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Copies of some papers sent by FAX were of such poor quality that they could not be reproduced. Consequently, only the abstracts have been included. Our apologies to the authors

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The 'psy complex' revisited: the psychologization of culture and the case of psychoanalysis (Abstract Conference Cheiron-Europe)

Our Western culture is thoroughly psychologized, and it was psychoanalysis that did the job. This claim, put forward by the Foucault-inspired French sociologists Robert Castel and Jacques Donzelot, is put to the test by an analysis of developments in mental health care in the Netherlands, based on about 25 studies undertaken in the period 1978-1992. These studies show that psychoanalysis indeed did play a pivotal role in the psychologization process, although it was not, strictly speaking the practice, techniques and theory of psychoanalysis that achieved this, but its general 'psychomedical' orientation. Psychoanalysis was a stepping stone for the introduction of humanistic psychotherapeutical orientations, that from the seventies onwards had to give way to behaviour therapy, biological psychiatry, and 'eclectic' psychotherapies.

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The 'psy complex' revisited: the psychologization of culture and the case of psychoanalysis

Western culture is thoroughly 'psychologized', and it was psychoanalysis that did the job. This claim, put forward by the Foucault-inspired French sociologists Robert Castel (1973, 1980) and Jacques Donzelot (1977), will be put to the test by an analysis of developments in mental health care in the Netherlands.

The psychologization of culture implies that not only professionals, but also ordinary citizens, view the problems they are confronted with as amenable to psychological treatment. They tend to formulate their problems in psychological terms and appeal, when they deem it necessary, to 'psy professionals' for help, while in earlier days they sought support from their family doctor or their local priest.

According to Castel and Donzelot psychologization is by no means an innocent affair: problems that are basically social and political in nature are emptied of their social content. Following Foucault, these authors postulate an indissoluble bond between *help* and *power*. These two notions do not, as in traditional 'triumphalist' visions on mental health care, refer to oppositional practices, nor is help, as stated in some radical views (e.g. anti-psychiatry) just power in disguise (Ingleby 1983, 1985). 'Psy practices' are powerful, *precisely because* they help.

The role of psychoanalysis

Dutch researchers, partly inspired by Donzelot and Castel, have analyzed various episodes in the history of the helping professions in the Netherlands. Some of their studies have confirmed the original ideas of Donzelot and Castel (Abma 1981; Wel 1981; Nijnatten 1986), while others have come to different conclusions. Van Berkel (1990), for instance, concluded in her survey of Dutch catholic magazines that the 'psy professions' did not replace religious practices but rather transformed them, and that it was not psychoanalysis but phenomenological psychology that brought about this transformation. Before embarking on these and other controversies, let me first sketch briefly the global line of the argument of Donzelot and Castel.

In the course of the 19th century several 'disciplinary' practices took shape in industrialized societies: psychiatry, as a new branch of medicine, and modern social work, as the successor to philanthropy. These new practices were added to the pre-existing religious and pedagogical practices. In them people were judged to be in or out of line by legal, moral or professional standards. Attempts to adjust them when out of line contained moral exhortations and various kinds of sanctions. Whatever the case, people were judged by fixed standards. (This does not mean that these standards did not change over time, simply that they were fixed *within* a certain historical period).

Psychoanalysis, often referred to as the 'second psychiatric revolution', signalled the rise of a new approach. First of all, in psychoanalysis there are no fixed standards for evaluating behaviour as right or wrong; in other words: there is no clear division between normal and abnormal. Rather, there is a continuum running from 'more' to 'less' normality. It is a flexible approach. Second, psychoanalysis focuses on the inner life, the 'unconscious' of individuals, as it takes shape in the course of their life. Patients are invited to associate freely on whatever comes to mind during treatment, and in this 'talking cure' no sanctions are applied, nor does the professional prescribe specific rules of behaviour. In the words of Donzelot, psychoanalysis is concerned with 'the regulation of images'. Third, psychoanalysis provides not just treatment, but also an elaborated theory about the mental (i.e. unconscious) causes of mental problems, which are deemed to originate in childhood conflicts. Fourth, psychoanalytic treatment, by contrast with psychiatric and social work practices, is engaged in voluntarily.

According to Donzelot and Castel these characteristics of psychoanalysis made it very suitable for transforming and expanding the fields of mental health, social work, pastoral work and education. In the course of this transformation, psychoanalysis itself became popularized and remodelled, to the extent that a whole new field of psychodynamic activity - psychotherapy - arose, which bore only a distant relation to the core of psychoanalytic theory and practice as fashioned by Freud (cf. Moscovici 1961; Richards 1989). Therefore, from the fifties onwards we find various forms of psychotherapy, counselling, and social case work in which there are no (manifest or explicitly stated) supra-individual standards for judging behaviour, but in which the professional activity is directed towards 'balancing reality and inner life'. In fact, it seems that the only required activity is 'confessing' or talking about oneself and one's interpersonal relations. In the counselling process, which moreover is freely initiated by the client, the professional encourages the client to

talk about his or her problems and subtly manoeuvres the conversational theme during the course of the interaction in the direction of a professionally treatable problem (cf. Davis 1983). Nowadays this is often not even necessary: many people 'know' how to present their problems in a psychological useful fashion - they have become, as Brinkgreve, Onland & De Swaan (1979) call it, 'proto-professionals'.

The 'psy complex' in the Netherlands

What evidence do we have concerning the development of the 'psy'-complex in the Netherlands and the prominence of psychoanalysis in it? To answer these questions, I will present a comparative analysis of about 25 studies undertaken in the period 1978-1992 on the reception and expansion in the Netherlands of psychoanalysis, psychotherapy and psychological ways of thinking.

With regard to the first question, on the psychologization of Dutch culture, there is overwhelming evidence generated by both qualitative and quantitative research that such a process did take place (Brinkgreve & Korzec 1978; Knijn & Verheijen 1982; Zeegers 1988; Hutschemaekers 1990; Lieshout & De Ridder 1990). These results concur with outcomes of related studies done on psychologization in England (Rose 1985), Germany (Mahlmann 1991) and the United States (Castel, Castel & Lovell 1979).

Regarding the role of psychoanalysis in the psychologization process, however, research outcomes are less uniform. In part, the differences originate in variations in definition, methodology and object of study. When making allowance for these differences, it is possible to give a more balanced and detailed account, that will be summarized here briefly.

In the period 1915-1940 psychoanalysis formed a major focus of controversy within Dutch psychiatry (Brinkgreve 1984). Traditional asylum psychiatrists for the most part rejected the new 'psychological' orientation, and stuck to the mainstream biological outlook (Ridder 1984). Independent and 'social' psychiatrists, however, took over the psychoanalytic theory and techniques in part, and psychiatric social workers, who visited (former) psychiatric patients in their homes did so to an ever greater extent (Boer et al. 1984; Schlatmann & Wolff 1985). From 1950 onwards the psychoanalytical orientation was replaced by 'phenomenological' psychiatry, which abandoned the focus on sexual childhood trauma's in favour of an accent on 'life goals' and present relationships (Berkel 1990).

In the broader field of mental health care (e.g. child guidance and marital counseling), psychoanalysis became a major point of orientation from the thirties onwards (Sommer 1983; Lieshout 1985), although some of the tenets of psychoanalytic theory (especially the focus on sexual trauma's) were rejected, mainly because of church influences (Bos 1992; Oosterhuis 1992). Authors on mental health issues in the fifties, such as Fortmann, Ruygers and Trimbos, mixed Freudian ideas with phenomenological and, in the early sixties, Rogerian and social psychiatric orientations (Meulen 1988, 1990; Pols 1988).

In helping professions more distant from psychiatric practice, such as social (case) work, pastoral work and pedagogy, psychoanalytic concepts also were introduced in the fifties, only to be gradually abandoned in the sixties in favour of either 'humanistic' psychological ideas, or group- or family-oriented practices, or a mixture of both (Winter 1986; Wel 1988; Wurff 1987).

Conclusion: from morality to 'psy'?

On the whole, it would seem that psychoanalysis was no more than a stepping stone for the introduction (and victory) of more 'humanistic' (phenomenological, Rogerian) orientations and practices, that in turn had to give way in the seventies and eighties to behaviour therapy, biological psychiatry, and 'eclectic' psychotherapies.

Nevertheless, the studies summarized here also show that it was primarily psychoanalysis that succeeded in bringing all sorts of social and religious 'life problems' into the realm of mental health. It was not, however, strictly speaking the practice, techniques and theory of psychoanalysis that achieved this, but its general 'psychomedical' orientation. Psychoanalysis bridged the gap between 'normal' and 'abnormal' functioning, thereby offering the possibility of 'treating' mental problems that were not 'real' neuroses. It also introduced psychomedical conceptualizations of childhood and the family - domains hitherto governed by traditional religious practices and ideas. The later shift to 'humanistic' orientations could never have taken place without the psychoanalytical ground-work.

The victory of humanistic psychology over psychoanalytic views stems in part from the fact that, unlike psychoanalysis, it allows itself easily to be combined with a moral position. Moreover, it contains a more formal morality: people should talk about their problems, 'work' on their intimate relationships and

consult a professional when they cannot sort their intimate problems out by themselves, and finally, once in treatment then should behave in a 'motivated' manner.

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1894. Causes et conséquences de *L'Année Psychologique*. Cheiron, Sept 94

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TOPIC : 100 years of *L'Année Psychologique*. History of psychological journals in different countries

PAPER : *1894, causes and consequences of the creation of* L'Année Psychologique.

ABSTRACT : Between 1843, the year of the creation in France of *Les Annales Médico-Psychologiques du système nerveux*, and 1894, the year of the création in France of *L'Année Psychologique*, we can observe four definition of psychologie auround four paradigms :

	Review	Paradigm/ Definition
1843	*Les Annales Médico-Psychologiques*	*The medical psychology*
1876	*La Revue philosophique.*	*The pathologic psychology and the Psychophysiology*
1893	*Les Archives de neurologie* *La nouvelle iconographie de la Salpétrière.*	*The neurologic psychology*
1894	*L'Année psychologique*	*The scientific psychology*

Our purpose is to take place the creation of *L'Année psychologique* in the international context of the born of psychology. We had made the hypothesis that the scientific psychology is also the moment, in France in 1894 but early in United States and In Germany, when the psychology to form into a naturalis science and like the basis of a human science.
For that we had reconstructed the history of the psychology in theses four review for explaining the signification of A. Binet's creation, but also the influences of german, british and american exemples of review's creation like *Mind, Brain, Philosophische Studies.*

1894, Causes et conséquences de *L'Année Psychologique*, Cheiron, Sept 94

La création d'une revue comme *L'Année Psychologique* doit d'abord être située par rapport au contexte des autres revues, puisque celui-ci est évoqué dans le n°1/1894 comme la fondation française d'un mouvement international.

En France

- *Annales Médico-Psychologiques du système nerveux* (Jules Baillarger, Cerise, Longuet, 1843)
- *La Revue philosophique de France et de l'Etranger* (Ribot, 1876)
- *Archives de neurologie. Revue trimestrielle des maladies nerveuses et mentales* (Charcot, 1880)
- *La Nouvelle Iconographie de la Salpetrière* (P. Richer, G. de la Tourette, A. Londe, 1888)
- *La Revue Neurologique. Organe spécial d'analyses des travaux concernant le système nerveux et ses maladies* (Brissaud et P. Marie, 1893)
- *L'Année Psychologique* (Beaunis et Binet, 1894)
- *Journal de Psychologie Normale et Pathologique* (Janet et Dumas, 1904)
- *L'encéphale* (A. Antheaume et M. Klippel, 1906)
- *Le Travail humain* (Psychophysiologie scientifique appliquée, 1933)

Le contexte mondial de la création de ces revues par rapport à la France doit être situé.

En Allemagne, on note une grande floraison de journaux éphémères consacrés à la médecine mentale :

- 1783 *Recueil pour l'étude du traitement des maladies mentales*(Moritz).
- 1805 *Magasin pour l'étude du traitement psychique* (par Reil, médecin et Kayssler philosophe).
- 1808 *Recherches d'une méthode de guérison pour les maladies mentales* (par Reil et Hoffbauer).
- 1818 *Journal des médecins psychiques avec des considérations spéciales sur le magnétisme* (par Nasse ; devenu en 1823 *Journal anthropologique*).
- 1829 *Magasin pour l'étude philosophique et médico-légale de l'aliénation* (par Friedriech)
- 1844 *Allgemeine Zeitschrift für Psychiatrie und psychisch-gerichtliche Medizin* (Journal général pour la psychiatrie et la médecine psycholégale)
- 1868 *Archiv für Psychiatrie und Nerverkrankheiten* (Berlin)
- 1876 *Vierteljahrschrift für wissenschaftliche Philosophie*
- 1878 *Centralblatt für Nervenheilkunde*
- 1881 *Philophische Studies* (par Wundt)
- 1882 *Neurolisches Centralblatt*
- 1903 *Psychologische Studien*

Le modèle imité par *l'Année psychologique* est celui des Etats-Unis concernant le rapport entre un laboratoire de psychologie et la création d'une revue.

