. Consequently, the results of Cattell's tests correlated literally with nothing of interest to any working psychologist or educator, and with the 1901 failure of this program Cattell abandoned his career in the laboratory for the editorial and library desk.

This move did not stop other psychologists from looking to Cattell, and in 1904 his contemporary Hugo Münsterberg commissioned Cattell to speak to the St. Louis International Congress of Arts and Sciences, held in connection with the Louisiana Purchase Exposition, on the subject of "The Conceptions and Methods of Psychology." Cattell's address reflected his 20 years of laboratory experience. He claimed, for example, that "most of the research work that has been done by me or in my laboratory is nearly as independent of introspection as work in physics and zoology," and that "it is usually no more necessary for the subject to be a psychologist than it is for the vivisected frog to be a physiologist."

Most importantly, in this presentation Cattell extrapolated from this point of view to emphasize "the practical applications" of his field and to call for the creation of "a profession of psychology." He explicitly stated that "if I did not believe that psychology ... could be applied in useful ways, I should regard my occupation as nearer to that of the professional chessplayer or sword swallower than to that of the engineer or scientific physician." As he continued, he saw no reason why "the application of systematized knowledge to control human nature may not in the course of the current century accomplish results commensurate with the nineteenth century applications of physical science to the natural world."

To be sure, American psychologists in the 1890s had often claimed the ultimate utility of their science – usually (as Cattell did for his tests) in educational settings -- and in 1893 Cattell even argued that psychology "shall be able to determine what distribution of labor, wealth, and power is best." Less rhetorically, from around 1900 Münsterberg and Walter Dill Scott, among others, began practicing what emerged during the 1910s as industrial psychology and even earlier Cattell's student Lightner Witmer modified Cattell's tests to define a specialty of (diagnostic) clinical psychology that gave shape to what remained a vision for Cattell.

Nonetheless, many of Cattell's younger American colleagues -- especially such important 20-century psychologists as James R. Angell and Lewis M. Terman and John B. Watson and Robert M. Yerkes – resonated heavily with all that Cattell had to say, and they each later claimed that Cattell's remarks helped determine the course of his career. Indeed, as Watson introduced a term that Cattell never used (in any of its forms), he dedicated to Cattell his Psychology from the Standpoint of a Behaviorist. Perhaps more notably, even though the details of Münsterberg's own later practice as an applied psychologist owed little if anything to Cattell's ideas, he also made sure to dedicate to Cattell his own major systematic book on the subject, Grundzüge der Psychotechnik. This presentation closes by reviewing the impact of Cattell's 1904 address on these colleagues in some detail.

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Edwin Guthrie's reply to Karl Lashley

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In 1958, the American Psychological Foundation presented its 3rd Gold Medal Award to Edwin Guthrie (1886-1959). The award was intended for psychologists who made a distinguished contribution to the content and status of psychology, previously honored were Robert Woodworth and Lewis Terman. Guthrie was honored for his "major theory of learning." The scroll accompanying his medal read, "... presented to Edwin Ray Guthrie for his distinguished contribution to psychology, as a theorist of the science of learning...." Robert Sears presented the award and said,

> Guthrie's flare for theory was first exemplified in a textbook he wrote with his good friend and colleague, Stevenson Smith, in 1921. For a decade, "Smith and Guthrie" was a provocative goad to the development of the American behavioral point of view in psychology. But, before the decade was much more than over, he had published the first edition of his little blue book called *Theory of Learning* (1935). ... It was a small book ... but it has become a milestone in the development of learning theory. (American Psychologist, 1958, 13, p. 739)

Sears's juxtaposition of these two textbooks was interesting. Although both were dedicated to a scientific explanation of learning, they represented substantially different approaches. *General Psychology* (1921) explained learning in terms of the conditioned reflex, while *The Psychology of Learning* (1935) was devoid of biology, and, in a word, it treated learning in the abstract. This difference marks an important change in Guthrie's thinking, and I believe it also marks his emergence as the psychologist who eventually received the gold medal.

What explains his radical new approach? Sheffield (1959) said that soon after the publication of Anrep's translation of Pavlov's *Conditioned Reflexes* (1927), Guthrie (1930) wrote one of his most important theoretical papers in which he expressed most of the ideas that later characterized his theory. Guthrie's son, Peter, supported this position (Prenzel-

Guthrie, 1996). Coleman (1999) indicated the 1930 article was important. These sources point the direction for exploration into perhaps the most creative and productive period of Guthrie's intellectual development; however, because Guthrie (1930) followed the publication of *Conditioned Reflexes* does not mean it was occasioned by Pavlov.

The events that constitute Guthrie's theoretical development appeared sequentially in several articles published in *The Psychological Review* between 1930 and 1935. The main event was Guthrie's (1930) alternative interpretation of Pavlov's research. It was brief, and it was the genesis of the "little blue book," described by Sears as a milestone in learning theory. Guthrie (1930) was criticized by Pavlov (1932) in the only article Pavlov ever published in an American psychological journal. Previously, I argued before the members of Cheiron that Pavlov's criticism occasioned developments in Guthrie's theory. However, further research suggested that Pavlov's active role in Guthrie's metamorphosis was only part of the story, perhaps a minor one, and I now believe that Karl Lashley (1890-1958) played a larger role, occasioning Guthrie's 1930 article, and consequently indirectly contributing significantly to Guthrie's theory for which he received the gold medal. I do not believe that this connection has been made before.

Ironically, Guthrie's intellectual development was possibly goaded on, perhaps the result of threatening events. It was plausible that Guthrie's actions were reactions, first to Lashley's criticism and only afterwards to Pavlov. In this paper, I will revisit the creative period of Guthrie's development between 1930 and 1935, and argue that an important influence, Lashley, was overlooked in Guthrie's historical portrait by previous biographers, including myself. First, I will discuss Lashley (1930) in terms of its relevance for Guthrie. Briefly, on September 4, 1929, as President of the American Psychological Association, Lashley addressed the Ninth International Congress of Psychology. His talk was later published in *The Psychological Review*, "Basic Neural Mechanisms in Behavior" (1930). Therein, Lashley criticized explanations of learning; the crux of his criticism was that research had failed to substantiate the conditioned reflex, a central construct in Smith and Guthrie's 1921 textbook. This implied not only that Guthrie's (1921) textbook was incorrect, but its existence was inhibiting progress in the science of learning. This, I believe, created a crisis for Guthrie, and he re-thought his position.

I will present Guthrie's (1930) "Conditioning as a Principle of Learning" as a reply. Lashley (1930) damned both the conditioned reflex theory and associationism. Guthrie had to accomplish two things: separate himself from Pavlov and the reflex, and reaffirm the usefulness of associationism for scientific discourse. Lashley implied that psychology needed a new guiding principle, Guthrie's stated goal was to formulate a theory to guide research in learning. I will argue the origin of Guthrie's major theory of learning was the result of this, and describe his change as a shift away from a conditioned reflex theory of learning to a fundamentally abstract definition of conditioning, albeit influenced by physiological assumptions.

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What was really going on at Bartlett's Cambridge University? Differing Perspectives

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The primary focus of this paper is to examine the climate of the Psychology department at Cambridge University during the 1940s, from differing perspectives. Ultimately the aim is to assess such statements as those made by Daniel Ellis Berlyne (1924-1976), referring to the work at the University as "that musty Cambridge stuff" (Berlyne, 1973, p. 91). I will address accounts made by Berlyne, reviewed and presented based on previously unexamined archival material. This material includes: a number of letters written to a life-long friend of Berlyne's from 1942 to 1950; over fifty interviews with friends, relatives and colleagues of Berlyne conducted by John J. Furedy from 1977 into the early 1980s; personal diaries and memoirs from Berlyne's school days; photographs; unpublished and published academic materials; as well as miscellaneous correspondence dealing with administrative matters related to academics, conferences, associations, organizations and affiliations etc.

Ultimately, Berlyne explicitly expressed discontent with the psychology at Cambridge, and felt that British scholarship was behind the times. Berlyne's thoughts are expressed in the following statement:

> ...because the type of psychology I was interested in was going on in the United States. We very much got the impression, which I think was accurate, that Psychology in the United States was 20 or 30 years ahead of British psychology. I don't just mean that as a value judgment. I mean they were doing things in Great Britain that they had done in the United States 20 or 30 years ago" (Berlyne, 1973, p. 114).

These thoughts will be presented in contrast to sentiments expressed by other students of the same time era.

Berlyne held his longest tenure at the University of Toronto from 1962 until his death at age fifty-two in 1976 however, his trans-Atlantic connection is clear. He received his early education in Britain, received his undergraduate training from Cambridge and did his early teaching at the Universities of St. Andrews and Aberdeen in Scotland, in 1948-1952 and 1953-1957, respectively. Berlyne was born in Salford, a city near Manchester, England. He attended Manchester Grammar School, during his primary school days and during his high school years attended Manchester Grammar. In 1941 at the age of 17, Berlyne went to study modern languages at Cambridge University. Commencement of his second year was interrupted in the fall of 1942, when Berlyne was called up for military service, and when he returned to Cambridge in 1946, he decided to switch from modern languages to psychology.

Although drawn to psychology, he was not really taken to the sort of psychology espoused by the field's representatives at Cambridge in the 1940's. During his undergraduate years, Berlyne wrote to a friend: "I feel skeptical and irritated by the limitations of

Cambridge psychologists". Such sentiments stand in stark contrast to those made by other students of Bartlett and who were affiliated with the Cambridge Psychology Department at this time. For example, Richard L. Gregory (1999) presents a different picture of the department, in which he describes the "Sir Fred's" department not as a limiting one, but as a highly energetic and creative place to be doing research. According to Gregory "it was possible to do just about anything in the Cambridge of that time" (Gregory, 1999, p. 806).

Berlyne's theoretical perspective can be understood in the context of his attraction to behaviorism and comparative research, neither of which much interested the Cambridge Laboratory under Bartlett. During his undergraduate years, Berlyne was taken by the promise of Hullian behaviorism, and he believed at the time that, "the Americans ...were using scientific method" to deal "with the vital questions" (Berlyne, 1973). Truth told: psychology at Cambridge under Sir Frederic Charles Bartlett (1886-1969), who was the school's first Professor of Experimental Psychology (1931- 1952), was somewhat "anti" American Behaviorism, of the Hullian genre which was occurring in America during the 1940s and 1950s. In particular, as expressed by Broadbent (1970), Bartlett was not a proponent for theoretical formalizations, as was Hull's endeavor.

Following World War I, Bartlett recognized that psychology needed to be harnessed to the solution of some very practical problems (Bartlett, 1936, 1937). Generally speaking, in Bartlett's department the primary focus was towards thinking and perception reflected in his most notable published works such as; *Remembering* (1932), *The Problem of Noise* (1934), and *Thinking* (1958), among others. So in large part it was focussed on remembering, memory, thinking, and errors in perception. However, Bartlett's work was largely impacted by both WWI and WW II. He was knighted in 1948 for services to the Royal Air Force, on the basis of his wartime work in applied psychology.

The work being done by the American behaviorists at Yale, under the direction of Hull, with its emphasis on systematic theory construction was regarded by affiliates of the "Cambridge school" as absurd and unfeasible (Furedy & Furedy, 1981). However, Berlyne did not concur. Given Berlyne's interest in working with animals and in motivation, during his time at Cambridge it is not surprising that he felt "bored" and discontent with Bartlett's psychology; a Hullian-inspired motivational theorist rightfully may have been apt to feel disconnected. Cambridge during the 1940s and 1950s was different, not necessarily "musty". It was not American behaviorism and it was geared at practical matters in the aftermath of WWI and WWW II, rather then on the construction of "grand" theories.

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12.30-1.30 LUNCH

Main Auditorium

Symposium: Female Psychologists in Britain, Continental Europe, and Japan, and "Territorial Segregation"	
1.30-2.00	Maria Sinatra (University of Bari), A Female Applied Psychologist: Franziska Baumgarten
2.00-2.30	Elizabeth Valentine (University of London), To Care or to Understand? British Early 20th c Female Psychologists
2.30-3.00	Miki Takasuna (Tokyo International University), Japanese Women Psychologists: the First Generation

Seminar Room 1

Making Up People: Genius, Insanity and Other Psychological Kinds	
1.30-2.00	Laura Ball (York University), Is it All Semantics? The Genius vs. Giftedness Debate
2.00-2.30	Kieran McNally (University College Dublin), The Image in Schizophrenia
2.30-3.00	Betty Bayer (Hobart and William Smith Colleges), Cognitive Dissonance: Reason over Revelation?

Seminar Room 2

Editors' Roundtable	
1.30-2.30	Jim Capshew (Indiana University), Chris Green (York University), Jim Good (University of Durham) and Hank Stam (University of Calgary)
2.30-3.00	Chris Green (York University), Podcasts and the History of Psychology

Main Auditorium
Symposium: Female psychologists in Britain, Continental Europe and Japan, and "Territorial Segregation"

Convenor: Elizabeth Valentine

This symposium brings together studies of lesser known female psychologists from Britain, Continental Europe and Japan, with particular reference to 'territorial segregation', i.e., the restriction of women to gender-stereotypical fields of work.

Paper 1 documents the life and work of Franziska Baumgarten (1882–1970), a Polish psychologist, who worked in Switzerland. One of the first women to work in applied psychology, her research focussed on social relationships in working life (the psychology of marketing and management), and characterology (for vocational guidance).

Paper 2 details 15 women who were members of the British Psychological Society before 1919 and 9 listed in the British Empire section of the 1929 edition of *The Psychological Register*. A very high proportion of them were employed as lecturers in teacher training colleges or university education departments.

Paper 3 presents four representatives of the first generation of female psychologists in Japan: Tsuruko Haraguchi (1886–1915), Tomi Kora (1896-1993), Sugi Mibai (1891-1969) and Tsuyako Kubo (1893-1969). Three of them undertook graduate training in the United States. Despite limited opportunities in higher education and employment, they carried out work in four different areas of psychology.

Paper 4 focuses on Dagmar Weinberg (1897–1946), a Russian-born German psychologist, who worked in France. Like Baumgarten, she was one of the very few women to devote her life to research in applied psychology, working with Henri Laugier on ergonomics, vocational guidance and 'biotypology'.

Paper 5 presents the work of Angiola Massucco Costa (1902–2001), an Italian psychologist, professor of experimental psychology at the University of Turin, where she opened a Specialization School in Social Psychology and Work Psychology, as well as an Orthophrenic School. A pioneer of Italian feminist emancipation during the 1950s and 1960s, she introduced Soviet psychology to Italy.

A female applied psychologist: Franziska Baumgarten

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University of Bari

Introducing the *Compte rendu* of the 9th International Congress on Psychotechnics, held in Berne in 1949, the Polish psychologist Franziska Baumgarten, at that time lecturer at the University of Berne, emphasized that applied psychological research should not only analyse social relationships in working life rather than attending to work processes and methods, but it should also focus on testing procedures assessing global personality and not any more on measuring the individual attitudes and abilities of human beings to carry out certain jobs. *The right man in the right place* was the key phrase of the period. In fact, two of her many books (most of which were translated into 14 languages) were titled *The Social Side of Psychotechnics* and *Psychology of Human Relations in Business*. In one passage she stressed how, according to the psychological study of professions, the popular distinction between higher and lower professions concerned their respective social levels, but did not indicate any

intrinsic difference. Baumgarten devoted her whole scientific activity to the defence of that idea without disregarding other fields.

This paper attempts to reconstruct Baumgarten's personality which despite her engagement in psychology, is still little explored nowadays.

She was born November 26, 1882, in Lodz, Poland, then part of Russia. Her father was an industrialist, manufacturing textiles. In 1905, she began university studies in philosophy, psychology, anthropology, and natural sciences at the Universities of Zurich, Paris, Kracow, Berlin, and Bonn. In Bonn she came into contact with O. Külpe. In 1908, she began doctoral studies in Zurich, completing her thesis in 1910 on *The Theory of Knowledge of Maine de Biran.* In 1910, in Berlin she happened to hear Hugo Münsterberg's lectures on industrial psychology and decided to specialize in that field. In 1919, she began lecturing on applied psychology at the University of Berne and in 1929 passed her professorial habilitation. She continued teaching there until 1954.

After getting married in 1924 to Moritz Tramer, a Swiss psychiatrist, she helped him in his research and published with him some writings on characterology. In this context, her investigations both on problems concerning adolescence and gifted children and on typology are worthy of mention. This research led her to postulate a method of cataloguing human characteristics drawn from biographies.

Being Jewish, she survived the Holocaust in Europe from the safety of Switzerland. Consequently, her work focused on psychological causes of war and on reactions to war experiences. She also documented women academics who had resisted the Nazis.

Baumgarten, however, paid particular attention to vocational guidance and selection, based on very careful and precise investigations of the structure of character. On this subject, the current paper focuses more upon Baumgarten's analyses of the psychology of salesmen and mechanical workers as well as of management principles and practices. It was not by chance that she had been busy with political psychology and trade-union organizations, a rare event for a woman at that time! The same can be said for her great interest in laboratory instruments: endowed with a background of deep personal experience in laboratory techniques, Baumgarten invented new devices to test aptitudes, such as instruments to measure hand trembling and to record hand movements, and practical intelligence tests. These instruments are carefully examined in my paper.

Besides teaching at the University of Berne, Baumgarten held various positions in the International Association of Psychotechnics and at the Bureau International du Travail. She died January 3, 1970. It is only now that Baumgarten's investigations are being fully appreciated.

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a. Baumgarten's main writings:

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To care or to understand? British early twentieth-century female psychologists

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This paper focuses on 16 women who were members of the British Psychological Society before the dramatic expansion in membership in 1919 (sample A) and 9 listed in the British Empire section of the 1929 edition of *The Psychological Register* (Sample B). Salient features of the sample are described and the form that territorial segregation took is discussed and contrasted with that shown in America during the same period.

The sample

Sixteen women are recorded as having been elected to membership of the British Psychological Society by the end of 1918, when the total membership stood at 98. Thus, women formed about 16% of the membership in the early days. When, after the expansion in membership, the first list of members was published in 1921, there were 203 (31%) women out of a total membership of 645. Ten women are listed in the British Empire section of the 1929 *Psychological Register*. The names and dates of these 26 women are given in an Appendix. Only three of Sample A also appear in the *Psychological Register*. It was possible to collect fairly comprehensive data for all except one (in Sample B), who was therefore dropped from the sample, leaving a total of 25.

All the women were born in the latter half of the nineteenth century. Most of them were from upper middle-class families often with privileged parentage; four of them were daughters of academics.

Sixteen remained single. Of the 9 who married, 4 had children.

Education, training and qualifications

The women frequently studied a number of subjects for their first degree, not necessarily including psychology. Ten of them studied at the University Cambridge at some point where degrees earned were not awarded to women until 1923. Two undertook studies abroad.

Almost all took some further qualification (e.g. a masters degree and/or a teaching diploma). Only six obtained only one qualification; the rest obtained 2, 3, 4 or 5.

Only 14 took doctorates. These were often achieved as long as 9 years after the first degree, sometimes at a relatively late age and/or for already published work.

Employment and career paths

Multiplicity and flexibility

Several of the women pursued a number of quite distinct careers. For example Dale worked in crystallography, psychology and educational administration. Saxby was successively a school teacher, education lecturer, psychologist, medical doctor and clinician. Many of them were teachers, researchers, clinicians and authors.

High proportion working as academics in education

Eleven spent a major part of their career as lecturers in teacher training colleges and/or university education departments, often in senior positions. In other cases, associations with teacher training colleges played a minor role, sometimes in the early part of a career or

following retirement from paid employment. Six published research on mental tests and three on the theory of mental abilities.

The remainder of the sample were also involved in education in some form or other, e.g., as school teachers, university lecturers in other subjects, psychotherapy trainers, educational administrators or school inspectors.

Other careers represented are child guidance, psychotherapy and industrial psychology. In 1921 there were twice as many women in the Educational and Medical Sections as in the Industrial Section of the British Psychological Society.

Publications

All of the sample published – in many cases extensively. Twenty-one of them wrote books. Several of them also wrote substantial numbers of journal articles. Creative arts were also among their products: novels, plays, poetry and music.

Conclusion

Cahan (1991) has documented gender-related territorial segregation – separate spheres of operation for men and women – within early American child psychology. Females predominantly occupied 'caring' practitioner roles, whereas males were predominantly in 'understanding' scientist roles. Although the relative distribution amongst educational, clinical and industrial psychologists in the present sample supports this distinction, the contrast between practitioner and scientist was not found. The women in this sample predominantly (though not exclusively) focussed on understanding rather than caring. They were typically employed as lecturers in teacher training colleges or university education departments. This underlines the key role that departments of education played in providing employment opportunities for women in higher education before the development of university departments of psychology in Britain.

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Appendix. Names and dates of members of Sample A (female members of the British Psychological Society before 1919) and Sample B (women listed in the British Empire section of the 1929 *Psychological Register*)

SAMPLE A	SAMPLE B
Laura Brackenbury (1868–1937)	Mary Collins (1895–1989)
Sophie Bryant (1850–1922)	Barbara Dale (1891–1986)
Nellie Carey (1886–1960)	Margaret Drummond (1871–1947)
Beatrice Edgell (1871–1948)[1]	Janet Lodge (1899–19??)[2]
Caroline Graveson (1874–1958)	Margaret McFarlane (1886–1962)
Victoria Hazlitt (1887–1932)[1]	Winifred Raphael (1898–1978)
Susan Isaacs (1885–1948)[1]	Mary Rivett (1896–1969)
Jessie Murray (1867–1920)	Mary Sturt (1896–1992)
Jane Reaney (1874–1936)	Olive Wheeler (1886–1963)
Ida Saxby (1883–19??)	Dorothy Wrinch (1894–1976)
May Smith (1879–1968)	
Mary Smith (1886-1974)	
Nina Taylor (1876-1951)	
Julia Turner (c. 1868-19??)	
Helen Verrall (1883-1959)	
Alice Woods (1849-1941)	

[1] Also listed in the *Psychological Register*
[2] Dropped from sample

Japanese women psychologists: The first generation

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The first generation of female psychologists in the United States has already been well described by Scarborough and Furumoto (1987). During the nineteenth century, the proliferation of social sciences -- including psychology -- emerged almost simultaneously, and women in the U.S. began to seek higher education; however, at this time in Japan, only men were allowed to enter a college or university.

Around the turn of the twentieth century in Japan, a few schools for women emerged with the word "college" attached to them (e.g., Japan Women's College, founded in 1901) but none of these schools was officially acknowledged as a college or university until 1947 when a law was enacted. During the Meiji era (1868-1912), possibly the highest level of education Japanese women could obtain was at one of the only two women's higher normal schools: Tokyo Women's Higher Normal School, which was originally established in 1874 as Women's Normal School, and Nara Women's Higher Normal School, which was set up in 1908. The students' goal would have been to become a teacher at a girls' high school.

I compiled a list of ten women psychologists already active during the pre-WWII period whose names appeared in Japanese psychological journals and *The biographical dictionary of Japanese psychologists* (Ohizumi, 2003) but I evaluated only four of them, all born in the nineteenth century, as being the first generation: Tsuruko Haraguchi, Tomi Kora, Sugi Mibai,

and Tsuyako Kubo. Three of these four ventured abroad to the U.S. for further education while in their twenties, earning PhDs before returning to Japan to teach psychology.

Tsuruko Haraguchi (née Arai, 1886-1915) was considered the first woman psychologist in Japan. She studied humanities and graduated from Japan Women's College in 1906. During her course of study, she attended psychology lectures presented by Matataro Matsumoto (1865-1943) and became interested in psychology (Ogino, 1983). Matsumoto was known for his positive attitude toward women's higher education. He encouraged Haraguchi to go abroad to the U.S. after graduation to obtain a doctorate, which she did in 1912, at the Teachers' College of Columbia University, under the direction of Edward L. Thorndike (1874-1949). Her study on mental fatigue was published in *Educational Psychology* (1913-1914, 3 vols.) by Thorndike. Although she was the very first Japanese woman to obtain a PhD in any field, her contributions were brief, as she died in 1915 at the age of 29.

During her funeral held at Japan Women's College, another young student, Tomi Kora (née Wada, 1896-1993), was impressed with Haraguchi's achievements and filled with regret for her early death, so she decided to follow in Haraguchi's path. Kora went to the U.S. in 1917, obtained an MA at Columbia University in 1920, then travelled to Johns Hopkins University for her PhD; her thesis was published in 1922 (Wada, 1922). Kora later became an assistant in the Department of Psychiatry at Kyushu Imperial University in Fukuoka. However, when her promotion to associate professor met with opposition because she was then unmarried and all of her students were single men, she became indignant and resigned the post. After her departure from Fukuoka, she became a professor in Tokyo at her alma mater. Following WWII, she was chosen to become a member of the House of Councilors in 1947, the very first election where women were allowed to vote and become candidates as men could. Kora participated as a Diet member for 12 years.

Because of time limitations, I will only briefly cover Mibai and Kubo. Sugi Mibai (1891-1969) graduated first from Baika Women's School in 1910 and then Kobe College (again, not authorized as a college but as a women's school) in 1915. She studied further at Mills College in the late 1910s and later obtained an MA and finally a PhD in experimental study (Mibai, 1931) at the University of Michigan. It was exceptional for a Japanese woman to go abroad three times while in her twenties to thirties.

Tsuyako Kubo (née Kurose, 1893-1969) graduated from Tohoku Imperial University in 1926 as the first woman graduate in the field of psychology. She was also distinguished from the three women who preceded her in that she was interested in developmental psychology.

Note that because women students rarely studied at national universities before WWII, all these pioneer women lectured in their field mainly at private colleges and universities. Despite a lack of access to the mainstream psychology profession before WWII, women psychologists in Japan continued to increase after WWII mainly after the college and university system was reformed in 1949.

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Seminar Room 1. Making Up People

Is it All Semantics? The Genius versus Giftedness Debate

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York University

The concept of *genius* has had a long history within academia and popular culture. The word *genius* comes from the Latin *gens*, which means family (Howe, 1999). Its roots can be traced back to the Roman Empire, where it referred to a male attendant, or guardian spirit (Murray, 1989). The female version was called a *juno* or *iuno*. From the earliest records, it appears that the *genius* was connected to the family and household (Murray, 1989). It was conceived of as a guardian spirit of an individual person, as well as a group of people, or a place (Murray, 1989). The spirit was thought to be external to the person or place, yet always connected to them (Howe, 1999). By the turn of the third century, BC, the relationship between the individual and their genius began to change, so that the genius, while still viewed as an external entity, was born alongside the individual and came to bear directly upon their personality (Murray, 1989). Roman custom was for each man to celebrate his genius on his birthday and provide his genius with offerings. Penelope Murray (1989) states that the modern practice of having cakes at birthday parties are the remnants of this tradition. The spiritual meaning of genius remained popular throughout the Middle Ages in Italy, England and France (Murray, 1989).

It is unclear when the word genius was first used in reference to a person, but it is thought to be sometime during the 16th century (Albert, 1969). At this time, the term was used to describe an individual's character, or personality. However, it is during the Enlightenment (18th century) that the meaning changed. Instead of describing an external spirit, its meaning was internalized, and came to denote certain unique abilities of an individual (Albert, 1969; Murray 1989).

In early psychological literature, genius was often used in connection to discussions of creativity, insanity, eminence and intelligence. The intelligence literature has often used the word genius as a synonym for giftedness. This was especially apparent in the works of Lewis M. Terman. When he unveiled the Stanford-Binet Intelligence Scale (1916), Terman defined an IQ of greater than 140 as "genius or near genius" (p. 79). Following this, he undertook a large-scale longitudinal study of gifted children who reached the *genius or near genius* distinction. This study was reported in the series *Genetic Studies of Genius* (1925, 1926, 1930, 1947, 1959). However, reaction to Terman's synonymous usage of the terms gifted and genius was swift and severe. Leaders in the field, most notably Leta S. Hollingworth (1926), argued that the two terms should not be confused, as giftedness is empirically and scientifically defined, whereas genius is not.

Adding to the complexity of the debate over terminology was the connotations of the word genius: many authors continued to retain the mystical connotations of the word in their writings (Albert, 1983; Lange-Eichbaum, 1931; Terman 1925). Wilhelm Lange-Eichbaum (1931) stated that there is always a religious sub-flavour to the term, even in scholarly work. Terman himself denounced the belief in the "Great Man, who has commonly been regarded by the masses as qualitatively set off from the rest of mankind, the product of supernatural causes, and moved by forces which are not to be explained by the natural laws of human

behaviour" (Terman, 1925, p. v). Due to this fact, many authors argued that genius has no place within serious psychological or scientific discussions (Hirsch, 1897; Hollingworth, 1926).

In the late 20th century, the debate over terminology resumed once again. Researchers argued that genius is an entirely separate phenomenon from giftedness (Albert, 1983; Jensen, 1996). Giftedness is meant to denote academic potential in children, whereas genius is reserved for those individuals who have attained eminence in their respective fields (Albert, 1969; Jensen, 1996). In addition, Arthur R. Jensen (1996) argued that giftedness was the result of high g-factor intelligence. In contrast, genius was likely the result of multiple factors applied synergistically. He appealed to Hans J. Eysenck's (1995) theory of genius as being the combination of creativity, intelligence and psychopathy (Jensen, 1996). Therefore, the use of the two words as synonyms impairs our ability to truly understand either phenomenon.

The paper will provide a brief outline of the etymology of the word *genius*, from its roots in the Roman Empire until the Enlightenment. The focus, however, will be on the use of the term in Psychology, the differences between genius and giftedness, and the debate over the use of the word genius within the discipline.

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The Image in Schizophrenia

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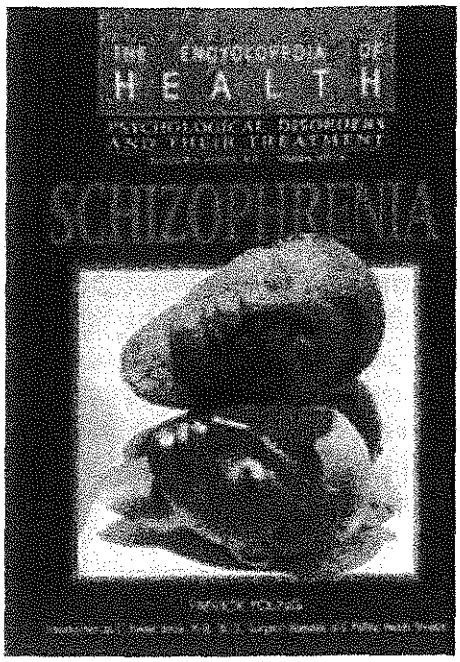
The history of schizophrenia exists in artefacts other than those which carry the written word. This paper examines the historical role of the image in the construction and maintenance of the concept of schizophrenia. It looks at and illustrates how theories of schizophrenia have been perpetuated through the production and transmission of the image. And it demonstrates how some such theories continue to persevere in images long after they have been considered disproved. This is of particular interest to historians of psychiatry, because the continued existence of such 'dead' theories is not without effect. They feed into the way in which people diagnosed with schizophrenia describe themselves, and they are encountered or enter the thoughts of psychiatrists who conceptualise the disorder; and as such loop back into the production of psychiatric discourse.

By way of illustration, the paper first discusses the myth of schizophrenia qua split personality, whose origins have elsewhere been traced to Eugene Bleuler (Mc Nally, 2007), the psychiatrist who first introduced the concept of schizophrenia into psychiatry. (Bleuler, 1911). By 1968 the split personality as an integral component of schizophrenia ceased to be acceptable in psychiatric thinking. From then on, orthodox psychiatric discourse through written transmission has, claimed (falsely) that the idea of the split personality is a public myth, originating and perpetuating in the public sphere. Officially the split personality now exists in the American Psychiatric Association, under the rubric Dissociative Identity Disorder (APA, 2004). This paper shows however, that from Bleuler onwards, the idea of the split personality continues to exist via the image, not just externally within wider society, where it is widely believed (Holzinger, Angermeyer, Matschinger, 1998), but within psychiatric communication -despite its now heretical content - until the present day. In addition to showing how the idea persists in the image (and even typography), the paper further explains how such counter-orthodox narrative has been produced and inserted into psychiatric communication naively by psychiatrists (through for example the interpretation of patient's mental images), and erroneously by the various publishers, editors and artists who facilitate psychiatric communication.



For example the cover of Patrick Young's *Schizophrenia (1988)*, shows a photograph of a sculpture where two heads are lying on their sides. One head is darker than the other, and because of its positioning, has dominance over the lighter head. As Barthes (1971), notes: the photograph is not simple a product of a channel of transmission, but also an object endowed with a structural autonomy. As such, irrespective of material contained within, the image transmits and allows for the interpretation of two personalities coexisting or struggling under schizophrenia. A historic array of such images carrying the split personality myth is provided, and are contrasted with more images from wider society, which carry a more explicit and emphatic variant of the split personality myth, through for example advertising and media sensationalism.

Such ideas are not without effect. The mythical idea of the split personality, can consequently be incorporated into the way patients then see and constitute themselves. And furthermore this process of self-identification can be unintentionally reinforced by psychiatry itself. For example the artist Rodger Casier (diagnosed as 'schizoaffective') produced a work entitled "Self-portrait", which shows a clear dividing line down the middle of his face and clearly evokes the concept of the split or divided personality. This image was subsequently selected to appear on the front cover of the journal *Schizophrenia Bulletin* (Casier, 2005).

Having examined the transmission of a single theme, this paper next highlights the existence of other images which help constitute other 'living' theories of schizophrenia. In particular this paper provides examples how the image has historically been and continues to be used, as a tool of theoretical propaganda, and erroneous pedagogic instruction. This occurs for example, through citation of the work of William Hogarth (*Scene in a Madhouse*), and Vincent Van Gogh (*Starry Night*) in order to retrospectively ensconce the concept of schizophrenia in the past. Theorising and producing evidence for the existence of schizophrenia in the past, though controversial, is not dead *per se*, and is still debated in psychiatric discourse. (Hare, 1998). But through the rhetoric of the image, such theories are supported by lower standards of evidence, than what is considered acceptable in historic, psychiatric and scientific discourse. Through such a process, they enter the pedagogic realm as 'acceptable' teaching material, masking the true history of psychiatry.

Finally, due to technological advances, and an increasingly visual culture, such images and associated theories are increasingly proliferating and gaining importance within

the schizophrenia literature. This paper therefore argues that for the producers and consumers of information pertaining to schizophrenia, and the historians who are attempting to understand the concept of schizophrenia, that the photograph or image in psychiatric literature, disserves as much, if not more, critical scrutiny than the accompanying text.

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Cognitive Dissonance: A Chronicle of Reason over Revelation?