- 1844 *American Journal of Insanity*(qui deviendra *American Journal of psychiatry*)
- 1876 *Journal of Nervous and Mental Diseases*
- 1878 *Brain* (J.C. Bucknill ; J. Crichton-Browne ; D. Ferrier, J. Huglings-Jackson)
- 1887 *American Journal of Psychology* (G.S. Hall)
- 1890 *The Monist.*
- 1890 *The international Journal of Ethics*
- 1891 *Pedagogical Seminary* (G.S. Hall)
- 1891 *The Educational Review* (G.S. Hall)
- 1892 *The Philosophical Review*
- 1894 *Bulletins de la Société américaine de psychologie*
- 1894 *Etudes du laboratoire de psychologie de Yale*
- 1894 *Psychological Review*(J.Mc Keen Cattel. J.M. Baldwin)
- 1917 *Journal of Applied Psychology.*

En Angleterre

- 1853 *Asylum Journal of Mental Science*
- 1876 *Mind* (Georges Croom Robertson).
- 1904 *British Journal of Psychology*

1894. Causes et conséquences de *L'Année Psychologique*. Cheiron, Sept 94

Le contexte franco-francais est organisé autour du débat de la philosophie et de la psychologie avec la neurologie, l'enjeu étant le statut de la psychologie physiologique.

I) 1843 : le modèle médico-psychologique

En 1843, soit à l'âge de 36 ans, Baillarger[1] fonde, avec Cérise et Longuet, *Les Annales Médico-Psychologiques du système nerveux*. L'idée d'un journal spécialement consacré à l'aliénation mentale aurait déjà été formulée par Pinel et Esquirol avant que l'élève du second n'en ait l'initiative. En 1847 il publie un ouvrage qui porte le tître suivant *Recherches sur l'anatomie, la physiologie et la pathologie du système nerveux* ; en 1890 l'ouvrage fut republié sous le titre *Recherches sur les maladies mentales*, comme pour indiquer le passage de la physiologie à la psychologie, le terme de maladie mentale recouvrant très souvent les pathologies du système nerveux. C'est ainsi que parmi les articles publiés par Baillarger dans les *Annales*, entre 1843 et 1890 seulement 4 relèvent directement de l'anatomie cérébrale. Les thèmes de clinique de pathologie mentale dominent : ainsi 31 articles concernant la paralysie générale, 8 articles sur le thème des hallucinations, et 6 articles sur le crétinisme. Les recherches de Baillarger sur la paralysie générale concernent d'abord la symptomatologie : la nature hypocondriaque du délire. Puis il étudia les apports de la paralysie générale avec la paralysie pellagreuse(« De la paralysie pellagreuse », 1848). Il décrivit ensuite la sclérose de la substance blanche superficielle des circonvolutions cérébrales dans la périencéphalite et signala la coloration ardoisée du cerveau dans la paralysie générale.
Le programme des *Annales Médico-Psychologiques* aurait été écrit par Laurent-Alexis-Philibert Cerise (1807-1869).
Les lignes directrices de ce programme sont les suivantes :
1) faire converger toutes les recherches spéciales vers la solution des problèmes médico-psychologiques.
2) donner place aux travaux historiques, dans un but de critique méthodologique.
3) limitation de la philosophie, stimulation de l'observation
4) accueil des convictions les plus opposées comme le vitalisme et le spiritualisme.
La revue va servir d'organe de diffusion à la Société Médico-Psychologique qui est fondée le 18 décembre 1848. Le programme de la Société concerne :
- la pathologie mentale et la pathologie du système nerveux.
- l'anatomie et la physiologie du système nerveux : l'anatomie pathologique est ajoutée en 1852.
- l'hygiène morale, l'éducation ou la prophylaxie de l'aliénation mentale et des névroses, l'hygiène pénitentiaire, les études historiques sur les maladies de la sensibilité et de l'intelligence.
- l'administration, la médecine légale, la jurisprudence, et les statistiques.
- la philosophie, la physiologie psychologique, l'ethnologie, l'histoire dans leurs rapports avec la science des rapports du physique et du moral.
- l'étude de la science des rapports du physique et du moral fut rajoutée en 1852.
Sans être spécifique à l'introduction en France de la neurophysiologie, *les Annales Medico-Psychologiques* ont fortement contribuer à rapporter les pathologies mentales à des causalités nerveuses, notamment dans le cas de la paralysie. La psychologie est comprise ici à partir de l'étude du système nerveux au point de n'être que médico-psychologique. Comme organe de diffusion d'une société médico-psychologique, elle instaure en France un lien de publication entre la recherche et la diffusion entre la société et la Revue.Mais la psychologie n'est encore ici conçue qu'à travers l'approche médico-psychologique.

II) 1876, les modèles de la psychologie pathologique et de la psychologie physiologique
Théodule Ribot[2] lance, sous l'égide de l'éditeur Germer Baillère à Paris en janvier 1876,

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[1] Geraud (M.), Bourgeois (M.), « Jules Baillarger (1809-1891), fondateur des A.M.P. », *Annales Médico-Psychologiques*, 1993, 151, n° 3, pp. 226-232 ; « 150 ans d'Annales Médico-Psychologiques », *op. cit*, pp; 232-239 ; « Les A.M.P. et la presse psychiatrique 150 ans après. Bilan 1992 », *op. cit.*, pp. 240-245.

[2] Thirard-Carroy (J.), « La fondation de la "Revue philosophique" », *La Revue philosophique.*, n° 4, oct-dec. 1976, pp. 401-413.

La Revue Philosophique de la France et de l'Etranger. Il est associé à Hippolyte Taine et Paul Janet. Dans un texte de présentation du numéro 1 de Janvier 1876 Th. Ribot précise que « notre Revue se propose de donner un tableau complet et exact du mouvement actuel sans exclusion d'école »(p. 1). Cet éclectisme se traduit cependant par la direction plutôt scientifique de la Revue qui veut rendre compte du «positivisme pur, l'Ecole Expérimentale qui compte des représentants en France et en Allemagne aussi bien qu'en Angleterre, le criticisme issu de Kant, le spiritualisme... s'inspirant de Maine de Biran »(p. 2). La restriction de l'éclectisme théorique est due au refus d'en revenir à une psychologie de l'introspection ou à une pure philosophie spiritualiste. A propos de la psychologie « on n'oserait plus prétendre qu'il suffit pour la faire de s'étudier intérieurement et l'on reconnaît, en général, que l'anatomie, la physiologie, la pathologie mentale, l'histoire, l'anthropologie sont pour elle d'une utilité directe et immédiate... »(Id.). L'influence d'Auguste Comte aura donc transformé les conditions du travail psychologique. Mais plutôt que de fonder une revue de psychologie physiologique, le projet est, comme le fera James, de transformer la philosophie en y apportant les connaissances positives de la psychologie scientifique allemande et anglaise. Taine et Ribot vont développer la méthode pathologique pour fonder la psychologie pathologique[3]

Selon Mara Meletti[4], la *Revue*, serait le lieu d'un dialogue continu entre une psychologie scientifique et une psychologie philosophique. Pourtant dès le premier numéro, *La Revue philosophique* publie un article de W. Wundt « Mission de la philosophie dans le temps présent »(*R.P.*, 1876, tome 1, pp. 113-124) qui défend ce point de vue. La philosophie conserve la place de « science des sciences ». Contrairement à une stratégie éliminativiste mise en œuvre par Le Cercle de Vienne, W. Wundt refuse d'adhérer à « l'opinion que la philosophie a fini son rôle, qu'elle doit maintenant disparaître de la scène pour céder entièrement la place aux sciences expérimentales »(*Op. cit.*, p. 113). La philosophie ne peut être remplacée du point de vue de la méthode et du point de vue de l'objet par les sciences expérimentales. En effet elle reste investie de la mission d'unifier les savoirs dans une science universelle, cette « science des sciences ». Mais les sciences expérimentales ont permis de proposer des principes à partir desquels une reconstruction de la philosophie : « la science actuelle tend à une conception unitaire de l'univers... Il s'agit de tendre de nouveau vers le but que la philosophie allemande croyait avoir déjà atteint au commencement de ce siècle »(*Op. cit.*, p. 122). Le retour au projet universel de l'idéalisme allemand ne retient plus les solutions spiritualistes dans la mesure où les sciences expérimentales fournissent des résultats quantitatifs à la qualification des états mentaux et psychologiques. La philosophie devrait servir de régulateur au travail de la science : «En contrôlant les résultats généraux des sciences, en développant les méthodes scientifiques de leurs principes, elle (la philosophie) a devant elle un champ de travail qui s'étend toujours avec le progrès de l'expérimentation scientifique... Car c'est la tâche de la philosophie de maintenir les bons rapports avec les sciences spéciales en leur empruntant ce dont elle a besoin, la base de l'expérience, et en leur communiquant ce qui leur manque, l'enchaînement général des connaissances »(*Op. cit.*, p. 124). Il s'agit donc de distinguer le bon grain de l'ivraie : c'est-à-dire de ne retenir de la philosophie que sa méthode qui vise l'universel par une traitement logique des connaissances et de refuser à la philosophie ses contenus métaphysiques. Comme si les sciences expérimentales ne parvenaient pas à former une perspective englobante et logique suffisante pour représenter une conception unitaire.

La Revue publie aussi des travaux de psychologie physiologique qui présente un autre modèle que celui de la psychologie pathologique défendue par Taine et Ribot. La société de psychologie physiologique, créé en 1885 par Th. Ribot, P. Janet, J.M. Charcot et Ch. Richet, apparaît à partir du tome XX de l'année 1885 comme constituant le courant psychophysiologique de la *Revue* à travers ses comptes rendus. Se retrouve au sein de cette Société l'un des futurs fondateurs de *l'Année Psychologique* H.Beaunis, C.Richet (1885), Babinski, Pierre Janet, Manouvrier, Ochorowicz, Cotard, Herzen, Marie(1886)... Th.Ribot n'apparaît jamais dans

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[3] Carroy (J.), Plas (R.), « La méthode pathologique et les origines de la psychologie française au XIXe siècle », *Revue Internationale de Psychopathologie*, n° 12/1993, pp; 603-612.

[4] Melleti-Bertolini (M), *Il pensario e la memoria. Filsofia e psicologia nella "Revue philosophique" du Th. Ribot(1876-1916)*, Milan, Franco Angeli, 1991. Une version a été publiée dans la *Revue philosophique*, 1994.

les comptes-rendus écrits d'activité de la Société alors que la présence de Charles Richet est constante entre la période étudiée de 1885 à 1891 pour organiser le défilé des travaux psychophysiologiques : deux sortes de travaux reviennent, d'une part les travaux autour des sciences parallèles(hypnose, graphologie, suggestion mentale, somnambulisme, les hallucinations, les effets des drogues, changement de personnalité...) et d'autre part des travaux autour des sciences du cerveau (temps de réaction, le sens musculaire, les réflexes, l'hystéri-épilepsie, la mémoire des sensations musculaires, les circonvolutions temporales...). Conçu depuis 1881 par Ochorowicz, le premier Congrès International de psychophysiologie a lieu à Paris en 1889 autour de rapports de la philosophie et de la psychologie avec la neurologie et la psychophysiologie.

III) 1893 : Le modèle d'une neurologie psychologique

L'année 1893 est celle de l'affrontement éditorial de deux conceptions radicales de l'homme : La *Revue de métaphysique et de morale* refuse[5] des recherches de faits purement scientifiques comme celles de la psychophysiologie telles qu'elles ont été développées dans la *Revue philosophique*. Elle défend une conception spiritualiste qui est élaborée à partir des idées plutôt que des faits. L'autre conception est strictement scientifique à travers deux publications spécifiquement consacrées à la neurologie[6], *les Archives de Neurologie* et la *Nouvelle Iconographie de la Salpétrière*.