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Today, the social psychological concept of cognitive dissonance seems to be ever-present. Used as a shorthand for contradictions in beliefs, 'cognitive dissonance' pops up almost daily in the news, interviews, web and blog sites, a musical band, and activist tactics, and is regularly invoked in scholarly works on everything from economics and political turmoil through to animal rights and consumer behavior. William Safire's 2007 predictions included one of Democrat's internal party struggle arising from "cognitive dissonance of anti-bias liberals" confronting their own biases; celebrity philanthropists' activist efforts are characterized elsewhere as "exploiting cognitive dissonance" by pairing in images and ads economically and socially distraught countries with feel good, apolitical solutions (e.g., buy this t-shirt, stop AIDS); foundations are said to suffer a basic cognitive dissonance between their "philanthropic arm and [their] investment arm" – "trying to heal the future, but with their investments, they steal from the future" (Peters, 2007); Bush's proposal to send 20,000 more troops to Iraq – the surge campaign – to "win" the war is at odds with those who want the war "over," leaving many Americans to suffer "cognitive dissonance by trying to believe these two positions can be held simultaneously" (Jonah Goldberg, 2007). What began in the 1950s as a social psychological concept transforms by the early twenty-first century into a sign of our time – an age of cognitive dissonance (Naomi Klein), or the age of dissonance, as Bob Morris calls his weekly column.

This paper argues that the chronicles of cognitive dissonance offer a window onto a larger history of reason over revelation in questions of social, moral and political order. Quite apart from a story of a concept's popular growth and spread, the history of cognitive dissonance's diffusion unfolds as one of humanity's search for the holy grail of rationality itself: How do we know who or what to believe? How deeply troubling are conflicting

views? How does inner anxiety over contradictory beliefs reflect broader social concerns? Does cognitive dissonance reflect that older debate over which comes first – what we know (epistemology) or what exists (ontology)? 'Cognitive dissonance' may be considered a conceptual Rorschach test of a deep-seated cultural fear of the cracks in the meaning of life as a rational endeavor. Its emergence in the 1950s, along with popular works such as Norman Vincent Peale's (1952) "The Power of Positive Thinking," Eric Hoffer's (1951) "True Believer," and David Riesman's (1950) "The Lonely Crowd," signaled new versions of the psychological and of the religious. As some scholars argue, these new versions were part and parcel of an epochal turn toward "subjective-life forms of the sacred…emphasiz[ing] inner sources of significance and authority and the cultivation or sacralization of unique subject-lives" (Heelas & Woodhead). This shift redrew intellectual lines between religion and psychology around consciousness, identity and freedom, on the one hand, and fear, anxiety, despair, sin and questions of "what the universe was going to mean for [one]…what to affirm as [one's] truth and meaning for oneself" (Ellwood; Homans).

'Cognitive dissonance' was first introduced in the 1956 book "When Prophecy Fails." Imagined as a tension that arises when two opposing beliefs are held simultaneously, the first "real" test of the idea was carried out by covert study of a small apocalyptic group, the Seekers, led by Mrs. Keech, a pseudonym for Dorothy Martin. It quickly caught on within social psychology experimental laboratory studies and as quickly became a focal interest amongst religious studies scholars; it soon became the subject of fiction and a television dramatization, and entered, over the years, into fields as widely disparate as philosophy of science, economics, political science, legal theory, sociology, not to mention a variety of religious sects and cults (including ufology)

This history of cognitive dissonance contrasts with those aimed at showing the discipline's declining interest in the concept itself. For what such treatments fail to take into account is the dispersion of the concept beyond social psychology. The life course of cognitive dissonance from its introduction in the 1956 book *When Prophecy Fails* to its uptake in a vast array of disciplines and to its current everyday use reveal the story of this concept as more than one of a catchy shorthand or a short-lived bandwagon effect of experimental social psychology. Using this broader historical framework, the chronicles of cognitive dissonance assume quite a different place, one that makes the concept as the book from which it emerged a pivotal node as much in the history of what Barbara Herrnstein Smith refers to as naturalizing the sacred in cognitive psychology as in sacralizing psychological cognition in religion and spirituality.

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Seminar Room 2

Editors' Roundtable

Jim Capshew (Indiana University), Chris Green (York University), Jim Good (University of Durham) and Hank Stam (University of Calgary)

In this session, editors of some of the major journals that concentrate on historical and philosophical issues in the human and behavioral sciences will share their experiences of and offer some reflections on the process of publication.

Podcasts and the History of Psychology

Christopher D. Green
York University

Over the past several years, I have concentrated on bringing the latest computer technologies – especially internet-based formats – to the scholarly study of the history of psychology. These efforts have included, among other things, the "Classics in the History of Psychology" website, the "History & Theory of the Psychology Question & Answer Forum," the "History & Theory of Psychology Eprint Archive," and two video documentaries on topics in the history of psychology.

In the same vein, over the past year I have been producing and distributing a weekly podcast series entitled "This Week in the History of Psychology." In addition to brief descriptions of a number of historically-significant events that occurred during that week in psychology's past, each of the 30 episodes features an interview with a working historian of psychology about a topic on which s/he has published. For example, among the featured interviews have been Raymond Fancher on Freud's 1909 lectures at Clark University, Michael Sokal on James McKeen Cattell's dismissal from Columbia University in 1917, David Baker on the impact of David Shakow's 1947 report to the APA on the training of clinical psychologists, David Robinson on Wilhelm Wundt's arrival at the University of Leipzig, and William Tucker on the controversy that surrounded the nomination of Raymond B. Cattell for the APA's Gold Medal award. Forthcoming in 2007 will be interviews with Eric Engstrom on the origins of Emil Kraepelin's typology of psychopathology, with Stanley Finger on the life and career of Paul Broca, with Dorothy Ross on the career of G. Stanley Hall, and with Ludy Benjamin on reasons behind the splitting of the American Psychological Society from American Psychological Association. (A full list can be found at http://www.yorku.ca/christo/podcasts/.

The aim of the podcast series is threefold: (1) to bring to students in history of psychology courses some basic informational content that rises above the material typically contained in the textbooks they are assigned, (2) to introduce students to important historiographic issues that are often not extensively covered in their textbooks (e.g., the relationship of psychological developments to broader socio-cultural trends), and (3) to

demonstrate to students that the history of psychology is a "living" discipline by "virtually" introducing them to researchers currently working in the area and by actively exploring some of the current debates and controversies in the field. Further, it was hoped that by couching all of this in an easy-to-use and popular format such as a podcast, it would be made more palatable, even enjoyable, to a student audience. The "flip side" of this was the hope that instructors of history of psychology courses would be made more aware and more appreciative of the pedagogical possibilities of popular internet-based formats such as the podcast.

My intention, in the presentation proposed here, is (1) to sketch the background to my decision to produce the "This Week in the History of Psychology" podcast series in the first place, (2) to describe some of the technical matters that enable a production of this sort (by way of encouraging others to develop related projects), (3) present some of the highlights of the podcast series (e.g., Who was interviewed about what topics? What was surprising? What worked? What did not?), and (4) discuss the use of the podcast series by the academic community (e.g., What was the reception? How widely was it listened to? Who used it? With what intentions? How did it work as a supplementary course material?).

3.00-3.30 TEA

Main Auditorium

Symposium: Female Psychologists (cont'd)	
3.30-4.00	Angela de Leo (University of Bari), Dagmar Weinberg: a Woman Psychologist in French Applied Psychology Laboratories
4.00-4.30	Lucia Monacis (University of Bari), Angiola Massucco Costa: a Female Pioneer in Italian Psychology
4.30-5.00	Discussant: Elizabeth Scarborough (Indiana University-South Bend)

Seminar Room 1

Comparative Psychology & Neuroscience	
3.30-4.00	Cecelia Watson (University of Chicago), Man's Best Foil: Dogs in the Work of William James
4.00-4.30	Gabriel Ruiz & Natividad Sanchez (University of Seville), No Single Measure Represents the Whole Picture: Gantt and the Study of Nervous Imbalance
4.30-5.00	Michael Pettit (University of Toronto), Even the Rat was Queer: Behavioral Endocrinology and the History of Sexuality
5.00-5.30	Katherine Harper (York University), Alexander Bain's *Mind and Body*: an Early Neural Network Model

Seminar Room 2

Interdisciplinarity and Big Social Science	
3.30-4.00	Rebecca Lemov (Harvard University), Five Cultures and the Fate of Big Social Science: a Parable of the Perils of Seeking Out Other People
4.00-4.30	Larry Nichols (West Virginia University), Social Relations in Retrospect: Explaining the Demise of Harvard's Interdisciplinary Department
4.30-5.00	Ryan Tweney (Bowling Green State University), Egon Brunswik and the Search for Unity in the Sciences
5.00-5.30	Leila Zenderland (California State University-Fullerton), Reassessing Yale's Seminar on the Impact of Culture on Personality: Intertwining Local and Global

Main Auditorium. Symposium on Female Psychologists, continued

Dagmar Weinberg: A woman psychologist in French applied psychology laboratories

Angela De Leo
University of Bari

In 1938 the Russian-born psychologist Dagmar Weinberg observed that, in order to distinguish between "bad" and "good" workers, it was necessary to consider a biotypological description of their personality.

With these words she defined her position in the context of mid-twentieth century French psychology, when the applied psychological model of selection, centered upon the scientific study of aptitudes, was turning toward the study of personality.

The current paper aims to reconstruct Weinberg's scientific career, which is still very little investigated. Having attended both the Sorbonne laboratory and the École Pratique des Hautes Etudes, she was able to clarify her position, which developed, despite their common purpose stated above, into two different research groups: those of Henri Piéron and Jean-Marie Lahy.

Dagmar Weinberg was born in Russia, in 1897. After a stay in Germany, she moved early in 1920 to Piéron's institute in Paris, working with Lahy.

As Piéron wrote, the first psychological institute was established in Paris in 1920 under his direction. He devoted it to specialized training in psychophysiology and psychotechnics, carrying out practical work at the Sorbonne laboratory as well as a laboratory for applied psychology opened in the École Pratique des Hautes Etudes in the Sainte Anne asylum. The latter laboratory was directed by Lahy from 1925.

Weinberg continued to work at the Sorbonne laboratory with Piéron until joining Henri Laugier as his assistant at the Conservatoire National des Arts et Métiers, where he had been appointed to a chair in 1929.

Piéron's group worked on the psychophysiology of the senses following the strong French sensationist philosophical tradition. Among Piéron's collaborators was Laugier, who also directed the laboratory for applied physiology at the Hospital Henri Rousselle. The group focused on a physiological approach to aptitudes and on applied psychological research. Piéron made several significant contributions to this field, among which was the creation of the Institut Nationale d'Orientation Professionnelle in Paris in 1928. Piéron's influence can be observed in some of Weinberg's writings on the selection of welders, mine and dye

workers, based on the analysis of psychophysiological phenomena, such as the effects of variations in temperature and pressure.

Weinberg's contacts with both Lahy and Laugier lasted until 1946, the date of her death, placing her in a central, link position between the two of Piéron's sub-groups. Furthermore, Laugier and Weinberg, as Jews, both suffered intense persecution during the war.

In the first years spent at the Sorbonne, Weinberg was employed in compiling bibliographies for journals, on account of her knowledge of several foreign languages. But thanks to her skills in mathematics and statistics she was soon employed in the fields of applied psychology and biometry. In fact, she was promoted *chef de travaux*, becoming assistant director of the Biometrics Laboratory at Viroflay, founded in 1936 by Laugier, who carried out government-sponsored research until 1940.

In this context, following F. Galton's idea of classifying human beings in order to improve future mating and procreation, Weinberg developed a standard biotypological form to record information about individuals in all studies and to provide a basis for comparison. The aim was the construction of a biotypological profile for each individual. In previous years, with her colleagues she had carried out a series of investigations for the Institut National d'Orientation Professionelle, concerning the professional goals of school children and a pioneering study on 4,800 students at the Faculté des sciences, in order to compare differences in science grades between male and female students.

A final research project of Weinberg's to be examined in the paper, which was sponsored by the Carnegie Corporation, concerned the comparison of biotypological profiles of 650 children aged 9-11 according to the standard of living of their parents.

These statistical analyses continued until 1946.

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Angiola Massucco Costa: A female pioneer in Italian psychology

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The purpose of this paper is to provide an epistemological reconstruction of the career of one of the first Italian female pioneers in psychology: Angiola Massucco Costa.

Born in 1902 in Brescia she came from a well known family in Biella and grew up in the post first world war economic context, which was dominated by a Tayloristic approach to working life and by a consequent psychological model of man at work without any interpersonal dimensions. As a matter of fact the dominant ideas were man's subordination to machine and the improvement of working efficiency. It was also a period when psychological devices were constructed in order to test human abilities and to select personnel.

Massucco Costa studied and graduated in philosophy at Turin University. Meanwhile one of W. Wundt's pupils, Friedrich Kiesow, who concentrated his early research on fatigue, introducing experimental methods from physiology and the natural sciences into psychology, taught psychology at Turin University, influencing his scholars with a Wundtian psycho-physical system and also with an aversion to the various contemporary psychological approaches, such as Gestalt and behaviouristic theories.

As a young woman, Costa taught in several high schools and in 1934 became assistant to Kiesow, completing her scientific education first in Geneva, dominated in those years by Piagetian genetic psychology, and then in Berlin, where she studied in several laboratories.

After the 7th National Congress of Experimental Psychology and Psychotechnics held in Turin in 1929, the second generation of Kiesow's scholars, including Massucco Costa, carried out research in psychotechnics and experimental psychology, in particular visual perception. At the Institute of Psychology, in collaboration with another of Kiesow's pupils, Alessandro Gatti, she developed innovative research in work psychology. From 1938 to 1942 Costa edited the review "Archivio Italiano di psicologia generale e del lavoro", founded by Kiesow and then headed by Gatti. Until she was in Turin, she obtained only temporary work at the university. Her academic career began officially in 1957, teaching psychology at Cagliari University. Some years later she became dean of the Faculty of Education. At that time she was involved in scientific and educational fields, as well as devoting herself to social and political work.

As a member of both the Italian Communist Party and Parliament between 1963 and 1968, she paid a lot of attention to women's issues regarding maternity and childhood, topics

of which she already had considerable experience during the 1950s. As she had witnessed personally the conditions of housewives and working mothers in the most depressed areas of Sardinia, she complained about the absence of children's gardens and counselling centres as well as the lack of sanitary conditions in work places. As a pioneer of feminist emancipation she protested against the current bad working conditions.

Back in Turin in 1964, she became full professor of experimental psychology at the Faculty of Magistero. Moreover, she founded and headed the Institute of Experimental and Social Psychology, focussing her research on psycho-social factors. After receiving teaching in Social Psychology, she founded the "Rivista di Psicologia Sociale". During this period she proposed the establishment of a Higher School of Psychology, which was the first attempt at professionalization for psychologists in Italy.

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Seminar Room 1. Comparative Psychology and Neuroscience

Man's best foil: Dogs in the Work of William James

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William James took great interest in animals' mental life and behavior. It is not surprising that a psychologist schooled in Darwin and well-acquainted with the laboratory would consider animal life important, and readers are therefore not shocked to see that the *Principles of Psychology* makes frequent reference to non-human species. James calls on animals ranging from mollusks to monkeys to draw comparisons with human behavior, citing animal activities in which hints of human activity can be perceived.

But these mentions of animals are more than applications of Darwin's theories; James's keen observations of animals led him to scientific, personal, religious and political insights that are articulated throughout James's writings. The role of animals in the formation of James's thought has thus far been underappreciated by historians of science: although James's interest in animal rights has received attention from scholars, no one has analyzed James's use of animals in his psychological and philosophical writings.

This paper explores this aspect of James's thought by focusing particularly on a species with which James was especially well-acquainted: the dog. James owned Scottish Terriers, collected anecdotes about dogs, attended dog shows, and copied dog scenes from the paintings of Delacroix. Each of these interactions with dogs, I argue, contributed to the development of James's psychology, his political views, and his spirituality. James believed that dogs had a "world" of their own, in which the sensible elements of the world that humans notice either go unperceived or have a different meaning for the dog. This insight about the worlds of animals was first articulated in the *Principles:* a dog visiting the Louvre, says James, perceives a world of smells lingering at the bases of sculptures, whereas a human in the same gallery has as little knowledge of the meaning of smells as the dog has of the meaning of art. In this passage and others, James uses the dog to illustrate the interplay between evolution, perception and attention.

By the time James delivered his lectures on Pragmatism, the world of the dog had taken on greater meaning and importance for James. "I believe rather," wrote James,

> that we stand in much the same relation to the whole of the universe as our canine and feline pets do to the whole of human life. They inhabit our drawing-rooms and libraries. They take part in scenes of whose significance they have no inkling. They are merely tangent to curves of history the beginnings and ends of which pass wholly beyond their ken. So we are tangent to the wider life of things. But, just as many of the dog's and cat's ideals coincide with our ideals, and the dogs and cats have daily living proof of the fact, so we may well believe, on the proofs that religious experience affords, that higher powers exist and are at work to save the world on ideal lines similar to our own. (*Pragmatism,* p. 300)

For James, the dog's world, when compared to the human's, provided a way to understand, and allow for, religious experience and a benevolent God.

My paper then considers the ways in which James's acquaintance with dogs informed his political views beyond his opinions on animal experimentation. James kept a folder of newspaper clippings recording abuses of dogs and other animals, and, in an article on lynch mobs, he speculated that lynching might be an outgrowth of the instinct to do violence to animals. James was not implying that the victims of lynch mobs were like animals; rather he was using the analogy to call attention to the psychological aspects of lynch mob behavior; he called lynching the next psychological step of our instinct to murder—an instinct he identified through his study of human-animal interaction.

My paper concludes by setting James's ideas about dogs and other animals in the context of animal and human psychology in the late 19th and early 20th centuries.

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No Single Measure Represents the Whole Picture: William Horsley Gantt and the Study of Nervous Imbalance

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Although scientists and clinicians have widely recognized the importance of experimental animal studies in the origin of North American psychosomatic research, historians of Psychology, Psychiatry, and Medicine have barely paid attention to the significance of these studies in the making of this field. In this paper, we have used archival materials to present a case study, that of contributions of William Horsley Gantt to Psychosomatic Medicine.

William Horsley Gantt became acquainted with Pavlovian methodology after a long stay (1925-1929) at I. P. Pavlov's laboratory in Leningrad. Gantt arrived at Johns Hopkins in 1929, as result of Meyer's invitation to make "your training available in our research and practical work at the Henry Phipps Psychiatric Clinic" [1]. Soon after his arrival at Phipps Clinic, Gantt founded and directed the Pavlovian Laboratory from 1930 to 1964. This research facility was continuously and exclusively funded by the Rockefeller Foundation until 1946[1].

In this laboratory Gantt and a group of collaborators[1] began a complex, Pavlovian research program that included the investigation of nervous disturbances in dogs. These studies were carried in the Pavlovian Laboratory of Phipps Clinic from 1931 to 1943. One year later, in 1944, Gantt gathered all these works in his monograph *Experimental Basis for Neurotic Behavior. Origin and Development of Artificially Produced Disturbances in Dogs*[1], a book published with the sponsorship of the American Society for Research in Psychosomatic Medicine.

Scientists in various specialties, neurologists, internists, psychiatrists, psychoanalysts and psychologists, responded favorably to Gantt's book, reviewing to it positively in their journals; Stanley Cobb, a well-know neurologist at Harvard, wrote the reviews for *American Journal of Psychiatry*[1] and *Archives of Internal Medicine*[1], and emphasized in both cases Gantt's demonstration of the importance of the individual differences. The question of types was also highlighted in other medical journals including *Journal of the American Medical Association*,[1] *British Medical Journal*[1], and such clinically oriented journals as the *Bulletin of the Johns Hopkins Hospital*[1], *Psychosomatic Medicine*[1], *Journal of Orthopsychiatry*[1], *Archives of Neurology and Psychiatry*[1], *The Psychoanalytic Quarterly*[1], and the *Journal of Consulting Psychology*[1].

In 1946, Gantt was awarded the Lasker Prize in Mental Hygiene "For Experimental Investigation into Behavior Deviation"[1]. As the citation noted, Gantt's book was "timely when psychiatric education is shifting its focus toward neurosis, when general medicine is beginning to develop a responsibility for the vast neurotic patient population whose ills do not come within the framework of traditional medical science, and when the pressures of a war have forced a higher public consciousness of neurotic disability"[1].

Why was a book on 'neurotic' dogs so significant? Gantt did not discover the phenomenon of "experimental neurosis", which had long been well known. Departing from the pioneering studies of I. P. Pavlov's Laboratory at Leningrad[1], many American

psychologists and psychiatrists had published about this topic before Gantt's book was widely known and valued[1].

To understand the importance of Gantt dog studies, we must take into account several factors concerning Gantt's scientific approach: 1) Gantt's methodological innovations –his novel combination of the Pavlov's physiological technique of conditional reflexes with Adolf Meyer's psychiatric method of writing the "life-chart" of an individual; 2) Gantt's concentration upon single individuals, which made his book recognizable and interesting to physicians, psychologists, and psychiatrists who also worked on individuals; 3) Gantt's interest not only in the so-called specific components of these reflexes –those most related with the unconditional stimulus in which they were based, i.e. salivation with food-, but also in the more general, non-specific supporting components of these reactions, e.g. cardiac or respiratory.

However, Gantt's methodological innovations are not enough to fully appreciate why Gantt's book crossed the boundaries of the hermetic Pavlovian psychiatry to find a place on the bookshelves of practicioners. We must also to take into consideration the social context in which he worked, a context imbued with the widespread influence of anti-reductionist and holistic movements in American and European Medicine of the 1920s and 1930s and the concern with social instability produced by industrialization; by World War I; by Depression; and by the rise of European fascism[1]. In this context, studies dealing with the problem of emotional instability and its treatment or prevention attracted a special attention. Gantt's book fully accomplished these expectations, and captured the interest of the new emerging field of Psychosomatic Medicine.

For that reason, the widespread resonance of Gantt's *Experimental Basis for Neurotic Behavior* should not be understood as a simple step forward within the boundaries of Pavlovian tradition but as the expression of the converging influences of the laboratory traditions with the more holistic approaches, in a moment, the World War II, of marked social instability and deeply concerns about how to face the emotional disturbances.

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Even the Rat was Queer:
Behavioral Endocrinology and the History of Sexuality

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This paper argues that laboratory rat ought to be understood as an agent in the history of sexuality and the sexual sciences. It takes as its starting point a series of reports that emerged from psychology laboratories from the 1920s to the 1940s detailing instances of "reversals" in sexual behavior within their rat colonies (Stone, 1924; Jenkins, 1928; Beach, 1938; Ball, 1940). From these not so chance glances, Frank A. Beach developed novel models for the

'nature' of sexuality, stressing that animals had the neurological capacity to perform behavior of either sex and the expression of exclusive hetero- or homosexuality was the product of external stimuli and social influence (Beach, 1941, 1942, 1948). Beach enrolled his new specialty, behavioral endocrinology, in support of Alfred Kinsey's controversial findings (Kinsey, 1948). For both Beach and Kinsey, the search for a biological foundation for sexual 'deviancy' was futile for all organisms functioned within a plenum of behaviors. Such a concurrence was critically important for Kinsey, who recognized that endocrinology was the science most likely to oppose his vision of the malleability of human sexuality (Kinsey, 1941).

That Beach, one of arch biological determinist Karl Lashley's students, came to stress environmental stimuli is all the more remarkable considering his research program integrated neuropsychology, that stressed the primacy of the innate organization of the nervous system in determining behavior (Weidman, 1999), and endocrinology, which emphasized the glands domination over the body (Sengoota, 1998; Fausto-Sterling, 2000). Because homosexual behavior could be observed in the everyday world of the colony and induced with hormonal treatments, Beach concluded that the innate hardwiring of the nervous system was for bisexual activity. That humans and other animals express sexuality along certain normative lines was the product of culture and society, not nature (Ford and Beach, 1951). Yet Beach articulation of a nurture account of sexuality was only thinkable by affirming his mentor's research program. Sex was merely a kind of behavior, without introspective content, that could be disaggregated into discrete and observable stages. Beach's selection of the rat as an object of study, his behaviorist epistemology, and the architectural arrangements of his laboratory all contributed to his reinterpretation of sexual norms.

A study of the resources Beach managed in order to craft behavioral endocrinology has significance beyond reconstructing the early history of an important branch of twentieth-century biology. Beach's laboratory practice illuminates the historical epistemology of the sciences of nurture. He adhered to Lashley's orthodoxy that whatever was programmed into the nervous system was the natural state of things. For this reason, behavioral endocrinology provides a fruitful arena for thinking through how the methods of the sciences of nature differ from and yet remain connected to the sciences of nurture. While Steven Pinker asserts that claims of humanity malleability are merely the artifact of bad liberal ideology (Pinker, 2002), the work of Beach and his contemporaries offers a window into the material culture, observational practices, and yes political commitments that went into making liberal claims about the centrality of external influences in the formation of subjectivity. With the exception of behaviorism, nurture paradigms tend to be associated with field sciences like Boasian anthropology or social surveys, but Beach's work illuminates how the resources of supposedly deterministic life sciences can deployed towards these ends.

Furthermore, I would argue that the careful observation of rodent sexual behavior in mid-twentieth-century psychology laboratories was an important episode in the historical ontology of the sex/gender distinction. While none of the actors I discuss explicitly used the concept of gender prior to John Money's influential formulation in the 1960s, laboratory observation and experimentation provided critical levers for the decoupling of anatomical sex from gendered behavior. Although the history of sexuality and its sciences has unsurprisingly focused on human subjects, the work of Beach and his contemporaries was not understood as being apart from that of Kinsey and others. A consideration of this topic will augment the coalescing narrative of shifting sexual regimes in U.S. culture (Terry, 1999; Meyerowitz, 2002; Serlin, 2004).

In making such claims, this work interprets behavioral endocrinology using the tools of that branch of science studies that stresses the importance of material culture and the laboratory practices. I am interested in elucidating how the "second nature" (Cronon, 1991)

of the laboratory's rat colony with its organisms, apparatus, practices, and rituals served an integral function in the redefinition of 'sex' in the twentieth-century. In doing so, I contribute to the historiography of the life sciences' use of standardized organisms as tools by examining what happens when such tools do not operate in an expected, standardized fashion (cf. Clarke and Fujimora, 1992; Kohler, 1994; Rader, 2004).

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Alexander Bain's *Mind and Body* (1873) as an Early Neural Network Model

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This paper explores a previously little appreciated aspect of the psychology of Alexander Bain (1818-1903) – namely his postulation in the 1873 *Mind and Body, The Theories of their Relation*, of something that anticipates the modern conception of the neural net. Bain, of course, is well known for founding the first psychological journal *Mind*, and for *The Senses and the Intellect* (1855) and *The Emotions and the Will* (1859), which became standard psychology texts used in Britain for nearly fifty years (Boring, 1950).

Bain's psychology always emphasized physiology and associationism. He cited the associative theories of Locke, Hume, and Hartley, and was also highly influenced by his friend and colleague John Stuart Mill (1806-1883) who strongly supported his integration of physiology and associationism. Bain began his *Senses and the Intellect* with a chapter on anatomy and the functions of the nerves, followed by chapters on the five senses, the appetites, movement, instincts, intellect, and the emotions. His aim was to discuss both the physical and mental aspects of each topic. Among the first to include a physiological chapter in his psychology texts, Bain set a precedent for how psychology texts were written thereafter and is considered to be one of the first physiological psychologists (Boring, 1950; Murphy, 1949, Young, 1970).

Although Bain's early work covered the anatomy of the nervous system, much of this was not coordinated with the later psychological chapters (Boring, 1940). In *Mind and Body* (1873), however, he more explicitly attempted the integration physiology and psychology by

conceiving a neurological network that could explain learning, memory, and consciousness. The physiological aspects of Bain's early work was highly influenced by anatomists and physiologists, Richard Quain (1800-1887), William Sharpey (1802-1880), William Carpenter (1813-1885), Johannes Mueller (1801-1858) and Charles Bell (1774-1842) (Bain, 1904). And further, the 1864 work of clinical pathologist Dr. Lionel Beale (1828-1906) inspired Bain's neural network model and particularly distinguished *Mind and Body* (1873) from his earlier texts.

In this work Bain hypothesized about the physiological correlates of memory and the association of ideas stating that, "For every act of memory, every exercise of bodily aptitude, every habit, recollection, train of ideas, there is a specific grouping, or co-ordination of sensations and movements, by virtue of specific growths in the cell junctions" (p. 91). Bain further argued that two primary conditions are necessary to explain distinct associations, sensations or movements: unique neuronal groupings and unequal strengths of stimulation. Bain provided two diagrams to illustrate this, one of the neuronal connections in the case of difference of groupings and another showing how a neural network with varying intensities would work. Generally, Bain provided a theory of memory and learning, showing how multiple associations are developed and stored while also proposing a theory of consciousness founded on his three general laws of alliance of mind and body; relativity, diffusion, and pleasure/pain.

Many histories of neural networks begin with the associative writings of Aristotle and jump to the 1949 work of Donald O. Hebb (1904-1985), with little or no consideration given to Bain (Quinlan, 1991; Valentine, 1989). Wilkes and Wade (1997), however, appear to be among the first to recognize Bain's *Mind and Body* in this context. They explain Bain's theory of neural groupings and emphasize his neurological theory of associative memory. They also see similarities between the work of Donald O. Hebb (1904-1985) and Bain and suggest that Bain anticipated some of the ideas that grew out of the neuron doctrine. They quote Hebb (1949) who stated, "When an axon of cell A is near enough to excite a cell B and repeatedly or persistently takes part in firing it, some growth process or metabolic change takes place in one or both cells such that A's efficiency, as one of the cells firing B, is increased" and then go on to compare Bain who stated, "I can suppose that, at first, each one of the circuits would affect all others indiscriminately; but that in consequence of two of them being independently made active at the same moment...a strengthened connexion or diminished obstruction would arise between these two..." (p. 302). This paper will build on Wilkes and Wade by examining Bain's Laws of Relativity and Diffusion, in association with his motor theory, which forms the foundation of his neurological theory of consciousness. Bain (1873) states, "It is by combining the two laws - Relativity and Diffusion- that we obtain the comprehensive statement of the physical conditions of all consciousness: *An increase or variation of the nerve-currents of the brain sufficiently energetic and diffused to affect the combined system of the out-carrying nerves (both motor nerves and nerves of the viscera*) (p. 57 italics in original). In addition, Bain (1873) states, "the union of different stimulations in different fibres and in different degrees, would unavoidably give birth to a complex and modified consciousness" (p. 86). Thus, Bain's theory suggests that unique neuronal groupings and unequal strengths of stimulation are not only the foundation of the laying down of associative memories, as Wilkes and Wade (1997) suggest, but are also the mechanisms that activate the motor system, which in turn gives rise to consciousness. For Bain, consciousness was possible only when the energy to motor components was reduced in force. Furthermore, the recall of an idea was dependent on excitation of the motor element.

In sum, this paper will expand upon Wilkes and Wade (1997) by exploring two aspects of Bain's work that have not been previously considered: his neural network theory of consciousness and the relevance of the motor system in activating consciousness. Further, it

will briefly discuss some of the key figures that influenced the development of this work, particularly Lionel Beale, and will consider some of the reasons Bain's neurological theory garnered little attention at the time of its publication.

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Seminar Room 2. Interdisciplinarity and Big Social Science

Five Cultures & the Fate of Big Social Science: A Parable of the Perils of Seeking Out Other People

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In the summer of 1949, anthropologists and other social scientists from Harvard University began a collective project to turn a remote area of the Western American desert known as "Ramah" into a veritable laboratory. As a spot where five different cultures happened to settle in proximity to each other--Zuni, Mormon, Anglo-Texan, Navaho, and Spanish-American--the area seemed to offer a controlled research situation, a kind of natural accident of cultural configuration that would enable intensive study. A "field laboratory," the eminent Alfred Kroeber declared it, "an excellent social science laboratory," others at the Laboratory of Social Relations dubbed it, while Harvard's Board of Overseers upgraded it to an "ideal laboratory." Eventually, over 60 investigators from various behaviorally-oriented sciences were dispatched to the New Mexican desert, hitherto distinguished primarily as the Pinto Bean Capital of the World. Their task was to study, isolate, and eventually render in scientifically adequate terms the most elusive part of culture: human *values.*

Imagine, then, this scene: five Ramah communities going about their business, living their lives, only to find themselves one summer saturated with scores of busy Harvard researchers. Some wanted to measure their subjects' "deviance" by showing them pictures of dogs in untoward predicaments and asking them to interpret. Some projected slides of circles with small openings from a 110v. AC-DC tachistiscope to ascertain the amount of "closure" performed, in an attempt to test whether perception is universal. Some asked seemingly intrusive questions about sex and dismemberment. Some used "a visiting dentist to produce anxiety in the experimental design" and measure that anxiety among children, hoping to arrive at "a comparison of the way in which individuals in the five cultures react to the threat of physical pain." Some were there to investigate the phenomenon of the henpecked husband. Some employed the Thematic Apperception Test to measure "self-hate" among the Navaho "[a]s a result of multiple-frustration at the hands of white-dominators," so as to get at "a final stratification of the population." Then there was Victor Goldkind's Nausea Project, the details of which, perhaps fortunately, remain vague. Many researchers were graduate students at work on Ph.D. theses such as "A Study of Husband-Wife Interaction in Three Cultures" or "Patterns of Humor in Relation to Values" or "Certain Aspects of a Study of Changing Navaho Religious Values with Special Reference to Survey-Sampling Techniques."

> Scientific investigators sought to establish rapport with their subjects but often found it to be in diminishing supply; after three years, in 1951, research had to be temporarily suspended due to rapport problems and an upsurge of paranoia among the residents of Ramah. In some cases a team of researchers might have to pretend not to know another team of researchers and to "practice avoidance." To live one's life in an excellent social science laboratory was to be under a microscopic eye--and "Where does Harvard get all the queer people it sends to Ramah?" a former resident was overheard to ask. Both parties, the scientific investigators and their objects/subjects of investigation, were under a high degree of scrutiny and stress.

Observed retrospectively, the Five Cultures project shared the common overarching concerns of postwar American social sciences. In the wake of World War II, humanity itself was taken by these sciences as a problem (as in the title of a contemporary pamphlet by the

Harvard Committee of Overseers, "Man's Greatest Problem--Man"). In projects such as Five Cultures, mankind was problematized, one can argue, in a new way, as summarized in the advent of the phrase "the human factor." There was a sense that the physical sciences had made great strides, but that the social sciences lagged behind. Thus there was much conversation among social and behavioral scientists concerning a "Manhattan Project" devoted to their own domains. (Indeed, Talcott Parsons often spoke of the necessary task of "splitting the social atom.") It was in this sense that Five Cultures formed a key component of Harvard's Department of Social Relations under Parsons, as well as the Laboratory of Social Relations to which it was affiliated and under which it was administratively lodged. The ambitious hope was that human values could be studied--and perhaps even instilled, engineered, modulated--more intensively than ever before.