Les Archives de Neurologie sont une revue trimestrielle des maladies nerveuses et mentales; elles avait été créées par J.M.Charcot(1825-1893) en 1880. Désiré Borneville (1840-1909)[7] en était le Rédacteur en Chef. Son rôle fut majeur dans la description de l'hystéro-épilepsie et dans la publication des *Leçons*. de Charcot. Bonduel... font l'hypothèse de divergences entre Bourneville et Charcot comme cause de la création de la *Revue Neurologique*. Aussi Charcot a voulu réunir autour de *La Revue* l'ensemble les spécialistes de la neurologie de l'époque : Amidon, Ballet, Bitot, Blanchard, Bouchereau, Brissaud, Brouardel, Cotard, Debove, Delaslauve, Duret, Duval, Féré, Ferrier, Gombault, Grasset, Hervé, Joffroy, Keraval, Landouzy, Magan, Marie, Maygrier, Mierzejewsky, Neumann, D'Olier, Pierret, Pitres, Raymond, Regnard, Richer, Séguin, Strauss, Talamon, Teinturier, Thulié, Troisier, Vigouroux, Voisin. Les *Archives* venaient compléter *Le Progrès médical* (lancé en 1873 par Désiré Bourneville et qui jouait un rôle important dans la diffusion de la nouvelle médecine) qui servait de présentation officielle des thèses de Charcot. Ce sont deux membres de cette première revue qui fondèrent la *Revue Neurologique* qui étaient aussi les plus brillants élèves de Charcot. Edouard Brissaud (1852-1909) fût nommé interne des hopitaux en 1875 auprès de Lasègue et Charcot dont il fut l'interne en neurologie. Il assiste Charcot à la Salpétrière. A la mort de Charcot, L'Académie de Médecine le désigna pour assurer l'intérim jusqu'à l'élection de Fulgence Raimond(1842-1910). Clinicien et anatomopathologiste, il publia *Leçons sur les maladies nerveuses*, contribua au *Traité de médecine* de Charcot et Bouchard, et d'une *Anatomie du cerveau de l'homme* chez l'éditeur de la *Revue*., Masson en 1893. Le second fondateur de la *Revue*. est Pierre Marie (1853-1940) : nommé à l'Internat en 1878, il fut l'interne de Paul Broca, de Bouchard et enfin de Charcot, en 1882, l'année où sa chaire de clinique des maladies du système nerveux fut créée. Il fut chef de clinique en 1890 avec Joseph Babinski.

Le but de la *Revue*. était d'être un « organe spécial d'analyse des travaux concernant le système nerveux et ses maladies ». Les analyses devaient « être aussi exactes que possible, mais concises ». Afin de suivre le rythme de parution des mémoires originaux, la *Revue*. est bimensuelle. La dimension internationale de la revue, à l'instar de *La Revue philosophique*, permettait de faire l'inventaire d'articles publiés dans plus de soixante journaux de langues étrangères. Les premiers auteurs sont Charcot, Déjerine (contribution à l'étude des localisations sensitives de l'écorce), Brissaud, Raymond, Schterbak (Moscou), Pitres, Pierre Marie, Marinesco, Sherrington (sur une

[5] Merllié (D.), « Les rapports entre la *Revue de métaphysique* et la *Revue philosophique* : Xavier Léon Ribot, Lucien Lévy-Bruhl » *Revue de Métaphysique et de Morale*, n° 1-2, 1993, pp. 59-90.

[6] Bonduelle (M.), L'Hermitte (F.), Gautier (J.C.), « La revue neurologique, 1893-1993 », *Revue neulogique*, Paris, 149, 2, pp. 91-112.

[7] Poirier (J.), Signoret (J.L.), « Note historique. Désiré Bourneville (1840-1909) », *Revue neurologique*, 1991, 147, 11, p. 705.

action inhibitrice de l'écorce cérébrale)... Il n' y avait pas de concurrence avec *La Nouvelle Iconographie de la Salpétrière* fondée en 1888 sous le patronage de Charcot par trois de ses collaborateurs : Paul Richer (1849-1933), Gilles de la Tourette (1857-1904), et Albert Londe (1858-1917) : cette revue était complémentaire. De 1893 à 1914 les articles portent sur la nature syphilitique des maladies et le rôle de la paralysie générale, sur la pratique de la ponction lombaire et l'étude du liquide rachidien, sur l'introduction de la radiographie, sur les premières tentatives de la neurochirurgie. La neurologie psychologique prépare le développement de la neuropsychologie

IV) 1894 : le modèle du laboratoire de psychologie scientifique

Depuis 1892 et 1893 Alfred Binet (1857-1911) et H. Beaunis publient à Paris chez Alcan un *Bulletin du laboratoire de psychologie physiologique* Longtemps directeur adjoint du laboratoire de psychologie physiologique de l'Ecole des Hautes Etudes de la Sorbone, il apparaît avec le titre de directeur dans le numéro 1/ 1894 de *L'Année Psychologique*. Pour fonder cette revue il poursuit son association avec H. Beaunis alors directeur honoraire du même laboratoire. Th Ribot n'est pas écarté comme collaborateur d'autant qu'il est titulaire depuis 1888 de la chaire de psychologie expérimentale et comparée du Collège de France. Pierre Janet, contre Alfred Binet, sera choisi pour succéder à Th. Ribot en 1896. La chaire de psychologie expérimentale de la Sorbonne sera confiée à Georges Dumas en 1905.

Pourtant Alfred Binet collabore avec Victor Henri qui est attaché au laboratoire créé par W. Wundt en 1879 à Leipzig. Cette référence à la psychologie expérimentale est explicite dès le premier numéro par la publication de travaux signés en commun, A. Binet et V. Henri, sur la mémoire. Dans « l'Avant propos », A. Binet revendique le modèle du laboratoire de la psychologie physiologique tant en publiant, en réalité dès le tome 2 de l'année 1895, les travaux du laboratoire de psychologie des Hautes Etudes de la Sorbonne qu'en publiant un article fondateur du professeur E.B. Delabarre sur « Les laboratoires de psychologie en Amérique ». Cet article, traduit par A. Binet, sert de référence étrangère pour, nous en avançons l'hypothèse, fonder la liaison entre la psychologie expérimentale effectuée au laboratoire et la psychologie physiologique défendue dans *l'Année psychologique*. E.B. Delabarre explique comment l'intérêt pour la psychologie a « d'abord dérivé d'un intérêt général pour la philosophie plutôt que pour l'étude scientifique des phénomènes mentaux »[9]. Mais la philosophie a trouvé des réponses plus souvent dans la religion que dans l'étude des états psychologiques. Il aura fallu attendre le «nouveau mouvement scientifique », né en Allemagne sous l'influence de Lotze, de Fechner et de Wundt, pour voir la création du premier laboratoire de psychologie, celui de Wundt en 1879. Comme a pu le montrer Geneviève Paicheler[10], le premier laboratoire américain fut fondé à l'Université de Hopkins à Baltimore en 1881, par un élève de W. Wundt, G.S. Hall. Au moment du lancement de *L'Année Psychologique*, E.B. Delabarre décompte pas moins de 27 laboratoires de psychologie aux Etats Unis[11] : 3 créés en 1888, 3 en 1889, 4 en 1890, 2 en 1891, 5 en 1892, 4 en 1893, 6 en 1894. Cet inventaire paraît considérable au regard de la France où on ne compte en 1894 au moins quatre laboratoires de psychologie : celui de la Société de psychologie physiologique créée en 1885 par Th. Ribot, P. Janet, J.M. Charcot et Ch. Richet ; celui de psychologie de la Salpétrière dont P. Janet deviendra directeur en 1890 ; celui de psychologie de l'hôpital Saint Anne dont G. Dumas sera le directeur ; et celui dirigé par A. Binet et A. Beaunis à la Sorbonne. Le développement universitaire des laboratoires de psychologie ne fut pas systématiquement organisé en France. Par contre le lien entre un laboratoire et une revue est repris depuis l'introduction des comptes-rendus de la Société de psychologie physiologique dans la *Revue philosophique* en 1885. Mais A. Binet, en traduisant l'article de E.B Delabarre,

[9] Delabarre (E.B.), « Les laboratoires de psychologie en Amérique », trad. A. Binet, *L'Année psychologique*, n° 1/1894, pp. 209-255.

[9] *Op. cit.*, p. 209.

[10] Paicheler (G.), *L'invention de la psychologie moderne*, Paris, L'Harmattan, 1993, chap. III, pp. 87-130

[11] Capshew (J.H.), « Psychologist on Site : A reconnaissance of the hitoriography of the Laboratory », Beginning of the New Psychology in America, *American Psychologist*, feb. 1992, vol. 47; n° 2, pp. 132-142.

voudrait faire de *L'Année psychologique* la revue exposant l'organisation des travaux de laboratoire de psychologie. Le lien entre un laboratoire et une revue, à l'instar des deux publications annuelles américaines nées en 1894 *Etudes du laboratoire de psychologie de Yale* et *Bulletins de la Société américaine de psychologie*, devait s'établir en France avec la même fréquence. Du point de vue de l'organisation de la bibliographie de la littérature psychologique de *L'Année Psychologique* en 1894, plusieurs types de psychologie apparaissent : les traités de psychologie, les articles sur la psychologie générale, les méthodes psychologiques, la psychométrie, la psychologie individuelle et caractère, la psychologie scolaire, la psychologie criminelle et la psychologie comparée. C'est la première fois en France que les psychologies sont étudiées systématiquement, car la revue *Mind. A quaterly review of psychology and philosophy* éditée depuis 1876 distinguait dès son texte fondateur plusieurs psychologies : la psychologie comparée, la théorie psychologique appliquée à l'éducation, l'investigation physiologique, les maladies mentales. La *Revue philosophique* n'avait réalisé que le programme philosophique de *Mind*.

Dans son texte d'introduction au numéro 1/1894 de *L'Année psychologique*, H. Beaunis justifie ce découpage de la psychologie et défend la thèse d'une psychologie physiologique. L'observation et l'expérimentation seraient la cause de la disparition de l'introspection pure et de la modification de la psychologie. La science de l'âme est devenue « la *science des phénomènes psychiques*, conception nouvelle qui faisiat de la psychologie une science naturelle et une branche de la biologie ». H Beaunis étudie la constitution de la psychologie à travers son affranchissement définitif de la métaphysique : ceux qui y ont contribué s'appellent Wolff, Krüger, Schönfeld, Herbart, Beneke puis Taine, Spencer, Bain reprenant la tradition idéologique de Locke et de Condillac. La physiologie, de J. Müller, E.H. Weber, Donders, Helmoltz, aura permis d'édifier une psychologie rationnelle entièrement expurgée de toute subjectivité.

Les *Elemente der Psychophysik* de Fechner (1860) seraient la date de naissance effective de la psychologie. Les recherches sur le fonctionnement du cerveau, les localisations cérébrales, les sensations, les phénomènes d'inhibition ou encore la vitesse de transmission dans les nerfs prouvent que tout phénomène psychique a un « concomitant cérébral » qui lui correspond. Dans une telle mise en perspective la psychologie est une science naturelle des faits mentaux : « de là sont nées ces deux branches de la psychologie, la psychophysique et la psychométrie ». La mesure et le calcul sont les moyens d'un « travail complet, systématique, basé sur des recherches précises et méthodiques et d'avoir essayé d'en formuler les lois ». Le cerveau est le modèle épistémologique de la science psychologique dont *L'Année psychologique* se veut l'exposé. Se référant à *Brain*, créé par J.C. Bucknill, J. Crichton-Browne, D. Ferrier et J. Hughlings-Jackson en 1879, H. Beaunis partage t-il la position réductionniste des fondateurs selon laquelle « les fonctions et les maladies du système nerveux seront discutées à la fois dans leurs aspects psychologiques et physiologiques »[13] ?

La différence est mince entre la psychologie expérimentale, qui prépare avec *Brain* la neuropsychologie, et la psychologie physiologique. H. Beaunis propose de retenir la méthode des laboratoires de psychologie afin de faire de la psychologie une « science d'observation et d'expérimentation, c'est-à-dire une véritable science, comme les autres sciences naturelles ». Il s'agit de constituer « la science de l'homme » sur le lien du psychisme et des organes dont en particulier le cerveau. La psychologie devrait servir à la constitution de la science de l'homme sur laquelle pourront se construire les sciences sociales, l'éducation et la criminalité.La référence classique à W. Wundt ne conduit pas *l'Année psychologique* au même sens de psychologie physiologique : là où la philosophie devait pour Wundt être « la science des science », la psychologie est la bibliothèque scientifique de l'homme sans laquelle aucuns phénomènes ne pourrait s'expliquer.