This paper uses archival investigations as well as published sources to construct a genealogy of this vast, collective enterprise in knowledge gathering and knowledge production. In particular, the aim is to explore the meaning of Five Cultures as an epistemological experiment in treating five different cultures as laboratory-like situations capable of generating new forms of fact in the *Geisteswissenschaften*. Finally, the paper considers how this project bears on debates over values, subject-object relations, and methodology in the human sciences today.

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Social relations in retrospect: Explaining the demise of Harvard's interdisciplinary department

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This paper analyses the convergence-divergence dynamic of Harvard's Department of Social Relations (1946-1970), with particular attention to its organizational context. Archival evidence indicates that the department was created largely as an expedient solution to problems involving Harvard's three "small" social science departments (Sociology, Psychology, Anthropology), that it was internally split from its earliest days, and that it began to lose its focus and sense of mission even during the chairmanship of Talcott Parsons (1946-1956). The analysis will also show that, although the sociologists ultimately toppled the experiment, the department's major fault line arose from the autonomous development of the discipline of psychology.

(A) FORMATIVE YEARS, 1946-1951.

1. The Administration's View. Correspondence between Dean Paul H. Buck and psychologist Edwin G. Boring suggests that the Conant administration did not justify Social Relations on intellectual grounds. Rather, the rationale was psychological and political: to bolster morale and to reduce the disparity between larger and smaller social science departments. Such sponsorship could not serve as a permanent charter for the experiment.

(cont. p. 213)

2. Theorizing the Experiment. Talcott Parsons believed that the Social Relations experiment required a grounding in fundamental principles. Pursuant to this end, he obtained a grant from the Carnegie Corporation to conduct an intensive seminar (in fall 1949) on theoretical synthesis. The project included appointments for sociologist Edward Shils and psychologist Edward Tolman. Its ultimate product was *Toward A General Theory of Action* (1951), which introduced a new interdisciplinary paradigm.

(B) **THE PEAK YEARS, 1951-1956**

1. Growth and Stabilization. From its earliest days, Social Relations dealt with far more students and majors than had the Department of Sociology, which it displaced. Within five years, Parsons could boast that the Department had attained a firm acceptance. In 1954, moreover, Social Relations gained votes of confidence from both internal and external evaluators.

2. Internal Conflicts.

A) Integration versus Autonomy. The statement of principles in *Toward A General Theory of Action* struck some as a threat to intellectual freedom. Sociologists Pitirim A. Sorokin (the first chairman of Sociology) and Carle C. Zimmerman were openly defiant. Far more serious, however, was the opposition of social psychologists George C. Homans and Jerome Bruner.

B) The Problem of Psychology. The most severe internal strain involved psychology, which was split in two by the reorganization of 1946. Some, especially Gordon Allport, accepted the rupture between "biotropes" and "sociotropes" as a necessary evil. Others, however, felt differently. Not surprisingly, Edwin G. Boring, the first chair of Psychology (1934), fiercely opposed the partition. Later, reacting against Parsons's unitary framework, Bruner also wrote Allpot of the need for greater autonomy for psychology.

(C) **DRIFT AND DISCONTENT, 1956-1965**

1. Personnel Problems. The ultimate instability of Social Relations resulted, in part, from personnel turnover and diminished collaboration. Surprisingly, this pattern emerged during Parsons's tenure as chair. Thus, in 1953-54, the department lost several promising junior members by resignation, including psychologists Robert Sears, Walter Toman and Henry Riecken, and sociologists Francis X. Sutton, Edgar Borgatta, Joseph Kahl and Kaspar Naegele. By 1962, only Parsons and Allport remained from the founding group of visionaries. The department's administrative sponsors, President James B. Conant and Provost Paul Buck, were also gone.

2. Transition in Leadership. While no one could have replaced Parsons as driving force, it is difficult to see his successor, Robert White, as more than a caretaker. White's reputation did not approach the renown of Parsons, Allport, Henry Murray, and anthropologist Clyde Kluckhohn. Indeed, White had not even been a contributor to *Toward A General Theory of Action.*

3. Psychology's Divergence, 1958-59. Harvard's official pronouncements and *Toward A General Theory of Action* claimed that the fields of sociology, social psychology, social anthropology and clinical psychology were converging on a new interdisciplinary synthesis. This scenario, however, overlooked forces that intensified differences among the social disciplines, as each rapidly expanded in the post-World War II period. The social psychologists and their physiological counterparts took a further step toward reunification at an internal gathering in January 1959.

(D) **FRAGMENTATION AND DISSOLUTION, 1965-1970**

1. Reorganizing Psychology.

A) The Demise of Clinical Psychology. The graduate program in clinical psychology was in difficulty from the start. Henry Murray's leadership at the Psychological Clinic had been largely discredited by the late 1940s. Talcott Parsons warned of conflict between the clinical

program and the training in psychiatry at Harvard Medical School. Jerome Bruner campaigned against separate degrees in clinical and social psychology. During the 1960s, the program came under criticism from the American Psychological Association and the National Institute of Mental Health. In spring 1967 the department recommended that the clinical program be replaced by training in personality and abnormal psychology.

B) Disciplinary Reunion. Upon his appointment as chair of the department in 1967, social psychologist Roger Brown, together with Richard Herrnstein, chair of the Department of Psychology, advanced a proposal to reunite Harvard's psychologists by granting all of them membership in both Psychology and Social Relations. This, however, provoked a backlash from the sociologists, whose influence would necessarily diminish.

2. Final Dissolution. In May 1970, the Arts and Sciences faculty approved the return of Social Relations's increasingly autonomous "wings" to their original disciplinary homes. In fall 1970, the Department of Sociology reappeared on Harvard's organization chart, under the chairmanship of George Homans, who had led the challenge to Parsons's unification efforts two decades earlier.

CONCLUSION

The Social Relations experiment began with a faith in the convergence of several related fields. A quarter-century later, however, despite ever-increasing interdisciplinary work, the earlier boundaries proved persistent, indeed resilient. While psychologists probed ever more deeply into micro-level biological sources of behavior, sociologists increasingly looked at economics, politics and their hybrid, political economy, for macro-level keys to understanding the turbulence of the 1960s. The Social Relations experiment was a casualty of the process of autonomous development that transformed its constituent disciplines from within.

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Egon Brunswik and the Search for Unity in the Sciences

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Egon Brunswik (1903-1955), a student of Karl Bühler at the University of Vienna, served first as Privatdocent under Bühler at Vienna, and, with Tolman's assistance, moved to the University of California at Berkeley in 1937, where he became full Professor in 1947. Brunswik's early death in 1955 meant that the implications of his "probabilistic functionalism" was never fully developed. In 1956 the manuscript of his major theoretical and methodological work, *Perception and the Representative Design of Psychological Experiments* was posthumously published. Toward the end of that book, Brunswik noted the potential contributions of cybernetics and communication theory, specifically comparing Wiener's time series analysis to Brunswik's own use of bivariate correlations among distal

and proximal cues, and Shannon's use of uncertainty measures to Brunswik's use of "equivocation" among cues and extraorganismic events.

Brunswik's copy of Shannon's *Mathematical Theory of Communication* (1949) has recently been found. The copy contains notes by Brunswik on the first blank page, and interesting underlinings. The copy also contains four inserted pages of extensive notes (two leaves, front and back) kept by Brunswik on other books he had read. These materials illuminate several aspects of Brunswik's thought, complementing existing records and biographical studies (e.g., Kurz & Tweney, 1997; Leary, 1987).

Brunswik's 1952 *Conceptual Framework of Psychology,* part of the *International Encyclopedia of Unified Science* is a major statement of the way Brunswik saw his work in the context of larger questions about the status and nature of psychology. As in his 1956 book, Brunswik hailed Shannon as having provided a potential key to the unification of psychology with the other sciences. Brunswik ended the 1952 book by noting that information theory held out the possibility of helping in "the efforts ... to determine the structural and functional properties of the unit of behavior in abstract terms." (Brunswik, 1952, p. 92). How this might have evolved, had Brunswik lived longer, is impossible to know, but we can see his concern with the quantification of uncertainty as a strong link. Just as noise in a communication channel can render a received signal ambiguous, so also does the multiplicity and partial correlation of cues with environmental objects and events render a behavioral judgment uncertain.

Because of the inserted notes in the Shannon book, however, we can see that there was more to this than simply an identified similarity of ideas between Shannon and Brunswik. Most of the notes concern three books (with incidental references to others): (1) Lotka's *Elements of Physical Biology* (1925), D'Arcy Thompson's *On Growth and Form* (1942), and William Smart's *Stellar Dynamics* (1938).[1] Brunswik's notes on Lotka and on Thompson show that one major interest was whether or not the supposed analogy between thermodynamics (which is based upon energy conservation principles manifested in statistical laws formulated over large numbers of, say, gas molecules) and psychological theories (which, at the time, were also seeking statistical aggregate laws over large numbers of individuals) could be sustained. In this regard, he noted (and sometimes quoted) numerous passages in which energy principles and least action principles were described and used.

Brunswik's careful reading of Smart's *Stellar Dynamics* reveals that, in part, he was looking for an example of the statistical development of probabilistic laws within theoretical astronomy, although he failed to find any, noting, in a summary judgment, "seems all nomothetic; no predicts from prob." (i.e., "no predictions from probability"). More positively, he singled out instances in Smart's book in which statistical criteria were used to eliminate certain astronomical observations ("outliers"). Since Brunswik's use of correlation statistics and single-subject methodologies was very much out of step with the prevailing mood of American experimental psychology in the 1950s, a mood which had by then solidified a statistical hypothesis-testing approach foreign to Brunswik, this exploration of work within other disciplines is especially notable. For Brunswik, as for Smart, statistics was most useful as a way to characterize data, rather than hypotheses.

Brunswik's 1952 book is an expression of his early interest in the unified science movement, a result of his early contact with the Vienna Circle (Brunswik, 1939). The apparent similarity between information metrics and the physical concept of entropy seemed promising and Brunswik searched for ways to instantiate the ideas for psychology. The unification had to be based upon underlying principles -- like the physical principles of energy conservation that underlie the use of statistical aggregation in thermodynamics. Thus, in his notes on Thompson's book, he singled out a discussion of equilibrium states in stable and unstable systems, quoting Thompson on the way principles of maxima and minima, when

counterpoised in a physical system, tended to produce symmetrical patternings, as in soap bubbles, or the shapes of soap films on water. Thompson used such principles to account for morphological changes over evolutionary time in the appearances of organisms, and Brunswik may also have sought similar principles that could be used in psychology.

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Reassessing Yale's seminar on the impact of culture on personality: Intertwining the local and the global

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This paper explores both the local and the global significance of the field that called itself "culture and personality studies" in the 1930s. It focuses on a unique academic experiment: the "Seminar on the Impact of Culture on Personality" held at Yale University during 1932-33. Sponsored by the Rockefeller Foundation, this seminar allowed 13 young scholars from 11 countries to spend a year working with American anthropologist Edward Sapir and social psychologist John Dollard. My paper explores the local as well as the global ramifications of this seminar, and of "culture and personality studies" more generally, by focusing on the work done by these participants after the seminar ended.

Yale's Seminar on the Impact of Culture on Personality, commonly called the "Impact Seminar" or the "Rockefeller Seminar," is often cited in histories of American social science largely because of its unusual organization and ambitious goals. By using foreign scholars as research assistants, native informants, and psychoanalytic subjects, this seminar's American organizers hoped to generate new types of interdisciplinary as well as cross-cultural collaboration. A phenomenon in its day, its guest speakers included Arnold Gesell, Robert Yerkes, Harry Stack Sullivan, Harold Lasswell, Bronislaw Malinowski, W. I. Thomas, Florian Znaniecki, and many others. By the year's end, however, with few evident

results, the Rockefeller Foundation decided against another year's funding. Thus the Impact Seminar has largely been seen, both by contemporaries and by historians, as at best a disappointment and at worst a failure.

My research reconsiders the results of this Seminar by focusing on its influence abroad. Towards this end, I have been tracing the work of its 13 foreign participants. These include Finnish psychologist Niilo Maki, Norwegian psychologist Henry Halvorsen, Hungarian psychiatrist Andras Angyal, Turkish educator Ali Kemal, German criminologists Walter Beck and Willy Gierlichs, Polish sociologist Jan Krzyzanowski, Yiddish linguist Max Weinreich, Chinese sociologist Bingham Dai, Japanese sociologist Michiji Ishikawa, Indian sociologist T.P. Chitambar, Italian writer Leo Ferrero, and French economist Robert Marjolin. Their accomplishments and failures tell a far more complex story about the legacies of this experiment.

This paper will focus on three of these participants. After the seminar ended in 1933, sociologist Michiji Ishikawa returned to Japan, where he became an editor of *Cultural Nippon,* an English-language journal whose purpose was to explain Japanese culture and personality to foreign audiences. In articles with titles such as "The Japanese Concept of Man," Ishikawa used culture and personality theories to explain Japanese actions. Over the next decade, his journal worked increasingly closely with government officials to defend Japanese imperial policies and military actions in China. Japan's wartime activities affected the Chinese seminar participant, Bingham Dai, in a different way. Leaving Yale, Dai completed his sociology doctorate from the University of Chicago as well as training at the Chicago Institute of Psychoanalysis before returning home to teach medical psychology at Peiping Medical Union College. Dai's dissertation, *Opium Addiction in Chicago* (published in Shanghai in 1937), is now a classic in the study of drug addiction. He also became China's first lay analyst. Fleeing the Japanese in 1939, Dai returned to the U.S. to teach at Fisk University, an all-black institution, where he worked closely with sociologist Charles S. Johnson on the Negro Youth Study. In striking ways, Dai's interests paralleled those of another seminar participant, Max Weinreich. Before being chosen for the Yale seminar, Weinreich had earned a German doctorate in sociolinguistics, writing a dissertation on the development of the Yiddish language; he had also helped to found YIVO, a Yiddish-language social science research institute headquartered in what was then Vilna, Poland. Much like Dai, Weinreich emerged from Yale with interests in African American culture, youth study, and psychoanalysis; he spent the following year studying in Vienna before returning to Vilna, where he organized a Jewish Youth Study project based on hundreds of adolescent autobiographies. While few of these Eastern European Jewish adolescents survived the war, many of their autobiographies did, for Vilna ghetto fighters risked their lives to save this collection. Weinreich himself escaped to New York in 1940, where he wrote his now-classic study in the sociology of knowledge, *Hitler's Professors.*

This paper will explore common themes in the writings of these very different social scientists working in different parts of the world, all graduates of the Yale Seminar on the Impact of Culture on Personality. It will focus on how these scholars understood and used specific concepts such as "culture" and "personality" and on the ways they explained racial prejudice, the intersection between the psychological and the political, and the social functioning of social science.

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Friday 29th June

Main Auditorium

Symposium: Vicissitudes of Theory in Social Psychology, with Reference to Milgram's Obedience Research	
9.00-9.30	Cathy Faye (York University), Sticking to the Facts: Theory and Metatheory in the History of Social Psychology
9.30-10.00	Sam Parkovnick (Dawson College) (Convenor), Theorizing from Social Psychology: A Case Study of Explanations of Perpetrator Behavior
10.00-10.30	James Good (University of Durham) (Convenor), Milgram, Arendt, and Nazi Germany
10.30-11.00	Gina Philogene (Sarah Lawrence College), Social Representations of the Obedience Experiments
11:00-11:30	John Greenwood (City University of New York) (Discussant)

Seminar Room 1

Psychology in Postcolonial Contexts	
9.00-9.30	Irmingard Staueble (Free University of Berlin), Bringing Western Social Science to the Postcolony: the Rise of and Challenge to US Hegemony
9.30-10.00	Wade Pickren (Ryerson University), Immigration and Naturalization: the Indigenization of Psychology in the United States
10.00-10.30	Adrian Brock (University College Dublin), The Quest for Empire: Psychology, Economics and the Third World
10.30-11.00	Ian Lubek (University of Guelph, Canada); Mee Lian Wong, Lakshmi Ganapathi, (National University of Singapore); Sarath Kros, Savun Touch, Sary Pen, Tim Tra, Maryan Chhit (Siem Reap Provincial AIDS Office, Department of Health, and SiRCHESI [NGO], Cambodia); Bor Ou (Siem Reap Orphanage and SiRCHESI [NGO], Cambodia); Bun Chhem Dy (Siem Reap Provincial Health Department, Cambodia); and Tiny van Merode (University of Maastricht, Netherlands): The Poverty Ahistoricism in Psychology and AIDS Interventions in Cambodia: A P for Historical Contextualization and Longitudinality in Social/Community Health Psychology
11:00-11:30	Alice Bullard (Georgia Institute of Technology): French West African Psychiatry in the Early Post-Colonial: L'Hopital Fann in Dakar

Seminar Room 2

Symposium: Institutional Sponsorship and the Content of Research in the Human Sciences: Perspectives on an Uneasy Relationship	
9.00-9.30	Dennis Bryson (Bilkent University) (Convenor), Reflections on the Foundations and the Social Sciences in America: the 1920s and 1930s
9.30-10.00	Zsuzsanna Vajda (Miskolc University) (Convenor), The Curious Power of Social Science Research in Hungary, 1963-1975
10.00-10.30	Trudy Dehue (University of Groningen), Changing Depression
10.30-11.00	William Woodward (University of New Hampshire), Who Sponsors the Liberation of the Oppressed?