Durant cette fin du XIXe s'opèrent avec S. Freud et W. James[14] des déplacements de la psychophysiologie vers la subjectivité psychique que *L'Année psychologique* refuse au nom de car la psychologie physiologique devient la psychologie scientifique.

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[12] Beaunis H. « Introduction », *l'Année psychologique*, 1/1894, pp. III à VII.

[13] « Introduction », *Brain : A journal of neurology*, Londres, MacMillan and Co, 1879, vol. 1, p. I.

[14] Andrieu (B.), « S. Freu et W. james : Déplacements de la psychophysiologie », *Revue Internationale de psychopathologie*, n° 13/1994, pp. 83-102.

ALFRED BINET ET LA PEDAGOGIE

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SUMMARY : The very varied works of Binet are made up of three aspects : psychopathology, experimental psychology, education.
This paper deals with the third of these three aspects.

L'oeuvre de Binet frappe d'emblée par la variété des problèmes dont elle traite ; on l'a même parfois suspectée de quelque dispersion et considérée comme constituée de thèmes disparates qui se succéderaient sans unité : centrée sur la psycho-pathologie, une première étape serait surtout marquée par ses ouvrages comme la *Psychologie du raisonnement* ou les *Altérations de la personnalité*. Plutôt réservée à la psychologie expérimentale, la deuxième coïnciderait avec la direction du laboratoire de psychologie physiologique de la Sorbonne et la publication de l'*Introduction à la psychologie* expérimentale et de l'*Etude expérimèntale de l'intelligence*. La troisième, enfin, serait consacrée à la psycho-pédagogie.

Apparemment pratique et vraisemblable, cette distinction chronologique ne manque pas d'être trompeuse. D'une part, elle laisse échapper les raisons auxquelles tiendraient la succession des phases supposées et le glissement d'intérêt qu'elle impliquerait ; elle ne permet pas, en particulier, de comprendre ce qui aurait amené Binet à la troisième ; d'autre part et surtout, une étude plus attentive révèle la constance et la permanence, donc la simultanéité, des trois séries de préoccupations.

Cependant, la place donnée à chacune a varié avec les années, même si l'émergence d'un nouvel axe de recherches n'entraîne ni l'oubli, ni la négligence des précédents. En particulier, Binet traite des problèmes pédagogiques plusieurs années avant de connaître la Société libre, et la place qu'il leur réserve ensuite ne lui fait pas négliger la psychologie pathologique, à laquelle il consacre encore, dans les trois dernières années de sa vie, plusieurs mémoires publiés dans l'*Année psychologique*. Si, donc il s'intéresse de plus en plus à l'éducation, ce n'est pas fortuit, mais en fonction même de la dynamique de sa pensée, marqué par la conviction que "l'expérience" est le seul critère que la psychologie moderne puisse retenir.

On montrera quels cheminements ont conduit Alfred Binet à l'étude de l'enfant, quelles problématiques il a été, à ce titre, conduit à retenir, selon quelles étapes et quelles procédures il a engagé ces recherches et quelle place y a tenue la "Société libre pour l'étude psychologique de l'enfant". On s'efforcera en particulier de saisir comment il a entrepris d'élaborer une "pédagogie scientifique", mais aussi en quel sens il l'a entendue.

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THE TRANSFORMATION OF THE SOUL INTO PSYCHE

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The second half of the 18th century is a fascinating period for anyone, who is interested in the genesis of the social and human sciences. In Europe then the socio-economic structure changed by the arrival of industrialization, the upper levels of the bourgeoisie (middle classes) conquered power on a national scale, and expressed a new consciousness of man, of the body and society by developing fresh and powerful paradigms which were going to serve as starting points for our present sciences of man and society. Amongst these emerged proto-psychology, i.e. the change from a rather vague and primitive "Seelenkunde" into a body of knowledge, in which not the traditional idea of soul, but a more plastic concept of the psyche led the way.

What did the old concept of soul contain in the beginning of the 18th century? That concept was in the tight place of a christian culture and of a deontic conception of reality: the conception of mind was imbued with a religious perfume, and what happened with men and things was not very factually interpreted but rather as an event in a history of destination. The life of the mind took place on the borderline of epistemology and ethics, with here and there first signs perceivable of an enlarged poetic doctrine of beauty, focussed on passions and affections (Affektenlehre in the theory of music, esp. opera). The soul was traditionally conceived of as the place of knowlegde and consciousness, mostly interpreted cognitively, a surprisingly stable center, not withstanding the waverings of the mind. Surrogate captain on the bridge, the Ego or 'I' laid the course of conscious life but without being pivotal. Under the influence of British philosophers the self, interpreted as continuity in the stream of impressions, sensations and inclinations (as commanded identity in the person) ranked as insufficient. That real men exist bi-sexually did not count philosophically, because "Geschlächtlichkeit" was conceived of as only the superficial appearance of the body, in spite of the ostentatious second-rate position of women in society. Ordinary appearence, philosophical thinking, and societal life were sharply marked out areas of reality, unconnected.

Which powers brought about the fundamental change in the old-fashioned conception of soul and propagated the advancement of a much more dynamic, deepened, no longer a-sexual psyche? Shortly resumed, that innovation resulted out of societal processes and secularization of christian culture and out of the emergence of new aesthetics, the appearence of Nature in a new figure, and the crumbling away of a selfevident ethic - deontic

order; but especially this innovation was connected with the enticement by another idea of art: Romanticism.

Paying attention to starting point, intermediate stage and ultimate result, one can delineate in an ideal scheme (ideal-typisch) how this transformation came about. For the sake of the argument I neglect the comparitive dissimilarity of ambiance, the artistic or more or less scientific language-areas, the religious background and origins in city-life respectively country-life. Intellectually and politically there existed between German speaking countries. France and Great-Britain so much traffic, that finally homogeneous results were effected everywhere.

WUNDT AND POLITICS

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Although Wilhelm Wundt (1832 - 1920) regarded politics as the "most powerful motive" of his life (Wundt, 1920, p.IV), this aspect of his career continues to be misinterpreted by historians of psychology. The present paper will reconstruct Wundt's political activities as President of the Heidelberg Workingman's Educational Association (1863 - 1866) and as Elected Member of the Second Chamber of the Parliament of the Grand - Duchy of Baden (1866 - 1869) on the basis of archival and documentary sources. The implications of Wundt's political views will be discussed within the context of current research on the political role of German academics before World War I.

Wundt and Politics

Historians of Psychology generally regard the 1879 establishment of the Institute for Experimental Psychology at Leipzig University as the greatest contribution of Wilhelm Wundt (1832 - 1920) to the development of modern psychology as an autonomous science. Therefore, it must come somewhat as a surprise to read in Wundt's autobiography, Erlebtes und Erkanntes (1920, p. IV) that he viewed his active participation in the political affairs of his native country, the Grand - Duchy of Baden, as the "most powerful motive" of his life.

With few exceptions noticeable exceptions (Brock, 1992; Ungerer, 1980), historians of psychology minimized or misrepresented Wundt's political career. The earliest detractor was Edward B. Titchener (1867 - 1927), who considered himself as Wundt's special envoy to the English - speaking World. He stated bluntly that Wundt (Wundt, 1921, p.255):

> ...had not fully learned to know himself. He tells us in the Preface [of his autobiography] that the cast of his mind was political, and that its leading motive was a desire to further state and society. There is no doubt a grain of truth in this characterization...In the

> main, however, the statement derives from a very natural illusion...[Wundt's] own political bent belongs without question to a partial or subordinate life, whose brief periods of dominance are separated by long stretches of scholarly absorption. (p.255).

Much more recently, Stubbe criticized Wundt's political activities in his essay, Wundt und die Herero (1992). After first castigating Wundt for his indifference to the "ethnocide [of the Herero]"by the German colonial forces in Southwest Africa (Namibia), he returned to Wundt's early political career in a long content footnote (Stube, 1992):

> As far as politics are concerned, one can characterise Wundt as a "bourgeois socialist" (who did not participate in the move [of his fellow socialists] to the Marxist camp)... as a later member of the "Baden Progressive Party" (A.Lamey), [Wundt] was (filled] with liberal civic - mindedness...[In] the Second Chamber of Baden...from 1866 - 1869, ...[Wundt] worked primarily for the introduction of the draft. [During this period], Wundt changed from a convinced opponent of Prussia into an admirer of Bismarck ([whom he called] "Messiah of the North")... (p.135).

The present research made extensive use of Wundt's autobiography and other memoirs from the period. Archival material has been utilized from the resources of libraries and archives in Amsterdam, Duisburg, Heidelberg and Karlsruhe.

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It is our purpose to provide new factual information about Wundt's actual political activities between 1863 and 1869 in the four sections of our essay.

Part I. will provide necessary background information about the political situation in Germany and, especially, in Baden during the crucial decade before the 1871 unification of Germany (Becker, 1973; Kistler,1957).

Part II. describes the origin, activities and development of the Heidelberg Workingman's Educational Association (Balser, 1959, 1962) with special emphasis on Wundt's role in this group. His contacts with leaders of early socialism like Friedrich A. Lange (1828 - 1875), Ferdinand Lassalle (1825 - 1864) and August Bebel (1840 - 1913) will attract special attention.

Part III chronciles and analyzes Wundt role in the Baden Parliament from 1866 - 1869 (Becker, 1973; Gall, 1968). A detailed analysis of legislative successes and failure, as well as the reasons for Wundt's resignation from parliament will be included.

Finally, Part IV will discuss Wundt's political career within the context of recent writings about the political involvement of German academic "mandarins" during the second half of the 19th century (Ringer, 1969).

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Bullough, Burt and the BBC: Binet's Influence on Psychological Aesthetics in Britain

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Abstract

The British Psychological Society supported an Aesthetics Section between 1922 and 1937. By far the most influential idea in psychological aesthetics at this time was Edward Bullough's concept of the 'four perceptive types'. It is possible to trace the origins of these four types back to Binet's *Experimental Study of Intelligence*, through the work of Charles Valentine, Cyril Burt and Hans Eysenck.

The discourse of psychological aesthetics both expanded and contracted during these years. It expanded because not only did psychologists forge alliances with art critics and writers, but also because it invested new domains of subjective experience with aesthetic significance. It contracted because that alliance encouraged the increasingly reified seeking of the *essential* component of aesthetic appreciation. This paper traces Binet's influence on the discourse, and considers why it became a self-subverting enterprise.

Bullough, Burt and the BBC: Binet's Influence on Psychological Aesthetics in Britain

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Introduction

It may come as a surprise to those unacquainted with the history of British psychology to learn that during the inter-war years the psychology of aesthetics was considered an important domain of study. Moreover, such persons may find it equally remarkable that in this esoteric field the work of Alfred Binet was regarded by some of its practitioners as of primary importance. Although the work of the Cambridge linguist Edward Bullough is doubtless unfamiliar to many, I am sure that many in this audience will be aware of both the work of the notorious Cyril Burt, as well as that of his student Hans Eysenck, whose doctoral dissertation on the psychology of aesthetics Burt supervised. Bullough, Burt and Eysenck all owed debts of acknowledgment to Binet, although they each adapted the latter's system to suit their own requirements. The purpose of this paper is to discuss the story of Binet's influence on the psychology of aesthetics in Britain, and to argue that a comprehensive account of that discourse must include a recognition of both the popular and professional dimensions of British psychology at this time.

The Aesthetics Section

The British Psychological Society established a forum for psychological aesthetics in 1922. The Aesthetics Section was by far the smallest, existing somewhat uneasily in the company of the General, Medical, Industrial and Educational Sections. Edward Bullough had published on aesthetics in the *British Journal of Psychology* since 1907, and in 1917 he assumed editorial responsibilities in the journal for papers

in the area. He was thus the obvious choice for the Section's first chairman. His primary professional concern was modern languages, but he had initiated a course of lectures on his 'intellectual hobby' - aesthetics - in Cambridge in 1907.[1] His notion of 'psychical distance' has subsequently enjoyed a wide audience in philosophical aesthetics, but it was his interpretation of some of Binet's work that was most influential during the interwar period.

In his *Experimental Study of Intelligence*, Binet had distinguished four types of apprehension: the *type descripteur*, the *type observateur*, the *type d'érudit*, and the *type imaginatif* or *émotionel*. Although Bullough suggested that only the last two of these four types had 'aesthetic significance', he utilized all four to construct his own system of aesthetic apprehension. Bullough's system then, employed an objective type, a physiological type, a character type and an associative type.[2] Admitting that Binet's original work had been done with children, and was moreover unconcerned with aesthetic questions, Bullough nevertheless employed Binet's system to create his own. His aesthetic types referred to modes of perception, or more specifically, to modes of appreciating single colours. These were not personological categories: a person could be described as exhibiting two of the modes simultaneously, for example. Moreover, a one-to-one correspondence to Binet's types could not be made because, for example, Bullough's 'associative type' incorporated three of Binet's types; the *observateur*, *émotionel*, and *érudit* types.[3]

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[1] Alix Florence attended these lectures. She was later to marry Freud's translator James Strachey.

[2] Edward Bullough (1910) The 'Perceptive Problem' in the aesthetic appreciation of simple colour-combinations. British Journal of Psychology III, 406-47.

[3] Edward Bullough (1912) 'Psychical Distance' as a factor in art and an aesthetic principle. British Journal of Psychology V, 87-118.

Solidifying the Discourse

Although there is then, a certain intentional plasticity evident in Bullough's apperceptive types, there is nevertheless the beginnings of a 'concretization' of the discourse, a 'psychologizing solidification'. Maintaining that the four perceptive types were adaptive, Bullough suggested that the modes determined a person's aesthetic responses across a whole spectrum of art objects. The aesthetic experience was like looking through a coloured glass

> through which one sees all objects steeped in a special colour, and this colour is precisely the aesthetic quality which the objects seem to display. It is irrespectively of the objects themselves, an aesthetic adaption, and resembles the whole groundwork on which the aesthetic experience rests.[4]

The understanding was that there was a qualitatively particular *aesthetic* experience, and it was the job of experimental psychology to investigate its parameters.

Charles Valentine would come to be remembered chiefly as an influential educational psychologist, but during the teens and twenties he was an important figure in the psychology of aesthetics. In his *Introduction to the Experimental Psychology of Beauty* (1913), Valentine extended Bullough's work in a number of crucial directions. It was at this point that the intellectual acknowledgment to Binet was dropped, and Valentine extended the apperceptive types to include musical appreciation. He claimed that people's responses to both simple musical intervals and compositions could enable the experimenter to classify them according to (by now 'Bullough's') four perceptive types. The discourse was both expanding, into new psychological territories, and

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[4] Edward Bullough (1907) The Modern Conception of Aesthetics: A Course of Lectures Delivered Autumn 1907 in the University of Cambridge. Privately Printed. pp. 93-4.

contracting, into the search for the essentials of the aesthetic experience.

Psychology and the Avant Garde

Such formalist assumptions underpinning the experimental work not only permitted a whole series of experiments on the combinations of colours, the arrangements of shapes, and musical elements and the like, but they also allowed the psychologists to align themselves with avant garde critical art theory. Art critics like Roger Fry and Clive Bell adhered to similar reductive and formal notions of the aesthetic. For them too, the aesthetic question was essentially a *psychological* one. Roger Fry and Herbert Read were only two of the many critics and writers who presented papers to the Aesthetics Section. In this way then, psychology's scientific ambitions brought it into line with contemporary art theory; an irony that was eventually to subvert the enterprise of psychological aesthetics.

Valentine had also bequeathed another psychological category to the discourse, one that the art critics wholly endorsed: emotion. For Bullough, emotion was only one possible component of the aesthetic experience, but for Valentine it was the primary discursive binding force. Formalist aesthetics were conjoined to the affective domain: psychology's task was to investigate the shapes, colours, and tones that gave the subject the greatest *pleasure*.[5]

Cyril Burt Speaks to the People

During the early 1930s, Cyril Burt was Britain's most well known psychologist. His weekly broadcasts to the nation on BBC radio were reprinted in *The Listener* magazine. His talks ranged from the 'Psychology of Politics' to the 'Psychology

[5] Charles W. Valentine (1913) An Introduction to the Experimental Psychology of Beauty. London: T. C. & E. C. Jack.

of Religion' to 'Criminals in the Making', and being well read in the psychology of aesthetics Burt devoted a number of talks to this topic. His talk on the 'Psychology of Art' was structured around Bullough's four perceptive types.[6] By now though, the types had not only become infused with an affective dimension, but they had also acquired a hierarchy of value. Burt also extended Valentine's reification of the four types, by curiously maintaining that true aesthetic experience was only available to certain persons, depending on which type they were. Most people gave not aesthetic judgments when viewing paintings, he argued, but personal judgments. Only the intellectual type of observer could appreciate beauty in its 'objectivity'.

Even the paintings chosen to illustrate the article were framed within an affective value hierarchy. Dicksee's 'Romeo and Juliet' was 'a good example of the associative type' whose choices were determined by the associations a picture arouses. 'This motive comes most plainly to light in the comments made by children', and more so in women than in men. Spencer's 'Resurrection' was chosen to illustrate the physiological type: 'Pictures are preferred because of the simple instincts they excite.' While a Van Gogh illustrated the character type, an abstract composition by Paul Nash had been chosen to represent the ('rarest') objective type. 'The persons in this group adopt an attitude that is intellectual and critical rather than emotional.'

In the article Burt recalled an experiment he had performed 'some years ago, to test the artistic preferences among different types of people.' Having asked children and academicians alike to sort 50 picture postcards in order of preference, Burt claimed that 'a statistical analysis of the results proved clearly that one fundamental factor, buried among a pile of others, was influencing every person tested.'

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[6] Cyril Burt (1933) The Psychology of Art. The Listener, 25th Jan. 138-40.

This was exactly the finding reported in Hans Eysenck's 1940 doctoral dissertation 'An experimental and statistical investigation of some factors influencing aesthetic judgment.' Eysenck claimed to have isolated both a T-Factor and a K-Factor of aesthetic appreciation, based on Spearman's model of intelligence.[7] The psychology of aesthetics had reached its nadir.

Conclusion

The British Psychological Society was engaged in a number of professionalizing enterprises during the 1930s, and this, together with the reductio ad absurdam character of the psychology of aesthetics had compromised the latter's validity.[8] The Aesthetics Section was abolished in 1937.

Beginning with Binet's appropriation by Bullough in 1907, the psychology of aesthetics in Britain was ever more reified during the short years of its existence. The discourse both expanded, by opening up new psychological regions for scrutiny, and contracted, by assuming the existence of increasingly essentialist aesthetic factors. As Clive Bell had argued that 'significant form' was the essential factor of aesthetic experience, Hans Eysenck proposed that the T-Factor of aesthetic experience had achieved statistical significance. It was uncomfortable alliances of this sort that ultimately exposed the fragility of the psychology of aesthetics in Britain. It was the tactic of self-subversion.

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[7] Hans Eysenck (1940) The general factor in aesthetic judgments. British Journal of Psychology, XXXI, 94-102, and Hans Eysenck (1941) 'Type'-factors in aesthetic judgments. British Journal of Psychology, XXXI, 262-270.

[8] A Professional Status Committee was set up in 1934 for example. In 1936 the Society initiated the process of seeking Incorporation status, and in 1939 the Society sent a register of the qualifications of its members to the government offering its services to the war effort.

BINET IN SPAIN: HIS EARLY INFLUENCE IN SPANISH EDUCATIONAL PSYCHOLOGY

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The aim of this paper is to assess the influence of Alfred Binet's psychopedagogical investigations in early 20th century Spanish psychology. Translations of his work as well as those books and papers dealing with it that were published in Spain have been examined. Certainly, Binet's research on intelligence and other basic psychopedagogical subjects has had a universal relevance, and its echoes were not missed by Spanish psychologists and pedagogues.

TRANSLATED WORK

Only eight out of Binet's numerous writings were translated into Spanish in the period here considered (1900-1936). The extended knowledge of French among Spanish intellectuals at the time made translations practically unnecessary. This may account for the scarcity of translated work.

Apparently, Binet's first writing appearing in Spanish was a paper on "Recent work on child psychology and pedagogy" (Binet, 1895). This is a minor publication not including any of Binet's most personal thinking, but reflecting instead its author's concern to inform the scientific community on the current state of affairs in science -an aim he was later to pursue through L'Année Psychologique.

In the following years, several other works were also translated: Introduction to Experimental Psychology (Binet, 1899); The psychology of Reasoning. Experimental Research through Hypnotism (Binet, 1902); Fetichism in Love (Binet, 1904); The Soul and the Body (Binet, 1907); Modern Ideas on Children (Binet, 1910); Abnormal children (Binet & Simon, 1917); and Measuring Intelligence Development in Children (Binet & Simon, 1918).

It may be worth mentioning that some of Binet's best known works on intelligence and mental fatigue did no deserve any Spanish translation.

THE IMPACT OF THE METRIC SCALE OF INTELLIGENCE

Binet's Metric Scale of Intelligence is a paradigmatic achievement of 20th century scientific psychology. As an instrument founded on well established data and making possible a planned psychopedagogical intervention (particularly as to its therapeutical aspects), it was spread throughout the world. Spanish editions of the test were all included in research works and manuals mostly devoted to child psychopathology.

The most concerned author with the study of Binet seems to have been A. Anselmo González, a physician and a pedagogue who

taught abnormal pedagogy at a Higher School for Teachers in Madrid (Carpintero, 1994). In his Abnormal Children Diagnostics (Anselmo, 1914), Anselmo values Binet- Simon's diagnostic test higher than any other, and goes as far as considering it as a basis for a modern pedagogy. The book includes the questions, plates and instructions for its application, and also some very brief explations or presentations. A reply to the Spanish critiques to the method at the time is also included. Anselmo also promoted a massive application of the test in the schools of Madrid.

In his book on Mentally Abnormal Children (Lafora, 1917), Gonzalo Rodríguez Lafora, an outstanding Spanish psychologist and psychiatrist,
reviews the existing methods for assessing child mental ability and diagnosticating abnormality. A whole chapter is devoted to Binet's Metric Scale. Lafora includes the required material for its application, as well as a critique of the method. Mention is also made to the methods devised by Sante de Sanctis, Weygrandt and Porteus, as well as to Yerkes-Bridges' and Terman's modifications to Binet's tests. The latter, "Terman's method", is presented as a definitive improvement.

Binet's test and plates are also included in D. Barnes' Experimental Psychology in French Pedagogy (1921), although no indications are given as to their use. Orellana's edition of the test is presented in the translation of Binet and Simon's paper "Measuring Intelligence Development in Children" (Binet & Simon, 1918), this probably being its most complete Spanish version. Not only does it include a detailed description of the tests, but also the plates, drawings, questions, requirements for its preparation, and instructions for its use and assessment.

R. Tomás y Samper, a Spanish psychologist and pedagogue, wrote a book on Psychometry in Primary School (1937) where the different methods for ability and intelligence assessment in primary school were analyzed. Although Binet's test is here briefly presented, more attention is paid to Terman's version of it, which is considered as more suitable.

Finally, an incomplete summary of Binet's test may be also found in the translation of Alice Descoeudres' book on Abnormal Children Education, where other methods for dignosticating mental deficiencies are also mentioned.

ON BINET'S THOUGHT

A few years after his Diagnostic of Abnormal Children, Anselmo González devoted a Pedagogic Reading Book to Binet. Binet's biography, research and work are here reviewed, and its main pedagogical implications analyzed. In this brief book, Anselmo makes a strong defence of Binet's achievements against those who criticized the Metric Scale in favor of new versions. Anselmo describes Binet as highly self-critical, ever searching for improvement and open to criticism. Yet, Anselmo refused to accept any modification to Binet's work. Some of them, however, proved later to be fruitful and would have probably been accepted by Binet

himself.

Binet is also many times mentioned in D. Barnes' handbook on the Sources for the Study of Pedology (1917). As many as 37 of Binet's writings published in L'Année Psychologique from 1894 to 1911 are here referred to. They were a result of Binet's research while heading the psychological laboratory at La Sorbonne, where he collaborated with Ribot, Beaunis and Simon. Of all these works, Barnés briefly summarizes some of the most relevant: "New Methods for the intellectual diagnostic of the abnormal" (1904), "On the Measure of Intelligence" (1905), "Imbecile Intelligence" (1908) and "New researches on the measure of school-children intellectual level" (1911), where the final version of the Binet-Simon test was presented.