Main Auditorium

The vicissitudes of theory in social psychology: With special reference to Stanley Milgram's Obedience Research

Convenors: James Good and Sam Parkovnick

This symposium will look at problems in theory in social psychology using Stanley Milgram's obedience research in the early 1960s as the main exemplar. The first paper, "Sticking to the Facts: Theory and Metatheory in the History of Social Psychology,' sets the symposium in historical context, there is in general a poverty of theory in social psychology, focusing on the interwar years, between World War I and World War II.

The second and third papers focus on Milgram's obedience research. Briefly, the research was done in the early 1960s, mostly at Yale University in New Haven, Connecticut. The first publication of the research was "Behavioral Study of Obedience" which appeared in the *Journal of Abnormal and Social Psychology* in 1963. Milgram would publish a number of articles on the research during the 1960s culminating in the publication of *Obedience to Authority* in 1974. The research came to be widely publicized, both inside and outside the academic community, and drew a great deal of criticism regarding both the ethics of the research, which will be bracketed in the symposium, and on theoretical grounds, the focus of the symposium.

The second paper, "Stanley Milgram, Hannah Arendt, and Nazi Germany," looks at Milgram's obedience research in conjunction with Hannah Arendt's attempt to come to terms with totalitarianism. It traces the development of the theorizing of both Milgram and Arendt and then takes up problems in theorizing in both, problems which are amazingly similar given the divergent backgrounds of Milgram (in psychology) and Arendt (in philosophy). One problem is whether the empirical evidence (in Milgram's case the experiments and in Arendt's case the case study of Adolph Eichmann) supports the theory. A second problem concerns the compatibility of theories in the social sciences which tend to be causal/deterministic with freedom, choice, and responsibility, to which both Milgram and Arendt subscribed. The paper ends with both Milgram and Arendt addressing America in the late 1960s and early 1970s.

The third paper, 'Theorising from social psychology: A case-study of explanations of perpetrator behavior', explores the theoretical use made by such scholars of the work of Milgram and Zimbardo in explanations of what has come to be called 'perpetrator behavior'.

Studies of perpetrators' history shift attention away from those who formulated policies – from Nazi elites and the institutions of the Third Reich, for example, onto those who performed the actual killings of civilians and combatants i.e those who acted as agents of those institutions. Three types of explanations of perpetrator behaviour are outlined structural, intentional, and situational. Examples are provided of each and these explanations are assessed in terms of their effectiveness in illuminating perpetrator behaviour. They are also examined in terms of the extent to which they are compatible with the presumed autonomy of historical agents.

One of the most influential developments in theoretical social psychology of the past half century has been the study of social representations, the focus of the fourth paper. Social representations of events play an important role in the constitution and reproduction of social facts and processes. Social representations of scientific events are central in the rhetorical constitution of disciplines. Thus the diverse social representations of the obedience experiments can be seen as reflecting (and in turn contributing to the development of) the perceived strengths and weaknesses of twentieth century social psychology: the power and elegance of ingenious experimental manipulations on the one hand and the morally questionable use of deception enshrined in the 'fun and games' mentality of 1960s social psychology.

Sticking to the Facts: Theory and Metatheory in the History of Social Psychology

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York University

In 1929, Thomas Eliot, a professor of sociology at the University of Washington took on an intriguing and important task. He, along with a graduate student, sought to re-examine the controversial issue of the nature of the group concept in social psychology (Cornell, 1935). In order to do so, they contacted leading scholars once embroiled in this debate and requested their current views. While the responses were many and varied, the replies indicate a growing fatigue with the seemingly intractable issue. Some respondents, such as E. A. Ross, were generally too busy to revisit the issue and others, such as Frederic Thrasher, responded more specifically that they were too consumed by empirical projects. Some believed the problem would be solved only through empirical research. Robert Lynd (1929, as cited in Cornell, 1935), for example, replied that a theoretical discussion of the problem was unnecessary: "The point is: What is your problem and what techniques do you have for digging into it? If we focus on data-gathering the verbal difficulties may take care of themselves." Indeed, social psychology at the end of the 1920s had become an increasingly empirical enterprise that had little time for either the grand social theory of the late nineteenth century or the metatheoretical debates that consumed the discipline in the early twentieth century.

Debates over the role of theory and abstraction in social psychology have peppered the literature since the inception of the discipline. The greatest surge of literature in the area, however, arose during the 1930s and 1940s as experimental social psychology came to the fore. Holt (1935) leveled a harsh critique against overly theoretical and overly abstract social psychology, stating that any textbook in the discipline was little more than "a farrago of vague, pedantic, and utterly useless abstractions" (p. 172). Britt (1937) applauded the burst of experimental work in the 1930s and contrasted it with the social psychology of the past which was "tainted by too much 'arm-chair' philosophizing" (p. 464). Cottrell and Gallagher (1941) agreed that persistent theoretical problems had been shelved, but believed this had led to

researchers "running in compulsive circles around only the baldest of positional concepts" (p. 137). Bernard (1942) took an intermediate position, applauding the abandonment of

"metaphysics and mysticism" (p. 21), but calling for theoretically minded scholars to replace the "statisticians, questionnaire makers and mongers, and machine operators" (p. 24) that had come to dominate the discipline. Throughout this literature, notions of theory, metatheory, and social philosophy are used almost interchangeably, such that an argument against any one often slips easily into an argument over the others. As such, a theoretical orientation became almost synonymous with a metatheoretical or social-philosophical orientation.

While many contemporary authors have noted this decline of theory in social psychology (Kruglanski, 2001; Schaller, 2002), the present paper aims to examine this decline more historically, focusing on potential reasons for the aversion to abstract theorizing within the discipline. Because theory in general became associated with the grand social theories of the late nineteenth-century and later the metatheoretical debates of the twentieth century, it became increasingly taboo in social psychology. All-encompassing ideas such as Gabriel Tarde's *imitation theory*, Franklin Giddings' *consciousness-of-kind* and Wilfred Trotter's *herd instinct* became distasteful to social psychologists of both sociological and psychological orientations. An attitude of "sticking to the facts" emerged and theoretical debates concerning the foundations and mechanisms of social life subsided. Simultaneously, metatheory also began to take a backseat. While turn-of-the-century scholars engaged in heated debates regarding the theoretical underpinnings of the discipline, twentieth-century scholars grew tired of the controversy and busied themselves with empirical research projects. While some middle-range theories would emerge from the Lewinian tradition, full-range theory (cf. Back, 1963) remained sparse and metatheory slowly became little more than background noise in a growing experimental enterprise.

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Theorising from social psychology: A case-study of explanations of perpetrator behavior

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The studies by Stanley Milgram and Philip Zimbardo are rightly regarded as among the classics of twentieth century social psychology. Their scientific merits and moral standing have been and continue to be widely debated within and outwith the discipline. In this paper I focus on the representation of some aspects of these studies by scholars who are not social psychologists – by philosophers, sociologists and historians. Some of the work of Hannah Arendt, Zygmunt Bauman, Christopher Browning, Daniel Goldhagen and Paul Roth, among others will be examined (Arendt, 1963; Bauman, 1989; Browning, 1992; Goldhagen, 1997; Roth, 2004).

In particular, I wish to explore the theoretical use made by such scholars of the work of Milgram and Zimbardo in explanations of what has come to be called 'perpetrator behavior'. Studies of perpetrators history shift attention away from those who formulated policies – from Nazi elites and the institutions of the Third Reich, for example, onto those who performed the actual killings of civilians and combatants i.e those who acted as agents of those institutions.

I begin by briefly outlining the principal explanations offered by Milgram and Zimbardo of their own results. In particular I note that Milgram rather underdevelops the theoretical significance of his work opting for a combination of situational factors and an appeal to an 'agentic state' (Milgram, 1974). In summarizing the lessons of the Stanford Prison Experiment, Zimbardo highlights a variety of situational factors (Zimbardo, 2000).

Following Roth (2004), I then distinguish between three types of explanations of perpetrator behavior: structural, intentional and situational. Structural explanations emphasize the fact that no individual or choice can account for the course of events. Thus Bauman emphasizes the role of the state and bureaucratic processes in bringing about what Arendt called the 'dehumanization of the perpetrators' (Bauman, 1989). In contrast, intentional explanations of genocide or other forms of perpetrator behavior see it as an intended outcome. As an exemplar of this type of explanation I draw upon Daniel Goldhagen's attempt to locate the chief causal factor in inculcated beliefs which rationalized and justified otherwise horrific and proscribed acts (Goldhagen, 1997). Situational explanations offer a type of behavioural account: focussing on how people act in certain kinds of environments. Here the work of Milgram (1974) and Zimbardo (Haney, Banks & Zimbardo, 1973) is taken as exemplary.

In the final section of the paper, I examine the extent to which these appropriations of social psychological work provide both explanations and understandings of perpetrator behavior, especially in relation to issues concerning human autonomy and agency. Whereas causal explanations are usually seen as less incompatible with claims about human autonomy, accounts focussing on understanding and exploring reasons and intentions are generally thought to be more autonomy friendly. Although perpetrator behavior may be predictable, however, it need not be seen as necessitated.

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Stanley Milgram, Hannah Arendt, and Nazi Germany

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This paper will look at how Stanley Milgram and Hannah Arendt came to think about Nazi Germany. Milgram was a social psychologist who approached the issue through laboratory research conducted largely at Yale University in the early 1960s. Arendt was a philosopher who did so through her writing on totalitarianism and the case study of Adolph Eichmann whose trial she covered in Jerusalem in the early 1960s.

The paper will be divided into four sections. The first will be on Milgram. It will look at the state of public awareness of the Holocaust in the United States in the early 1960s as well as biographical information to explain Milgram's interest before tracing the development of his ideas on obedience from grant proposals and his early publications, beginning with "Behavioral Study of Obedience" in 1963 and ending with *Obedience to Authority* in 1974. Finally, it will point out the theoretical limitations/problems of Milgram's views on obedience: first, experimental results, in which there was significant disobedience, something inconsistent with Milgram's explanation of obedience in terms of agency which does not in theory allow for the existence of disobedience; second, the incompatibility of a

social-science explanation of obedience which is causal/deterministic and Milgram's insistence on freedom, choice, and responsibility.

The second will be on Arendt. It will present her thoughts on totalitarianism, in *The Origins of Totalitarianism* and various journal articles, focusing on the notion of radical evil and the role of the masses in totalitarianism. Then, it will turn to *Eichmann in Jerusalem* and her depiction of Adolph Eichmann in terms of the banality of evil. In the process, it will try to clarify what Arendt was trying to say, develop the psychology necessary to underly her notion of radical evil (from comments in *The Origins of Totalitarianism* and *The Human Condition*), look at Adolph Eichmann as the prototypical mass man, and, finally, discuss the ethical questions that follow from her depiction in *Eichmann in Jerusalem* of Eichmann who did comply and Sargeant Anton Schmidt who did not.

The third section will compare and contrast Milgram and Arendt, pointing out similarities and differences, weaknesses and strengths. The purpose in doing so is to raise questions and hopefully learn some lessons about how we theorize from the writing of Milgram and Arendt as well as the many who have commented upon, criticized, or praised their writing. One such issue is the compatibility of social-science explanations which are causal/deterministic with freedom, choice, and responsibility, an issue which runs through the work of both Milgram and Arendt. Milgram seemed to hold onto both while Arendt believed in freedom, choice, and responsibility, but her theorizing regarding the masses, for example, lends itself more readily to a causal/deterministic explanation.

The final section will have Milgram and Arendt confront America of the late-1960s and early-1970s, specifically race, student protests, and the Vietnam War. Milgram was a liberal whose views can be pieced together from his correspondence and filled out by the writing of the sociologist Nathan Glazer whose views at the time were very close to those of Milgram. Arendt described herself as a philosopher; her views, which fit no political ideology, can be found in various publications and in her correspondence, a good deal of which has been published.

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Obedience to authority and minority influence

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The study of social influence trivialized Floyd Allport's individualistic approaches to the field of social psychology (Allport, 1924). Whereas he insisted on looking at the group as a summation of individual aggregates, other researchers, starting with Muzafer Sherif, were focusing on the integrative and holistic emergence of a group mind (sheriff, 1936). By the late 1930's the group approach dominated experimental social psychology. Around that time we note that Solomon Asch was developing studies on conformity, and Kurt Lewin had stimulated a large body of research on group dynamics (Lewin, 1945).

In the early fifties, the height of McCarthyism provided a fertile ground for the investigation of group pressure. These studies were brilliantly designed, and the social as well

as political context of the time turned them into instant classics (Jones, 1985). Asch's research yielded a number of additional investigations throughout the 1950s and 1960s, leading to the most controversial follow-up study carried out by Stanley Milgram in 1963. While a graduate student at Harvard (1958), Milgram was the research assistant for Solomon Asch who was a visiting professor at the time. Milgram acknowledged that Asch carried the greatest influence on his own work.

Milgram's famous study on obedience stressed the idea that the authority figure has the power to bring about change. It occurs when a person alters his or her behavior as a response to a command from a person in authority. His study could be recast in a historical context as an analysis of a harmful obedience ("destructive obedience"), such as the one rendering possible the systematic annihilation of Europe's Jews by Nazi Germany. History has provided ample examples of such destructive obedience. However, the compliance to authority so highlighted in Milgram's study has been shown to be significantly reduced when one or two confederates objected to the order of the authority. As a result 90% of the subjects liberated themselves by allying their views with the defiant confederates (Milgram, 1974). The research on social influence focused exclusively on the ability of the majority to influence the individual to conform, thereby neglecting whether an individual (or minority) can influence a majority. This exclusive focus on majority influence corresponded to the then-dominant functionalist paradigm emphasizing pressures towards uniformity and dependency as the key psychological mediating process. According to this perspective social influence could only flow from those who have the power to create psychological dependency (such as, a majority) to those who do not (such as, a minority). Deviancy has consequently been seen as dysfunctional and a threat to group harmony, with deviants either seen as having to conform to the group in the end or face rejection.

Serge Moscovici (1985) objected to this generalized and dominant association between the processes of influence and the idea of conformity. His analysis of influence, which emphasized the ability of a minority to exercise influence on a majority, reminds us of the power of an individual who even in the face of overwhelming pressure to conform can also assert individuality and freedom to resist (Moscovici, Lage & Naffrechoux, 1969; Moscovici, 1985).

It is in the juxtaposition of these two very important paradigms that we can fully comprehend the tumultuous period of the 1960s, marked by moments of blind conformity as well as profound societal transformations at the same time. In this context reframing Milgram's authority figure within Moscovici's paradigm of minority influence provides us with a window to understand better how both an authority and a minority group can exercise considerable influence on a person.

One of the defining factors of influence in the case of a minority has been the behavioral style adopted by that group. To explain why behavioral style is important to minority influence, Moscovici has relied upon Kelley's (1967) attribution theory. By being consistent. the minority is visible in the group and therefore cannot be ignored. In being consistent and persistent, it attracts and even demands attention (Schachter, 1951). Therefore, response consistency leads to attributions of certainty and confidence, especially when the minority is seen to reject publicly the majority position.

Such a style of behavior creates two types of conflict within members of the majority. On the one hand, there is a cognitive conflict due to an increase in the response diversity. On the other hand, there is also a social conflict which arises from the threatened interpersonal relations. And according to the theory, the majority members resolve this conflict by questioning their own position and considering the minority's position as a tangible

alternative (Martin & Hewstone, 2003).

However, if we reflect on the question of the behavioral style a little more, we can easily conclude that it is a representational question. It is by virtue of social representations that we can understand more precisely how these two sources of influence, an authority versus a consistent minority, operate on the individual within the same framework. Depending on how the behavioral style is represented to a group of individuals, the result will yield compliance or innovation. The predisposition of individuals in a group to the source of influence, their history with it, their relationship to it and meaning of it, determines what makes them more inclined to either one of the two possible responses.

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Discussant: John Greenwood, City University of New York

Seminar Room 1. Psychology in Postcolonial Contexts

Bringing western social science to the postcolony
The rise and challenge of US hegemony

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Recent studies of the international expansion of psychology have emphasized the rise of US psychology to dominance in the world, especially in the second half of 20th century (Staeuble, 2004; Moghaddam, 2006). Can the same be said of sociology, political science or even anthropology? Moghaddam states in a side remark that American dominance in psychology "has not been replicated in sociology, anthropology, and other social sciences" (2006, p. 170). This clear 'no' needs some qualification. Not only is there evidence of an Anglo-Americanization of sociology and political science in terms of English as hegemonial disciplinary language (since the 1970's 90 percent), but also of Euro-Americanization in terms of citations in journals, nine tenths of which refer to European and North American authors (cf. Staeuble, 2006, p. 191). There is also the recent observation of a "growing ascendancy" of US anthropology "over many world anthropologies" (Restrepo & Escobar, 2005, p. 101). In epistemological terms, it has been argued that the very construction of social reality underlying the social science disciplines is Eurocentric with deep roots in colonial expansion (Staeuble, 2006).

The paper will try to shed some light, inevitably selective, on the implementation and transformation of social science in Latin America, Asia, and Africa. As to broad context, the large-scale export, after World War II, of sociology, anthropology and political science to the postcolony was closely connected with the activities of the United Nations' educational, scientific, and cultural organization (UNESCO). After the experience of fascism and war, the Social Science Department (SSD) of UNESCO was designed "to unite the social scientists of all countries in a concerted attack upon the crucial question of the age – how the peoples of the world can learn to live together in peace" (UNESCO Secretariat, 1949). From the beginning, there was a striking ambiguity between the emphasis on issue-centered transdisciplinary and the Social Science Department's initiative in establishing discipline-based international associations – Comparative Law (1949), Economics (1950), Sociology (1950), Political Science (1950), and Psychology (1951). The reasons behind this decision for a global structure of knowledge production that was to inhibit rather than to foster the generation of transdisciplinarity, comparative knowledge, not to mention new ways of thinking, are yet to be clarified.