In the period under study, Binet became one of the most commented on psychologists in Spain. References to his work in Spanish psychological literature are indeed very frequent. Thus, Pavlov, Toulouse and Binet are often referred to by F. Santamaría when dealing with psychological practices amenable in classrooms (1912). Binet is also one of the most cited authors in M. Navarro Flores' Handbook of Experimental Psychology (1914), together with Simon, Weber and Fechner (Carpintero, 1994). This shows the importance attached by this author to psychophysical and intelligence research. Also concerned with Binet's contributions was M. Arnáiz, who devoted a second volume of his Elements of Psychology Founded in Experience (1914) to the study of intelligence. Finally, in a prologue to Binet's Introduction to Experimental Psychology (1899), J. Besteiro sees the translation of this book as a clear sign of a growing interest in experimental psychology in Spain.

THE APPLICATION OF BINET-SIMON'S METRIC SCALE OF INTELLIGENCE IN SPAIN

The Binet-Simon Metric Scale of Intelligence was applied in schools by students of abnormal pedagogy at the Higher School for Teachers in Madrid. More than 20.000 observations were made (about 200 per student), and a systematic study on the applicability of the tests and the results of their application was carried out by the students Raimundo and Rodriguez Mata.

After these studies, Anselmo González concluded that similar results were obtained when applying Binet's test in Madrid and Paris. The problems requiring a higher intellectual level were indeed the same in Paris and Madrid. Likewise, both in Madrid and Paris the number of children giving right answers to most questions was above an 80 per cent.

Rodríguez Mata and Raimundo examined 279 children from six to twelve years in several schools of Madrid. They found the test very useful for establishing the abnormality or retardment of a child, as well as for acknowledging the mental development of normal children and grouping them together according to their mental level. Among their conclusions, they pointed out to the existence of retarded children in Madrid and to the need of providing special

centers for educating them properly.

Anselmo thought the simplicity of the application of the test and its efficiency for diagnosticating mental abnormality made it possible for schoolteachers to use it with no support from the psychologist. Thus,
while in Paris the test had been applied through a period of time of more than twenty years by a team of psychologists collaborating with Binet, and even then Binet himself was not sure it did not need any further modifications, in Spain it was applied by science education students without any guide from a psychologist. Nevertheless, Anselmo was certain about its efficiency and refused to accept the new versions of the test carried out after Binet's death.

SOME SUGGESTED MODIFICATIONS

Rodriguez Mata thought it necessary to modify Binet's test for its better application in Spanish schools. In his Diagnostic of Abnormal Children, Anselmo González reviews these suggested changes and finds that some of them were already taken under consideration in Binet and Simon's latest version of the test; others, he says, are not significant. To Anselmo, thus, the method ought to be applied as it stood. Rodríguez Mata also admitted the solid foundations of the Binet-Simon method, but thought some modifications affecting minor details might improve its application in our country.

In his translation of the test, also Lafora suggested a number of minor modifications. They mostly apply to the translation of terms, since some of them were not familiar to Spanish children. Thus, the term "car driver" was used instead of the original "bicycle rider", not so well known to children in small villages.

It must be noted, however, that other significant difficulties, such as the scenes of Paris everyday life portrayed in the plates, were totally ignored by the translators and therefore remained unchanged.

ANSELMO GONZALEZ AGAINST THE REVISIONS OF THE METRIC SCALE OF INTELLIGENCE

The application of Binet's test in the United States inspired a great number of publications on the subject. Also, a series of modifications were made to the test which resulted in the now well-known I.Q. tests. When Terman proposed his early variations to the Binet-Simon's test, some Spanish psychologists, like Lafora, preferred it to the original.

Anselmo Gonzalez thought the Metric Scale of Intelligence to be a much more useful tool than any other, and replied to those preferring Terman's
revision that they had not taken into account Binet's 1911 final version of the test. Furthermore, he criticized Binte's critics for not having applied either test, Binet's nor Terman's.

One of the most compelling critiques made to Binet's test at

the time was made by the Italian psychologists Treves and Saffiotti at the Brussels Pedology Congress (1913). This critique was grounded on the failure of its application in Milan and Lyon schools. According to the critics, the test was unable to measure what they called an "uneducated" intelligence. Anselmo rejects this critique and replies that the the test was originally designed for students of particular schools; its results could therefore vary depending on the circumstances surrounding the subjects of study.

Thus, Binet himself seemed to have acknowledged the socio-cultural limits of his Metric Scale. Surprisingly enough, however, Anselmo failed to notice that such an orientation of the test toward a specific subject profile rendered its universalization problematical. Therefore, whether or not the results of its application were satisfactory, a thorough review of the test was required prior to its application.

CONCLUSION

Binet's test enjoyed a wide early acceptance in Spain. A great number of translations were made and it was widely applied in Spanish schools. Spanish psychologists soon rejected Binet's test, however, and favored Terman's version instead. Nonetheless, Binet continued being one of the most appreciated authors in pedagogy. Still, Anselmo González, who favored a wide application of the tests, refused to accept this criticism and continued defending the feasibility of the original test and its application.

The strong cultural element of intelligence tests is nowadays widely acknowledged. It must be thus taken into account prior to their application. This element was not properly considered by Binet himself, nor most significantly by those who translated and applied Binet's test in Spain.

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LA "VERITE EN MARCHE" ET L'HYPNOTISME : UNE SOMNAMBULE DREYFUSARDE?

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Abstract

In 1899, there was a controversy between the pro and anti Dreyfus factions regarding the influence of the somnambulist Léonie - well known in the history of French psychology - upon Dr Gibert's position in the Dreyfus matter.

Dr Gibert was a respected physician from Le Havre, and a friend of the President of France, Félix Faure.

This issue raises the question of the commitment to the truth of the protagonists.

"La vérité, n'importe par quelle bouche; le bien, n'importe par quelles mains"

(devise du *Journal du magnétisme*, 1845)

Le 13 janvier 1898, dans *L'Aurore*, journal républicain de gauche, Zola écrit "J'accuse" sous le coup de l'indignation suscitée par l'acquittement du commandant Esterhazy, contre lequel les présomptions se sont accumulées.. A propos de Du Paty du Clam, qui avait mené en 1894 les premiers interrogatoires d'Alfred Dreyfus, et qu'il présente comme l'instigateur de l'Affaire, il affirme : "... il n'y a d'abord que le commandant Du Paty du Clam, qui les mène tous, qui les hypnotise, car il s'occupe aussi de spiritisme, d'occultisme, il converse avec les esprits". A cet accusateur "spirite", il oppose implicitement "la vérité en marche". Les caricatures dreyfusardes de l'époque la représentent comme une femme nue qui sort d'un puits sombre. Ainsi s'opposent le monde de l'ombre - sous-entendu le monde du mensonge et de l'occultisme - et celui de la lumière - sous-entendu de la justice et de la science, à l'image du duel, réel et symbolique, où s'affrontent les militaires Picquart et Du Paty du Clam, "...l'un le visage

découvert, l'autre masqué"[1].

La récurrence de ces poncifs correspond à un infléchissement de l'Affaire. De 1895 à 1897, seuls quelques "initiés", disposent de preuves tangibles, qu'ils ne peuvent, parfois, pas divulguer. Janine Ponty [2] montre qu'à cette époque, être dreyfusard, c'est souvent faire confiance et accorder crédit à l'autorité morale que représente, par exemple, le sénateur alsacien protestataire Scheurer-Kestner. A partir de 1898, l'innocence de Dreyfus n'est plus seulement, pour les dreyfusards, une question de croyance ou de conviction, mais elle engage plus fortement qu'auparavant un rapport à la vérité, d'où le succès de la formule de Zola :"la Vérité est en marche et rien ne l'arrêtera". Ce moment correspond à la mobilisation massive d'un certain nombre d'intellectuels, terme que Clémenceau popularise à l'époque. L'engagement dreyfusard de nombre d'entre eux s'inscrit alors dans le droit fil de leur pratique scientifique. C'est le cas par exemple du sociologue Durkheim et de ses élèves[3] ou encore de l'historien chartiste, d'origine protestante, Gabriel Monod, tenant de la critique historique, le premier universitaire à avoir demandé, en solitaire, la révision le 5 novembre 1897.[4]

Or, qu'en est-il de ceux des "amis de la vérité" qui pratiquent l'hypnotisme et en sont des adeptes? Certains, comme Charles Richet, franc-maçon, physiologiste, initiateur de Charcot à l'hypnotisme, ont séparé leur intérêt pour le somnambulisme et la métapsychique, et leurs engagements dreyfusards. Il en va de même pour Hippolyte Bernheim, porte-parole de l'Ecole de Nancy, juif originaire de Mulhouse, comme la famille Dreyfus. D'autres, comme Janet, semblent être demeurés dans une réserve prudente. Mais n'est-il pas paradoxal d'étayer peu ou prou ses convictions à partir des affirmations d'une somnambule? Telle est la question qui s'est posée à propos du Docteur Joseph Gibert, l'un des dreyfusards de la première heure. Nous nous proposons, à travers l'analyse de "L'affaire Gibert", qui fit la "une" des journaux entre le 24 mars et le 15 avril 1899, de montrer comment s'aiguisent des contradictions, se manifestent des paradoxes et s'instaurent des lignes de démarcations là où Zola amalgame, pour les besoins de son pamphlet, hypnotisme, spiritisme et occultisme.

Essayons, tout d'abord, avant de suivre la chronologie de cette "affaire dans l'Affaire", d'en évoquer les deux principaux acteurs, que les uns et les autres firent alors parler dans la presse, le Docteur Gibert et la somnambule Léonie.

Joseph Gibert et Léonie Leboulanger

Joseph Gibert (1829-1899), est issu d'une famille protestante réfugiée en Suisse à la suite de la Révocation de l'Edit de Nantes. Après une thèse de médecine soutenue à Paris en 1859, il s'installe au Havre, et se marie en 1861 à Mulhouse avec Fanny Dolfus, issue d'une famille de filateurs protestants. Après avoir choisi de retrouver la nationalité française en 1872, il devient une notabilité havraise : il est par exemple le médecin et l'ami du maire Jules Siegfried, et du futur président de la République Félix Faure, originaire du Havre. Il y fonde en 1875 un dispensaire pour les enfants pauvres. Il participe au Conseil municipal de 1878 à 1881, et obtient la création d'un bureau municipal d'hygiène. Gibert fait figure de grand hygiéniste et de grand philanthrope, n'hésitant pas à soigner gratuitement beaucoup de malades pauvres. Correspondant de l'Académie de médecine, il s'intéresse au magnétisme animal bien avant que celui-ci ne soit réhabilité sous le nom d'hypnotisme; on lui donnera alors le surnom de "Charcot de la Normandie". Lorsque le jeune Pierre Janet s'installe au Havre en 1883 comme professeur de philosophie, c'est Gibert qui lui fait connaître Léonie Leboulanger, une somnambule déjà célèbre localement. Pierre Janet dédicace sa thèse de 1889, *L'Automatisme psychologique*, où celle-ci joue un grand rôle, aux docteurs Gibert et Powilewicz, "en hommage de reconnaissance et d'affection". Après la condamnation d'Alfred Dreyfus fin 1894, Gibert prend contact avec Mathieu Dreyfus, le frère d'Alfred, issu lui aussi, comme Madame Gibert, d'une famille de filateurs de Mulhouse, et se dit persuadé de son innocence . Il demande à ce sujet une entrevue à son ami Félix Faure. Il s'affirme publiquement dreyfusard par conviction républicaine et par foi religieuse [5].

Née en 1838, Léonie Leboulanger, toujours présentée comme une simple paysanne normande coiffée de son bonnet, devient, à la fin des années 1880, un sujet à personnalités multiples presque aussi célèbre, chez les psychologues européens, que la fameuse Félida d'Azam. Pierre Janet et Charles Richet l'avaient tout d'abord fait connaître par des expériences de suggestion mentale et de suggestion à distance menées au Havre aux alentours de 1885. Si Richet la considéra toujours comme un sujet doué de facultés que nous appellerions "paranormales", elle fut essentiellement étudiée par Janet . Elle est présentée dans *L'Automatisme psychologique* comme une hystérique dont les personnalités correspondent à des différences de profondeur dans le somnambulisme; cela posait la question du nombre de ses sous-consciences . Par la suite Janet, dans *Les Médications psychologiques* en 1919, soulignera que Léonie était loin d'être un sujet naïf puisqu'elle avait été en particulier déjà magnétisée par un certain Dr Perrier de Caen vers 1854-1860. Il évoque même une affaire de chasse au trésor dans laquelle elle aurait été utilisée pour ses dons de voyance au château de Crèvecoeur-en-Auge en 1864. Toutefois, Léonie, qui tenait beaucoup à n'être pas confondue avec un médium spirite, n'était pas non plus somnambule professionnelle. Elle est toujours décrite comme une femme honnête, laborieuse et sensée, ayant son franc-parler, employée pour garder les enfants ou tenir la maison de ceux qui étudiaient ou utilisaient ses dons de somnambule. Il semble qu'après l'épisode de Crèvecoeur elle ait été ainsi embauchée par Gibert, qui, comme on l'a vu, la confia à Richet et Janet, et, comme on le verra, la mit à la disposition de Mathieu Dreyfus, le frère d'Alfred en 1895. Mais l'affaire dans l'Affaire ne débute que deux ans plus tard, et c'est alors que le rôle de Léonie sémera le trouble parmi les dreyfusards...