A first focus will be on the early activities (about 1948 to late 1960's) taken by UNESCO in reconstructing social science in Europe and in implementing social science in the Third World. These activities comprised the establishment of research centers (1952 Cologne, 1953 Paris, 1956 Calcutta, 1958 Rio de Janeiro and Santiago de Chile), 'missions of consultants' to report on the state of social science teaching, teaching missions to help create a well-trained local teaching staff, and regional networking. The issues of the UNESCO *International Social Science Journal (ISSJ)* provide a rich guide to these and related activities and will thus be taken as a basis for analysis.

The rise of US hegemony in the course of consolidating institutionalized activities in research and teaching at an increasing number of universities varied, of course, from country to country and from discipline to discipline. Still, by the early 1970's a widespread

response to the received product was a critical stance towards its cultural bias and universalistic claims. Efforts to organize regional collaboration became prominent in Latin America, Asia, and even among the new nation states of Africa. The impact of this first wave of regionalization on sociology and political science will be analyzed.

The emergence of special journals of the international social science associations (e.g. *International Sociology*, from 1986; *International Political Science Review*, from 1980) may indicate a widening gap between disciplinary rigidification, on the one hand, and a continued emphasis on transdisciplinarity in the *ISSJ* as well as regional publications. A comparative content analysis will be made to probe into this assumption. In particular, it will be asked to what extent alternative views are given a voice in the respective media.

To end with an encouraging note, two recent initiatives will be discussed. In 2005, a South/South collaborative program was launched by several social science research networks of Africa, Asia, and Latin America. Its aims as stated in a first Newsletter include the fostering of alternative paradigms on critical issues like democratic governance and peace building. The other initiative, by a collective of anthropologist, has already been much debated. The World Anthropologies Network (WAN) collective aims at rethinking and remaking anthropology "to contribute to a more pluralistic landscape of anthropology round the world" (Ribeiro, in Anthropology News, October 2005; cf. also Restrepo & Escobar, 2005).

Journals to be analyzed:

International Social Science Bulletin/Journal, 1949 to present

International Political Science Review, selectively

International Sociology, selectively

Current Anthropology, selectively

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Immigration and Naturalization: The Indigenization of Psychology in the United States

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In the period between the two world wars, Psychology became fully indigenized in the U. S. What began as a borrowing or importing of a science that originated in a German context with the purpose of providing support for the foundations of rational knowledge had become localized to the American context. Kurt Danziger and others have addressed the rapid adaptation of psychological science to the American setting in this period (Bakan, 1968; Danziger, 1985). Here, the main argument is that it was the rapid expansion of psychological services in this period that was the "naturalizing" force in the indigenization of U. S. Psychology. It was this usefulness that Americans wanted. As William James had pointed out many years before,

> What every educator, every jail-warden, every doctor, every clergyman, every asylum-superintendent, asks of psychology is practical rules. Such men care little or nothing about the ultimate philosophic grounds of mental phenomena, but they do care about improving the ideas, dispositions, and conduct of the particular individuals in their charge (1892).

The growth of the application of psychology to schools, industry, and a variety of practical domains was remarkable between the wars. A case example of this growth and its impact is psychology in New York City in the 1920s and 1930s. Indirect evidence for this impact is drawn from archival correspondence between Harvard psychologist, E. G. Boring and several correspondents. Direct support for the argument is found in the published member directories of the American Psychological Association and, by the late 1930s, the directories of the American Association for Applied Psychology.

In the years immediately after World War II, there was a rapid extension of services to the arena of private, professional psychotherapy. Although this arena had been extant in American psychology since its beginning, as Eugene Taylor has shown, it was in the postwar period that psychotherapy became central to the identity of American psychologists. The Archives of the History of American Psychology holdings of archival materials from the files of Molly Harrower, Robert Holt, Karen Machover, Dorothea McCarthy, Sol Garfield, and other psychologists will be used to explore the postwar growth of American psychotherapy. Published work, both recent, as in the work of Heinze (2004), and older volumes (e.g., Napoli, 1981), and the archival materials will be used to show how and why this particular focus was so reflective of the new, fully indigenized psychology.

Finally, it is asked if historical analyses of the indigenization of psychology in the United States can help us understand the more recent indigenization processes in India and the Philippines. While the process in the United States was relatively smooth and uneventful, this has not been the case in India and the Philippines. In both places, there has been a rejection of perceived American imperialism and exploitation in psychology, with concurrent efforts to develop psychologies that reflect local knowledge and remain true to their particular cultural settings. Ironically, as the quote from James above indicates, this was the same impulse that motivated indigenization in the U. S.

Archival Materials:
E. G. Boring Papers, Harvard University Archives
Archives of the History of American Psychology
Papers of:
Molly Harrower
Robert R. Holt
Karen Machover
Robert I. Watson
Dorothea McCarthy
Sol Garfield
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The Quest for Empire: Psychology, Economics and the Third World

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One of the most important changes that occurred in the aftermath of World War II was the dismantling of the European colonial empires. This development was, for the most part, involuntary. The west continued to need the raw materials of these countries, access to their markets and places where surplus capital could be invested, but it now had to deal with independent governments. Military intervention could only be used in exceptional cases. The Cold War made the situation even more complicated. The biggest threat to the continuation of economic relations was that these newly-independent countries would fall into the sphere of Soviet influence or retreat into isolation, as China had done. The western powers needed to persuade their former colonies that it was in their interests to remain allied to the west.

The words, 'underdeveloped', developing' and 'developed' when applied to countries - as well as the related notion of 'first and third worlds' – are so embedded in our language that few people realise that they are a product of this time (Leys, 1996). They were a part of 'development theory,' which held that the newly-independent countries could become as rich as their former colonial rulers if they pursued the right economic policies. Not only that, the west would give them expert advice and provide them with material assistance. As the Cold War began to intensify, so did the discussion surrounding 'development'. Both reached their height during the Kennedy Administration from 1960 to 1963. A consequence of this situation was that economics took on an importance that it had not enjoyed before. It is no accident that Kennedy's Ambassador to India was the famous economist, John Kenneth

Galbraith. There was almost unlimited funding for the new field of 'development economics' and economists were employed to oversee the spending of 'development aid'.

It was perhaps only a matter of time before others would try to jump on the bandwagon. In 1961, the Harvard psychologist, David McClelland published his well-known book, *The Achieving Society* (1961). According to McClelland, the basic problem of the developing countries was not economic but psychological: their inhabitants were lacking in what he called 'the need to achieve'. He also outlined ways in which this situation could be changed. McClelland's work opened up the possibility of psychologists having a major role in dealing with one of the burning issues of the day and the Society for the Psychological Study of Social Issues (SPSSI) subsequently devoted a special issue of its journal to economic development in Asia. McClelland contributed an article on China (McClelland, 1963). Perhaps more importantly, the American Psychological Association sponsored an educational film, "The Need to Achieve" in 1962. It was essential that these ideas should reach a wider audience if psychologists were going to get a share of the development aid. In 1963, the US government's Agency for International Development, better known by the acronym, 'USAID', funded a project at the Harvard Business School to increase 'the need to achieve' among businessmen from 10 developing countries.

McClelland wanted to follow it up with a similar scheme in India but found to his surprise that USAID refused to support it. He saw a hidden hand at work and it was not the hidden hand that had been described by Adam Smith. It was that of the economists who were advising USAID (McClelland, 1976). They resented this 'intruder' from psychology, who was not merely offering supplementary advice on how development might be achieved. He was making a bid for the territory itself. The scheme in India went ahead with private funding but the success of the overall project was inevitably limited without government support (McClelland & Winter, 1969).

Anyone who reads the contemporary literature will be struck by the viciousness with which economists attacked McClelland's theories (e.g. Higgins, 1968; Schatz, 1971). The favoured method of attack was to question his empirical data and this was easy to do because the data were extremely weak. However, this alone cannot explain the ferocity of the attacks. We also need to ask if the advice that economists were offering was any better. Subsequent developments suggest that it was not, making it difficult to account for the controversy on scientific grounds alone.

Since the end of the 1970's, development theory has been heavily criticised by intellectuals from the third world (e.g. Dube, 1988; Escobar, 1995). The main problem is that 'development' has not occurred. In general, the economies of the poorer countries have been growing but nowhere near at the same rate as those of the richer countries. The end result is that the gap between rich and poor has been gradually widening. This was not supposed to happen. The gap was supposed to be narrowing as the so-called 'developing' countries started to catch up. The whole thing began to be viewed as a confidence trick and there was much talk of "false prophets" whose erroneous advice had led to a massive misdirection of effort. McClelland was included among these false prophets but so were his economist critics as well.

The quest for empire can take many forms. It can involve a scramble for territory and the economic benefits that it provides. Any country that engages in such a scramble will usually come into conflict with other colonial powers. It can also involve a competition for social authority among disciplines and the more tangible benefits that this authority provides. Anyone who believes in 'interdisciplinary cooperation' must take this situation into account. Expertise in any particular area is not to be taken for granted. It is socially defined and can be challenged at any point.

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The Poverty of Ahistoricism in Psychology and AIDS Interventions in Cambodia: A Plea for Historical Contextualization and "Longitudinality" in Social/Community Health Psychology

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In times of budget cutbacks and emphasis on academic productivity, psychology's "long past" but "short history" is historiographically exacerbated when modern PhD theses in some research areas do not consult literature more than 10 years old, nor provide contextualization much deeper than the knowledge accumulated under a mentor's current grant. Lubek & Stam (1995) described how social psychology's "quick and dirty", and occasionally "ludic", laboratory experimentation replaced more labour intensive field research in mainstream social psychology. Danziger (2000) described how "Psychological experimentation was limited to the investigation of effects that were proximal, local, short-term and decomposable". Systematic enquiry before 1920 on " the social effects that primarily concerned such disciplines as political economy, ethnology, sociology, historical linguistics, or Völkerpsychologie had characteristics that were the opposite of those investigable in the psychological laboratory. These effects were largely non-local, distal, long-term, and experimentally non-decomposable..... The effects that preoccupied the older social sciences were also typically long-term, manifesting themselves over many years, often over generations....." (ibid). In conducting multi-disciplinary health interventions in Cambodia, we borrow from disciplines with more longitudinal views-- medical epidemiology and community and public health psychology. This in turn can guide public health education, epidemic prevention and control, etc. (Lubek & Wong, 2003; Lubek, Wong et al, 2002). (Paul Farmer's and Catherine Campbell's multidisciplinary work in Haiti and South Africa, respectively, is reviewed by Lubek, 2003; Lubek in press)

A short-term, cross-sectional view of the world is often not helpful when a deadly epidemic develops within a particular geographical, cultural and gendered location; interventions must be designed, implemented (and evaluated) to block the predicted spread of infection. Within social psychology, a methodological "sea change" occurred following Kurt Lewin's death in 1947; laboratory studies with manipulable "independent variables" dominated over field work studies, either short- or long-term. Danziger (2000), Stam, Lubek and Radke (1998), and Stam, Radke & Lubek (2000) described the upsurge in cross-sectional research, usually completed within a 50-minute hour, by university students acting as (voluntary) research participants between two scheduled classes, and whose "random" data could be aggregated. These post-1950s laboratory procedures "approximate an anomic state in which isolated individuals without historical ties drift from one brief encounter to another" (Danziger, 2000). Apfelbaum (2000) suggested that social psychology's "mainstream theorizing has relied on the epistemological fiction of an ahistorical, (emotionless,) decontextualized, subject governed only by an economic rationality. Instead of this, a historical perspective is required, a theoretical framework which accepts the importance of socio-cultural contexts and the fact that we are enmeshed in genealogical filiations, as members of a family history as well as the broader society's history and its changing representations."

How can such an historically impoverished social psychology provide knowledge, methods and theory relevant to the practical problems faced by community health psychologists confronting HIV/AIDS in an impoverished, developing country such as Cambodia? Could we simply translate and adapt effective health behaviour change techniques from Canada and apply them to Cambodia? But we would fail to take into consideration the socio-historical context weighing heavily on almost all Cambodians: the heritage of the 1970s, 1980s and 1990s when bombardments, Pol Pot's genocidal starvation and "killing fields", occupation armies, rampant diseases and utter poverty combined to leave over one quarter of the population dead (Kiernan, 1999) and the other three quarters, and succeeding generations, psychologically affected. To understand and mobilize community resources against an epidemic such as HIV/AIDS (or STIs, or malaria, or tuberculosis) both a long-term, socio-historical contextualization is needed about the past as is a longitudinal study "forward" of any changes introduced into the community and their effectiveness.

Yet within psychology, there is still today an over-simplified representation of the statistically "universalist" "individual" as the centerpiece of social psychology, methodologically linked to a "quick" 50-minute cross-sectional ANOVA-driven research design, a too-short contextualization of problematics, and little research will to study a social group over a time-frame longer than a research grant-publication cycle. Why are longitudinal studies in social, developmental, community or educational psychology, with their fuller view on the life of social phenomena, groups and individuals– so few and far between? The classic study by Lewis Terman and associates with over 60+ years of coverage in psychology has few rivals; outside of psychology, the insightful cinematic follow-ups every 7 years of children in the UK (7- Up...42-Up) by Michael Apted (and other similar studies underway in Russia, South Africa, etc.) stand out. In psychology, such studies require long-term planning, financing and commitment of researchers. The studies of media and aggression by Huesmann, Eron and associates from the 1960s onwards (Lubek, 1995) "challenged social and developmental psychologists ... to look longitudinally at aggression's development. In a positivist world of ahistorical/ instantaneous/synchronic/ "snapshot" conceptualizations of social phenomena (Gergen, 1973), Huesmann and Eron provided a refreshing alternative as a trans-historical/longitudinal/ diachronic/long-term formulation... updat[ing] psychology's methods toolbox to include epidemiological, cross-lagged panel designs..." . With new statistical techniques to infer "causality", why have so many studies in developmental, social, health and educational psychology still clung to research attempting to squeeze social and health phenomena, like grain

crops, into factorial tables of analysis of variance?

In ongoing work in Cambodia, a number of longitudinal indicators are used to guide and steer the educational prevention and outreach programs to reduce HIV/AIDS infections and alcohol abuse in the community. Using Lewin's (1946/1947) Action Research perspective on community social psychological research, and later Participatory Action Research (Chattaway, 1997; Fine, 2005), our "longitudinal study forward" sees results fed back constantly to the community stakeholders and changes initiated accordingly. Government programs employing medical and social epidemiology supply biennial random blood sampling for populations at risk (HIV Sentinel Surveys or HSS) while daily blood-testing goes on in a Voluntary Counseling and Confidential Testing program (VCCT); these permit comparisons of year-to-year prevalence since 1995; in additional, a government Behavioral, Sexual and Social Survey (BSS) also biennially monitors changes in behaviours such as use of condoms, extra-marital sex, etc. A more comprehensive survey with in-depth focusing on specific risk behaviours has been conducted by the authors on 560 persons annually in Siem Reap, Cambodia beginning in 2001. The results of the changes in behaviour or serology inform any necessary reconfigurings of health education and prevention programs. Examples of how this longitudinal approach to community health psychology has recently helped reduce risk behaviours and infection rates for HIV/AIDS will be provided, alongside attempts to reverse the "poverty" of antiretroviral treatment by challenging profitable globalizing industry to contribute (Lubek, 2005; van Merode, Kros et al, 2006).

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French West African Psychiatry in the Early Post-Colonial: L'Hopital Fann in Dakar

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Postcolonial psychiatry in French West Africa centered at the Fann Hospital at the University of Dakar (now the Universite Cheikh Anta Diop). If colonial psychiatry characteristically favored biology and race as explanatory factors in mental illness, the postcolonial era took a cultural turn and thereby opened the life-worlds of the Africans to medical understanding. Directed from its opening until 1979 by Dr. Henri Collomb, the Fann hospital was the site of significant innovation that sought to blend African healing traditions with Western biomedical psychiatry. Much of the Fann research was predicated on restoring a cultural dimension to mental illness and valorizing locally rooted treatments.

The route from colonial oppression (late 19th C through the 1940s) to cultural immersion (characteristic of the 1960s and 70s) passed by way of psychological testing (in the 1950s). In previous published papers I have explored the use of the Thematic Apperception Test in Africa, and the rewriting of the Oedipal story by Fann-University of Dakar researchers Marie Cecile and Edmond Ortigues (Bullard 2005a, 2005b)

This present paper seeks the roots of the cultural turn in the colonial era. If it is everywhere written that colonial psychiatry was racist, oppressive, and dismissive of local traditions, nonetheless significant precursors to the postcolonial recuperation of culture were present in colonial psychiatry. This paper traces the colonial roots of the postcolonial innovations in psychiatry.

French psychiatrists such as Cazanove (1933) and Aubin (1939) called for increased attention to the study of local cultures, largely via ethnography. Their minority outlook surfaces in a mass of psychiatric literature in which culture was routinely either ignored or deplored. The practices of magic, belief in spirit possession, and fears and allegations of

cannibalism appeared to most French, including doctors working in mental health, at best as dubious superstitions and at worst as demonic and criminal practices.

At stake in this history of the emergence of psychiatric understanding of French African cultures is linguistic incommensurability combined with ontological uncertainty about the demonic realm. The linguistic incommensurability has not faded as the postcolonial era has itself receded into history; the languages of religion and ancestry simply do not equate with the languages of medical diagnosis and treatment (Beiset et al. 1972; Storper-Perez, 1974). Even the historian must confront this persistent divide, navigating a set of compromises and make-shift epistemological bridges between, for example, spirit possession and psychosis.

Geschiere (1997) has explored the "modernity of witchcraft" – a condition in which most people in a relatively modernized African society remain convinced that witchcraft exists and is deeply powerful. This contrasts not just with a scientific positivism and classical theories of 'disenchanted' modernity, but also with the culturally attuned psychiatry of the Fann researchers. Fann researchers, exemplified by the Ortigues (1966) and Zempleni (1968) understood witchcraft, cannibalism, and magic as indicative of psychological disturbances analogous although not equivalent to disturbances suffered by Western Europeans. Fann researchers understood the demonic as a version of the psychological. In advancing this interpretation they built upon research by doctors such as Cazanove and Aubin, as well as on ethnographers such as Bastide (1960) and Jean Rauch.

An ethnographer and film director, Jean Rauch produced a particularly rich study based on extensive witchcraft confessionals. Rauch culled these testimonials from the records of a Christian cult, at Bregbo in the Ivory Coast. These testimonials are a treasure trove for the historian seeking to produce history featuring laymen's voices. In this instance we can also chart the inscribing of these voices into professional mental health discourse via Marie-Cecile Ortigue's doctoral thesis in psychology completed at Fann in the early 1960s.

The search for a universal, scientific language for psychiatry, however, did not halt as culture flourished among Fann researchers. Postcolonial psychiatry remained in the shadow of hegemonic Western biomedical psychiatry. The paper concludes with a brief consideration of increased African population in France and the increased relevance of Fann-informed psychiatry in French cities.

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Seminar Room 2

Institutional Sponsorship and the Content of Research in the Human Sciences: A Symposium on an Uneasy Relationship

Convenor: Dennis Bryson

Inspired by recent debates on the effects of various kinds of sponsorship on scientific research, we propose a symposium on this issue. Many scholars have expressed concern about the intrusive penetration of the market into academic life, suggesting that it threatens the autonomy of scientific research. But to what degree did such autonomy ever really exist? In practice, academic research, publication, and teaching has been deeply embedded in networks of institutions and relationships of power. The pioneers of modern science were initially at mercy of kings, churches, and aristocrats. This situation has changed within the last two centuries, as science has become institutionalised in modern societies. Coming to possess its own institutions, science has obtained a greater degree of autonomy, but – as the contributions to this symposium demonstrate – it has also come to be directed and circumscribed in various ways.

The contributors to this symposium deal with the effects of institutionalisation and sponsorship on research in the human sciences. In the United States, the philanthropic foundations were major sponsors of such research, especially in the first half of the twentieth century. As we learn from two of the contributors, foundation sponsorship tended to be selective, encouraging some trends while discouraging others. Those affiliated with the foundations, including officers and scientists, possessed their own visions of human behavior and culture, and they promoted research that was congruent with these visions. The third contributor will discuss a form of sponsorship that has had a long tradition in Europe: state sponsorship. State sponsorship—of the state socialist variety—could not prevent researchers from exposing social problems, and, in the process, these researchers came to contradict official propaganda. The paper of another contributor explores the long-term effects of advertising. She examines how marketing and advertising initiatives conducted by the pharmaceutical industry constructed depression. A final contributor will discuss participatory action research (PAR). With PAR, there is no sharp differentiation between the subject and

the object of scientific research; the examination of such research projects may perhaps shed new light on issues of support and sponsorship.

In discussing threats to the autonomy of science, we do not want to say that the autonomy of scientific research is not a significant goal and that we should give up on this idea. On the other hand, we do think that a deeper understanding of the relationship of sponsorship to the content of scientific research can make us more aware of the covert or overt intentions of the sponsors and thus encourage us to think critically about the orientations and limitations of such scientific research.

Reflections on the Foundations and the Social Sciences in America: The 1920s and 1930s

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The first decades of the twentieth century witnessed a great sense of excitement among various groups of social scientists, biomedical specialists, and foundation administrators about the possibility of utilizing the social and life sciences both to understand and to reform human society. This sense of excitement was given special impetus by the Rockefeller philanthropies—especially the Laura Spelman Rockefeller Memorial, the General Education Board, and the Rockefeller Foundation—during the 1920s and 1930s. It seemed to social scientists and specialists in the life sciences, as well as to some foundation officers—including Robert S. Lynd, Beardsley Ruml, Lawrence K. Frank, Wesley C. Mitchell, Charles Merriam, Harry Stack Sullivan, Harold Lasswell, and Edward Sapir—that the formulation and articulation of certain key concepts such as behavior, personality, and culture might provide the basis for a new interdisciplinary approach to the social sciences. This new approach would break down the traditional boundaries of the academic social sciences in order to focus on pressing social problems and issues. It would foster a social science oriented toward empirically-oriented research as opposed to armchair speculation and theorizing. Moreover, the new social science would aim at a holistic, comprehensive understanding of social life, with the goal of reconciling the social and life sciences. Most importantly, it would also be geared toward reconstructing social life in order to promote the welfare of the population.

My argument here is not that the Rockefeller philanthropies simply imposed ideas and scientific approaches in a rigid fashion upon social scientists and others. Ideas about behavior, personality, and culture had been developed by the social scientist themselves, before the advent of the Rockefeller sponsorship. Nevertheless, research on behavior and its formation and breaking, on the investigation of personality, and on the interrelationship of personality and culture was taken up and promoted by the foundations because aspects of this research seemed congruent with their program for utilizing the social sciences as a means for promoting their vision of human welfare, not because the foundations were devoted to the pursuit of knowledge for its own sake. The foundations fostered communicative networks among social scientists and encouraged them to direct their efforts at certain problems and projects. They also attempted to promote interdisciplinary cooperation as a means to formulate common perspectives, concepts, and terms among the social scientists. Generally, these efforts involved amplifying and elaborating pre-existing intellectual trends, perspectives, and concepts, not simply imposing them on the social scientists. The foundations possessed immense resources, utilized these resources selectively, and were often

secretive in their ways, but they operated by forming alliances with the social scientists, rather than by controlling them in a top-down fashion.

All the same, Rockefeller sponsorship of research in the social and life sciences undoubtedly did skew these sciences in certain ways, encouraging social and life scientists to focus on certain problems and issues and discouraging them to examine others. For example, in fields such as sociology and personality and culture, Rockefeller philanthropy encouraged a focus on the microsocial terrain of family life, marriage, the socialization of the child within family, school, and neighborhood, and so on. Most significantly, the microsocial dimension was conceived as providing a privileged route to social reform and reconstruction—while the nineteenth-century (and early twentieth-century) concern with macrosocial structures and conflicts (capitalism, class conflict, etc.) was dismissed as being irrelevant to constructive social change. Ironically, this focus on the microsocial was to bear unanticipated fruit. Social scientific knowledge oriented toward the microsocial problematized the givenness of various cultural practices and aspects of social life—thereby subjecting them to various modes of reflexivity, critique, and contestation. Thus, in a curious way, foundation-sponsored social science contributed to the destabilization of gender roles and race relationships in the United States during the 1960s.

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The Curious Power of Social Science Research in Hungary, 1963-1975

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In this paper I would like to focus on period in which the social sciences flourished in Hungary: 1963-1975. During this period, not only were social scientific studies of lasting

value produced on the development and structure of Hungarian society, but social science research came to influence political practice as well.

It may be unnecessary to explain that the only sponsor and consumer of the academic work in social science was the single-party state. It may be less known that it was a generous sponsor: in the 70s and 80s, government offices, factories and enterprises, the Academy of Science, and the central committee of the communist party each had their own institute or even network of research institutes. As Péteri critically commented (1998), Hungary sustained a level of research equal to that of a much richer country such as the Netherlands. The situation was quite similar in other socialist countries. Was this situation an example simply of the wasteful spending of money (as Péteri believed)—or did it indicate a genuine respect for knowledge? Perhaps how one answers this question is, at least in part, a matter of world-view. The picture becomes even more complex if we take into account the deep ambivalence of the sponsors, the officials of communist party and the communist state, toward the results of the social research that they had funded. Getting more deeply acquainted with the period in question clarifies how this odd system worked.

There are several reasons why a boom in the social sciences had taken place in 1962. According to István Kemény (1991), one of the most important scholars of the era, the main thing that gave a boost to the social sciences was the amnesty of the prisoners of 1956. Research institutes seemed to be good places where the unreliable intellectuals, released from prisons, could earn some money and be kept isolated from everyday people. But there was another motive: curiosity. Scholars are a curious and inquisitive group; this is one of their most important features. But the curiosity of scholars would not in itself be enough: political decision makers wanted to learn about the impact of their actions. This attitude made the post-1956 period significantly different from the harsh dictatorship of the fifties. Moreover, social and economic research was inspired by preparation for economic reforms—which were introduced in 1968 and partly withdrawn in the early 70s.

In 1963 György Péter, the president of the Central Office of Census, a former illegal communist (that is he entered communist party before the war when it was still illegal) and the earliest initiator of economic reforms in 1954, noted the importance of studying the social structure of Hungary. In the same year, the Hungarian Academy of Science established a research centre of sociology under the leadership of András Hegedüs, the defeated prime minister in 1956. Thus, an extensive sociological and statistical research project got underway with the contribution of István Kemény, Zsuzsa Ferge, and others. The project revealed that Hungarian society was stratified, and that the various strata did not share equally in the distribution of goods. Though such inequality was not comparable with what it currently is, reference to it seemed to refute party propaganda—according to which the workers and peasants were to be privileged in a socialist society. Consequently, representatives of political power were angry and disappointed. István Kemény was released from his job after he started to talk openly about poverty problem. Moreover, since he did not, after that, stop investigating the situation of manual laborers, poor people, and romas, he gradually lost all opportunities for making a living, and, in 1977, he emigrated to France. The same happened with Iván Szelényi, who demonstrated that the redistribution of income benefited influential social groups, especially the intellectuals. I had a similar experience when I started my carrier as a social psychologist in the biggest heavy industrial enterprise of Hungary, and I am going to discuss this as a "case study".

But in spite of censorship and repression, the government and the party could not escape from the necessity of having to deal with the social problems created by inequality; they were thus pushed to pursue both political measures and more research.

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Changing Depression

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Since the late 1980s, the incidence of clinical depression as well as the prescription of antidepressants has been increasing rapidly. In 2000, the WHO declared depression to be the leading cause of disability in the Western world (www.who.int/mental_health). Epidemiologists estimated the prevalence of major depression for lifetime in the US at 16.2% (Kessler et al, 2003), and in Western Europe at 5-10% (Möller et al 2005). In addition, many more people suffer from the milder forms of dysthymic disorder and minor depression.

How to account for the emergence of this depression-epidemic particularly in the affluent parts of the world? The most common explanation is that depression is a brain disease that has always existed but only recently can be diagnosed and treated properly. According to this biological explanation not the disease but its treatment has increased. And that occurred in the richer countries first because there the right diagnostic tools and effective treatments are available. Whereas formerly sadness and listlessness were seen as signs of weakness, the argument goes, today patients are no longer put to shame; family-members are less reproachful and doctors more willing to help.

This biological explanation of the depression epidemic, however, is inadequate in several ways. Neuroscientists do not claim that the biological mechanisms of depression are known. And even if they did it takes social agreement to consider it to be a disease, just like it does in the case of, for instance, aggression or homosexuality. How could it happen that most professionals as well as patients in Western societies currently conceptualize unacceptable sadness as a disorder of the brain?

This paper makes a start in studying the latter question by focusing on the biological explanation's historical claim. I will argue that it takes a finalistic view of history to describe former accounts of 'melancholia' as proof that 'depression' has been prevalent since ancient times. Finalistic histories of biological psychiatry typically ignore that notions such as 'melancholy' and also 'depression' in the past have referred to varying kinds of behavior and emotional states. It is only in this way that they can maintain that current depression treatments relieve us from an age-old condition former generations simply had to accept.

By the 1860's medical dictionaries began to include the word 'depression' in a psychiatric sense. The expression referred to spiritual lowness caused by physical disease (Berrios, 1996). In the early 20th century Freud defined 'melancholia' as extreme mourning caused by unaccepted loss in life. His psychodynamic view inspired the notion of 'depression' in the early versions of the DSM as a response to an inner conflict that in each patient was unique. It was with the introduction of the DSM-III in 1980 that depression became a disease in itself. And it was the chance discovery of antidepressants in the 1950s

that gradually established its image of a biological disease (Rosenberg, 2002; Mayes & Horwitz, 2005). But even then the definition of depression did not stabilize. Using antidepressant advertisements, I will demonstrate that in the 1950s depression hampered people's ability to deal with daily duties whereas today it impedes one's capability to excel. The most recent advertisements completely reverse the earliest definition in mid 19th century dictionaries. Currently, pharmaceutical industries depict depression as a cause of emotional but also physical suffering, which, of course, strongly extends the range of complaints antidepressants must treat.

Most importantly, these advertisements also express the present-day duty to manage one's destiny. As commentators of neo-liberal societies have argued, in such societies people no longer are only responsible for their faults but also for their fate. And this includes responsibility for one's body and brain (Rose, 2006). To be sure, I would not want to argue that the depression-epidemic can be fully accounted for by the actions of pharmaceutical industries. More than that they can shape them, antidepressant ads give expression to common beliefs. This paper's main claim is that historical awareness challenges the explanation of the current depression-epidemic that declares depression an age-old disease. The general notion of depression as a brain disease does not offer an explanation of the depression-epidemic but needs itself to be explained. Antidepressant advertisements express the moral standards that the word 'depression' in its current sense conveys.

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Who Sponsors the Liberation of the Oppressed?

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What we know today as "participatory action research" began in the 1960's in the Third World in a variety of liberation movements (Anon, 2005). As such, it did not have official sanction or institutional funding. The proponents were academics, priests, NGO consultants, and popular leaders, all finding common cause in reforming the existing social order in specific cultural contexts. The characteristic of these ideological counter-elites is their work toward subversion of the status quo. Often, but not necessarily, they disqualified themselves for institutional funding and had to flee the university or the church which had employed

them. As émigrés, their intellectual leaders gained support from world churches and think tanks abroad, including United Nations agencies.

For example, in 1967 Orlando Fals Borda wrote a book on military subjugations of peasants in his book *Subversion and Social Change in Colombia* (1969). Subversion had the goal of enabling social, intellectual, and spiritual freedom for workers on the land. Falls Borda wrote as a professor at the National University of Colombia; after his flight from Colombia, he served as Visiting Professor in the U.S.; the sponsoring institution was the Institute of Latin American Studies at Columbia University. . His group aimed to create agrarian entrepreneurs and a rural middle class.

Falls Borda dedicated his book on social change to a renegade Catholic priest, Father Torres, who put forward a pluralist utopia in 1965, left the Colombian Catholic church, and advocated counterviolence toward the government-church-military elites. Torres sought to alleviate the poverty and persecution of the farmers – for which he was killed eleven months later. Father Torres' writings and violent death served to "unite the left" -- the ecumenical movement, other worker priests sympathetic to Marxism, and The United Front, among other grassroots political groups.

In another Latin American country, Paulo Friere completed his dissertation on literacy methods at the University of Recife in Brazil in 1959. Following a broader literacy campaign, he was arrested in the military coup in 1965 and fled to Chile. There he worked on adult education in the Agrarian Institute for Social Reform with UNESCO support for five years. After a stint at Harvard's School of Education, he joined the Office of Education of the World Council of Churches in Geneva as a special consultant. He argued that genuine revolutionary leaders learn how to name the world" (1982 [1968], p. 179). They cannot say their word alone but must do it with people, acting dialogically, in order to organize others. Those who insist on their word by imposing it upon people do not liberate, they oppress. Paulo Friere thus worked with and inspired the masses, offering instruments for self improvement beginning with literacy.

On another continent, a Finnish social scientist, Marja Liisa Swantz (1975) spent 1965 to 1970 at the Bureau for Research on Land Use and Productivity of the University of Dar es Salaam. She initiated a number of projects with participatory change in Tanzanian communities, publishing an article called "Research as an educational tool for development" (1975). Her North American colleague Budd Hall worked at that time in Tanzania. He may have coined the term for a new North American field in his 1975 essay "Participatory research: An approach for change" (1975). He transformed the subversive "development and resistance" focus into a methodological critique: standard social science research methods were not appropriate in certain situations. Research based on isolated individuals, and even aggregates of data on individuals, could not reliably state what the social behavior of a group would be. Given that traditional data collection only occurred at single points in time, it was ahistorical and therefore also could not adequately describe social change.

Thus, resistance and genuine creative innovations came from the non-Western world in the 1960's. Let us refer to this, following the Mexican applied anthropologist Rodolfo Stanhagen (1971), as "decolonializing" or "de-elitization." The principles expressed in these terms have "thick textures" in local sociopolitical struggles over the right to own land, the right to an education, and the right to be heard in court. These liberation movements later gained a reception in the embryonic field called "participatory action research" in the 1970's, a field that was developed by North American academics in the subsequent decades. The North American story of participatory action research (PAR) would take us beyond the theme of this brief paper. For PAR was clearly not forged in the crucible of popular movements that risked lives. Rather, it began with efforts to insert and adapt Western methods of social science in non-Western countries, and then it began to study disenfranchised groups (women,

minorities, ethnic populations, disability groups) in such a way as to assist them in achieving their goals.

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