Premiers échos (1897-1898)

En novembre 1897 et février 1898, quatre publications passent relativement inaperçues, qui seront réexhumées et citées abondamment lors de l'affaire Gibert. Le 21 novembre 1897, *Le Cri de Paris* publie un petit écho intitulé "L'occultisme et l'affaire Dreyfus" . C'est, apparemment, la première fois qu'il est fait état, publiquement et par écrit, du rôle de Léonie : "Il fallait s'y attendre !

L'innocence de Dreyfus aurait été annoncée, entrevue par une somnambule fameuse dans le monde des occultistes, Madame Léonie, qui opère généralement pour le compte d'un célèbre docteur suggestionniste du Havre, le dr. Gibert . On ne parle que de cela, depuis déjà plus d'une année, parmi les mages, - et, bien avant que Monsieur Scheurer-Kestner se soit dressé en justicier intègre, ladite Léonie extra-lucide déclarait que l'ex-capitaine était la lamentable victime d'une erreur judiciaire et que sûrement, avant que se terminât la troisième année de la peine, son innocence pleine et entière serait démontrée .

Si, après cela, tout le monde n'est pas convaincu - c'est que vraiment il y a des gens difficiles à convaincre !" A l'époque, ce journal, comme beaucoup d'autres, est loin d'être ouvertement dreyfusard comme il le deviendra très vite par la suite. Si l'écho est franchement ironique en ce qui concerne le monde de l'occultisme, il est ambigu vis-à-vis de Scheurer-Kestner : on ne sait s'il faut prendre au sérieux l'expression "justicier intègre". La conclusion stigmatise la croyance immédiate et irréfléchie des "mages", sans lui opposer véritablement une conviction qui serait fondée sur des preuves scientifiques de l'innocence de Dreyfus.

L'Avenir du Havre, dans un article intitulé "Les intrigues de M. Gibert", le 27 novembre 1897, fait état d'une entrevue entre Gibert et le président, mais affirme que Faure a refusé de parler de l'affaire Dreyfus. La semaine suivante, dans ce même journal, Gibert nie, dans une lettre datée du 30 novembre, que cette entrevue ait eu lieu.

Trois mois plus tard, *Le Cri de Paris* est devenu ouvertement dreyfusard. Le Docteur Gibert, cité par la défense lors du procès Zola, n'a pas témoigné. On se demande pourquoi. Le journal apporte bientôt la réponse en évoquant, le 27 février 1898, dans un article intitulé"Le Dr Gibert et M.Félix Faure", une entrevue qui aurait eu lieu entre le Président et Gibert. Selon *Le Cri de Paris,* Mathieu Dreyfus aurait déchiré la lettre que Gibert, malade, se proposait d'envoyer à l'avocat de Zola, Me Labori. Gibert apparaît cette fois-ci comme le détenteur d'une vérité cachée qu'il souhaite porter au grand jour, même

s'il est évoqué un peu ironiquement par ce journal comme un "docteur Miracle" doublé d'un "généreux altruiste".Ces quatre publications vont servir après coup d'enjeu dans une polémique.

"Ceci est un mensonge"

Le Docteur Gibert meurt le 19 mars 1899, un mois après Félix Faure, au moment où la Cour de Cassation est saisie d'un pourvoi en révision, toutes chambres réunies. Parmi beaucoup d'autres notabilités, Pierre Janet vient de Paris pour assister à ses obsèques. L'affaire se déclenche lorsque Gabriel Monod publie dans *Le Siècle* du 24 mars 1899 une lettre indignée, largement citée dans la presse par la suite, que lui avait adressée Gibert en date du 13 novembre 1897. Il y confirmait que, lors d'une entrevue avec Félix Faure en février 1895, ce dernier avait fait état de documents secrets prouvant la culpabilité de Dreyfus, dont ni l'accusé ni son avocat n'avaient eu connaissance, en violation de "toutes les règles du Droit". Cette lettre devient une preuve de plus pour la marche vers la révision et la vérité, puisque Gabriel Monod déclare avoir déposé l'original entre les mains de Me Mornard, avocat de Lucie Dreyfus, pour qu'elle soit remise à la Cour de Cassation, si besoin est.

Le 26 mars, Le Gall, ancien secrétaire général de la présidence de la République, révèle, dans une dépêche à l'agence Havas, largement reprise par les journaux, que Félix Faure a écrit en marge du *Cri de Paris* de 1898 : "Ceci est un mensonge". Qui a menti, parmi les morts et ceux qui les font parler, pour reprendre un titre de *L'Eclair* du 29 mars : Gibert ou Félix Faure, Le Gall ou Gabriel Monod ? tel pourrait être le débat qui s'engage alors dans la presse.

Les journaux anti-dreyfusards se mettent à l'affût d'articles anciens et de lettres posthumes de Gibert qui pourraient jeter le doute sur la lettre publiée par Monod. Dans *La Libre Parole* du 28 mars, A. de Boisandré, qui est apparemment allé fouiller par ruse les archives de *L'Avenir du Havre*, devenu entre temps dreyfusard, en ressort "Les intrigues du docteur Gibert", ainsi que la réponse du docteur. Celui-ci se serait ainsi démenti lui-même par cette lettre.

"Histoire de somnambule"

Quant au révisionniste *Figaro* en date de ce même jour, il choisit la voie prudente d'excuser le vieux médecin en le déclarant irresponsable, et en ressortant la somnambule du placard. Une "enquête" de son directeur G.Calmette, explique, à la une, que, si Gibert n'avait pas témoigné au procès Zola, c'est que "...depuis près de trois années son esprit s'était modifié : il avait renoncé peu à peu à l'exercice de la médecine; il s'était voué, vers la fin de sa vie, à l'étude des sciences psychiques et s'était placé sous la direction absolue d'une somnambule qui l'avait persuadé, en effet, de l'innocence du condamné de l'île du Diable; Il n'avait plus, dès ce moment, par suite de la surexcitation dont il faisait preuve, à propos de "l'Affaire" aucune relation avec le président de la République". Calmette, probablement inquiet de la dépêche de Le Gall, préfère ainsi abandonner la défense de la mémoire "dreyfusarde" de Gibert. Par la suite, *Le Figaro*, dont l'équipe dirigeante s'entend mal, ainsi que le note J. Ponty, fera machine arrière, et choisira de défendre de nouveau le médecin du Havre en reprenant une interview du *Temps*.

En effet le 29 mars, ce journal publie des "explications" apportées par un ami de Gibert en riposte immédiate à l'article de Calmette : "Il convient d'observer que M.Gibert était correspondant de l'Académie de médecine, qu'il s'est occupé avec M.Pierre Janet d'hypnotisme, mais qu'il n'a jamais ajouté foi aux pratiques des médiums". L'interviewé conclut : "Sur tous ces points, la lumière sera faite - et les amis du dr. Gibert et de la Vérité diront tout, sans réserve et sans dissimulation". A l'encontre de l'amalgame fait par Calmette, il s'agit de bien distinguer le spiritisme et l'hypnotisme scientifique. C'est à cette condition que l'on peut se revendiquer tout à la fois comme ami de Gibert et de la Vérité . Ce n'est pas par hasard qu'est évoquée là, pour la première fois, la caution scientifique de Pierre Janet, celui qui, dans *L'Automatisme psychologique*, explique scientifiquement le spiritisme par le somnambulisme, ainsi

qu'on l'a vu.

"Pour un coup d'assommoir, en voilà un qui compte. Et venant du camp dreyfusard, il compte double", dira *L'Intransigeant* du 30 mars à propos de l'article du *Figaro*. Dès le 29 mars, *La Libre Parole*, renchérit et en rajoute par rapport à Calmette. A.de Boisandré continue son "enquête" au Havre : "Quant au Dr. Gibert lui-même, La somnambule qui acheva de lui tournebouler l'entendement est une dame L..., qui habite Caen. Cette pythonisse ne s'était pas bornée à hypnotiser l'infortuné avec la prétendue innocence de Dreyfus; elle l'avait littéralement affolé avec toutes sortes de prédictions sinistres.

Entre autres choses, elle lui avait annoncé, paraît-il, qu'il mourrait juste un mois après le président Félix Faure, et qu'il ne verrait pas le 20 mars 1899. Sur ce point, la prophétie s'est réalisée ..." Comme Calmette, Boisandré insiste sur l'emprise de Léonie sur Gibert, mais il en fait de plus, non plus seulement une femme qui "persuade", mais aussi une Cassandre inquiétante.

C'est alors que *Le Gaulois* vient relayer *La Libre Parole*, en apportant, toujours le 29 mars, une nouvelle "révélation" posthume. Au moment où était paru l'écho du *Cri de Paris* sur "L'occultisme et l'affaire Dreyfus", Le Roy, un rédacteur du journal, l'avait communiqué à Gibert qui avait aussitôt répondu le 22 novembre 1897, en évoquant, à propos de "cette femme déjà âgée" les travaux de Richet, et de Janet[6] sur "les maladies nerveuses et le magnétisme", mais aussi ceux d'Ochorowicz sur "la suggestion mentale". Gibert concluait à propos de la somnambule : "...Léonie, qui est une femme d'une incorruptible honnêteté, est-elle lucide ? Quel est son degré de lucidité? Ce n'est pas ici le lieu de l'examiner. Ce que je puis dire, c'est qu'il y a bien plus de deux ans et demi que je me suis occupé d'elle et que ce qu'elle a pu dire, penser au sujet du capitaine Dreyfus n'a eu aucune importance ni dans le passé, ni, j'en suis certain, dans le présent". Cette lettre fait la "une" du journal, car elle est présentée comme une preuve supplémentaire, donnée par Gibert lui-même, de l'inexistence, ou du peu d'importance, de l'entrevue avec Faure. *Le Gaulois* réexhume ensuite, en deuxième page, l'écho du *Cri de Paris*, "L'occultisme et l'affaire Dreyfus".

La Libre Parole du lendemain publie en première page un dialogue burlesque de Raphaël Viau, intitulé "La somnambule", qui a pour exergue : "C'est une somnambule qui a affirmé au docteur Gibert l'innocence de Dreyfus". L'affaire est-elle entendue, et le docteur est-il ainsi définitivement disqualifié et ridiculisé? Dans cette guerre des lettres posthumes, *Le Siècle*, toujours le 30 mars, contre-attaque, puisqu'il apporte un supplément de preuve par rapport à la lettre à Monod publiée le 24 mars qui avait déclenché toute l'affaire; il publie une autre missive de Gibert, en date du 15 février 1898, au moment du procès Zola, à un correspondant anonyme, M.A., qui reprend en termes analogues le récit de l'entrevue avec Félix Faure. *L'Aurore* de ce même jour, dans "Histoire de somnambule", répond au *Gaulois* de la veille en présentant la lettre de Gibert concernant Léonie, comme une "preuve des rapports d'ordre purement scientifiques qui existèrent entre lui (le docteur Gibert) et la somnambule".

La défense d'une mémoire

C'est alors que Gabriel Monod, absent de Paris, entre en lice pour défendre la mémoire de Gibert, dans trois lettres adressées au *Temps* le 28 mars, au *Figaro* le 29, et au *Journal du Havre* le 2 avril. La première parle de "l'impeccable sincérité" de Gibert. La seconde, en réponse à Calmette, affirme que Gibert a continué d'exercer la médecine, et conclut : " Quant à ses études psychiques elles remontent à sa jeunesse. Il les a continuées toute sa vie et les beaux travaux de Monsieur Pierre Janet sur l'hypnotisme ont été faits, en grande partie, en collaboration avec le dr. Gibert. Prétendre qu'il a cru à l'innocence de Dreyfus sur la foi de la fameuse Léonie est une pure imagination". Monod ne se contente pas, comme l'ami de Gibert interviewé dans *Le Temps*, de citer une caution à l'appui des recherches du médecin du Havre. Il fait apparaître le dédicataire de *L'Automatisme psychologique* comme le collaborateur de Pierre Janet. S'agit-il, entre les lignes, de rendre aussi justice à un précurseur méconnu? Il est possible que l'historien ait eu connaissance de certains articles sur l'hystérie et l'hypnotisme publiés notamment en 1892 et 1893 dans une revue régionale de médecine qui, *La*

Normandie Médicale [7]. Son ami havrais y faisait état par exemple de traitements de l'hystérie menés vers 1860 par le rappel puis la suppression de souvenirs très proches du cas de Marie dans *L'Automatisme psychologique*, qui sera cité par Freud et Breuer en 1893 dans la "Communication préliminaire", reprise en 1895 dans les *Etudes sur l'hystérie* . Monod s'affirme en tous les cas comme un connaisseur éclairé, qui inclut les "beaux travaux" de Janet et de son collaborateur dans le domaine de la science.

Dans la troisième lettre, en date du 2 avril, adressée à Maurice Dumoulin, directeur du *Journal du Havre*, Monod réfute point par point les interprétations de *La Libre Parole* étayées sur les publications anciennes de *L'avenir du Havre* . Il réaffirme que "M. Gibert est un homme qui n'a jamais menti", et révèle la raison secrète qui aurait ligoté Gibert lors du procès Zola, en invoquant le témoignage de Siegfried, qui "sait commemoi que Monsieur Faure, en voyant le nom de Gibert parmi ceux des témoins de Zola, l'a menacé non seulement de le démentir, mais aussi de faire dire qu'il avait perdu la tête et se laissait berner par une somnambule". Loin d'avoir correspondu à une réalité, comme l'affirmait Calmette, l'emprise de Léonie n'aurait été qu'une menace de Félix Faure. C'est donc la mémoire du président, et non plus celle de Gibert, qui serait ternie par cette histoire de somnambule.

Dernières salves

Mais Léonie est décidément encombrante pour bien des dreyfusards, et certains, comme D.Siciliano dans *L'Avenir du Havre* du 8 avril, déclareront sans ambages : "Il résulte des recherches personnelles que j'ai effectuées que Mme Léonie n'a jamais existé, si ce n'est, du moins, dans la cervelle de notre honorable confrère du *Figaro* .

Au Havre, à Trouville, à Caen et ailleurs, la somnambule Léonie est absolument inconnue" .

Ce même jour, dans *Pstt...!* Caran d'Ache, sous le titre "Magie", caricature Gibert sous les traits de "Mame Gibou, seule élève de Léonie. Trouve dans les lignes de n'importe quelle main les preuves indiscutables de la Grande Innocence". La semaine suivante, le 15 avril, un dessin de Forain en première page représente Marianne endormie par Reinach, l'homme politique dreyfusard qui est la bête noire du journal. Un officier prussien dit à l'hypnotiseur Reinach : "Je n'aurais pas fait mieux, Monsieur Reinach". Ainsi se trouvent retournés à l'adversaire les arguments de "J'accuse" : ceux qui se targuent d'être des amis de la vérité sont montrés comme des magiciens et des endormeurs.

Après ces dernières salves, le feuilleton de l'Affaire se braque sur l'enquête de la Chambre criminelle de la Cour de Cassation, que publie *Le Figaro* le 31 mars, à la suite de fuites calculées. L'affaire Gibert n'en a pas moins rendu célèbre la somnambule Léonie. L'article de Calmette, celui du *Cri de Paris* , et la réponse de Gibert à Le Roy ont été par exemple largement reproduits dans toute la presse [8] . Ni Richet, ni Janet n'ont pu ignorer cette histoire , où ils ne sont pas directement intervenus, bien qu'ils aient été cité. Lorsqu'ils reparleront de Léonie après la guerre, ils ne signaleront jamais son rôle dans l'Affaire. Ils maintiendront toujours après coup un clivage entre le sujet scientifique et/ou merveilleux qu'elle fut dans l'histoire de la psychologie, et le personnage qu'elle fut dans l'histoire tout court.

Une somnambule dreyfusarde?

Peut-il y avoir une somnambule dreyfusarde? Telle est la question posée publiquement en 1899. Les révisionnistes et les dreyfusards y ont alors tous répondu négativement, mais selon trois stratégies différentes. Les uns, comme Calmette, ont amalgamé spiritisme, hypnotisme, occultisme et sciences psychiques; ils ont préféré invalider de fait le témoignage de Gibert et le sérieux de ses prises de position dreyfusardes. Les autres, comme "l'ami de Gibert" dans *Le Temps*, ainsi que Monod, ont établi des lignes de démarcations entre hypnotisme scientifique et occultisme d'une part, entre sciences psychiques et engagement dreyfusard d'autre part. Contrairement à ce qui se passait dans d'autres domaines de connaissance comme l'histoire et la

sociologie, on ne pouvait que séparer fermement les genres. C'est à ce prix qu'ils ont tenté de préserver la mémoire de Gibert comme homme sincère et héraut possible d'une Vérité en marche. D'autres encore, comme Siciliano, ont choisi d'annuler la question en niant purement et simplement l'existence de Léonie.

Mais à travers des rumeurs insistantes, s'est néanmoins posée officieusement la question d'un lien possible. Et si Gibert était aussi devenu dreyfusard à cause de Léonie? Si l'on relit sa lettre à Le Roy, on remarquerera que celle-ci demeure ambiguë, contrairement à ce qu'affirmait *L'Aurore*. Gibert ne se contente pas de se référer au thème des maladies nerveuses cher à Janet, mais il évoque celui, plus sujet à caution d'un point de vue scientifique, de la suggestion mentale . Par ailleurs il ne dit surtout pas que Léonie n'est pas lucide, il s'abstient de trancher, et repousse la question de cette lucidité ailleurs que dans l'arène politique publique.

Après coup deux textes apporteront un autre éclairage. En 1903, Dreyfus est libre, même s'il n'a pas encore été réhabilité, et l'on peut commencer à parler plus calmement de Gibert et de sa somnambule : Joseph Reinach, l'un des premiers historiens de l'Affaire, qui avait lu les mémoires de Mathieu Dreyfus, cite, à propos de Léonie, Pierre Janet et Taine, les pionniers de la psychologie pathologique française. Il insiste sur l'audace scientifique de Gibert, comme si, implicitement, celle-ci l'avait prédisposé à être un dreyfusard de la première heure[9].

Ce sont surtout les mémoires de Mathieu Dreyfus, écrites à partir de 1899, mais puliées seulement en 1978, qui proposent un récit différent. Selon leur auteur, troublé par le déroulement du procès, Gibert commence à croire à l'innocence d'Alfred Dreyfus; il fait alors appel aux dons de Léonie. "Le docteur a fait venir Léonie chez lui. Les premières séances n'ont rien donné. Léonie était sous l'impression des mensonges des journaux qu'elle avait lus. Mais peu à peu, poussée par le docteur, elle s'est mise à chercher"[10], raconte à Mathieu la parente du Havre qui sert d'intermédiaire et qui lui propose, de la part de Gibert, les services de Léonie. N'ayant rien à perdre, Mathieu accepte d'aller en Normandie; il est troublé par les renseignements que Léonie, en état de somnambulisme, lui donne sur lui et sa famille, lors de leur première entrevue. Et, surtout, il raconte que, la première, avant, semble-t-il, l'entrevue de Gibert avec Félix Faure, elle lui parle de pièces secrètes. Même si elle a "puisé" en ses hypnotiseurs pour voir, ainsi que Mathieu le dit à un moment, elle a bel et bien participé à sa manière à une recherche de la vérité autour de 1895, à une époque où l'on ne pouvait que croire sans preuves, et où règnait un "silence de mort" autour de la famille du déporté de l'île du Diable.

A lire Mathieu Dreyfus, on a l'impression rétrospectivement que Gibert a été pris entre deux secrets, celui, inavouable au grand jour, de sa relation avec Léonie, et celui qu'il souhaitait désespérément divulguer, de son entrevue avec Faure. S'il voulait servir la cause de la Vérité, et en même temps ne pas mentir, il lui fallait surtout ne pas dire qu'une somnambule avait pu, d'une manière ou d'une autre, affermir ses convictions en lui renvoyant une vérité cachée, obscure et improuvable.

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1 la Vérité est prête à sortir du puits dans *Le Sifflet* du 30 décembre 1898, et du 6 janvier 1899 . Le 7 avril , soit la veille du jour où Caran d'Ache publie "Magie", Ibels la représente sortant enfin nue du puits, aidée par Figaro.

2 Ponty J., "La presse quotidienne et l'affaire Dreyfus en 1898-1899. Essai de typologie", *Revue d'Histoire moderne et contemporaine*, XXI, avril-juin 1974, 191-220. Cf. p.208. Cet auteur présente une typologie de la presse quotidienne. Elle montre que de septembre 1898 à juin 1899, les journaux peuvent être répartis en trois catégories : 1 - les anti-dreyfusard plus ou moins virulents, nationalistes, monarchistes, catholiques, comme *La Libre Parole* (LP), *Le Gaulois* (G) *L'Eclair* (E), *L'Intransigeant* (I), *Le journal.La Croix* (JC), *L'Univers et le Monde* 2 - Les révisionnistes, qui, sans préjuger de l'innocence de Dreyfus, protestent contre les irrégularités du procès et sont partisans de sa révision. Il s'agit en général de journaux républicains modérés, comme *Le Figaro* (F), *Le Matin* (M) et *Le Temps* (T). 3 - Les dreyfusards, qui proclament, en outre l'innocence de Dreyfus, tels que *L'Aurore* (A), journal républicain de gauche et *Le Siècle* (S), républicain du centre. Elle remarque que, même si certains journaux, comme *Le Temps* (ancêtre du journal *Le Monde*), *Le Siècle* ou *Le Figaro* ont un tirage limité (40 000 exemplaires au plus) ils ont une audience beaucoup plus large, du fait qu'ils servent de référence et sont largement cités par les quotidiens parisiens et surtout provinciaux à grand tirage.

En ce qui concerne la presse quotidienne du Havre, dont nous aurons à parler, on peut opposer *Le Journal du Havre* (JH) et *L'Union républicaine du Havre* (UH), qui sont révisionnistes sinon dreyfusards, et *Le Petit Havre* (PH), anti-dreyfusard. Les uns et les autres reprennent la plupart du temps les informations de la presse parisienne, ainsi que

ses conclusions, en fonction de leurs opinions

Nous nous référerons par ailleurs à des hebdomadaires : *Le Cri de Paris* (CP), journal d'échos, qu'on pourrait vaguement comparer à l'actuel *Canard Enchaîné*, et dont le directeur était Alexandre Nathanson, également directeur de la fameuse *Revue Blanche*, ainsi que *L'Avenir du Havre* (AH). Cet hebdomadaires, comme *Le Cri de Paris*, est peu favorable à la cause de Dreyfus en 1897, et s'affirme dreyfusard en 1899. Enfin, nous citerons deux célèbres feuilles hebdomadaires de caricatures, spécialement centrées sur l'Affaire, *Pstt...!* (P), anti-dreyfusard, et *Le Sifflet* (Sf), dreyfusard.

[3] Cf Charle C., *Naissance des intellectuels*, Paris, Minuit, 1990, et Mucchielli L., "Le rejet de la notion de race chez les sociologues durkheimiens dans le contexte Fin de Siècle (1885-1914)", in Ducrot A. et Panoff M. éd., *L'idée de race dans l'histoire des sciences*, Paris, 1994, à paraître.

[4] Sur Gabriel Monod, Cf. Rioux R., "Saint-Monod-la-critique" et "l'obsédante affaire Dreyfus", *Mil neuf cent*,, 1993, 11, "Comment ils sont devenus dreyfusards", p. 33-38 .

[5] Pour cette présentation biographique, nous nous appuyons sur quelques notices nécrologiques parues en 1899 dans les journaux parisiens et havrais (UH, S, 20 mars - A 21 mars -*Argus de la presse*, PH, 22 mars -*Le signal* ,24 mars - *La cloche* , AH, 25 mars 1899), ainsi que sur une "Chronique" de *Paris médical* (7 décembre 1912) et des documents manuscrits conservés aux Archives de la ville du Havre . Nous remercions monsieur Claude Barrault pour les recherches qu'il y a effectuées .

[6] *Le Gaulois* dit :" ...les travaux de Richer de Javet et les miens propres" Il s'agit très probablement d'une erreur de celui qui a déchiffré la lettre, et il faut sans doute lire: "de Richet, de Janet et les miens propres".

[7] Cf. par exemple, "De la suggestion dans le sommeil chloroformique", *La Normandie médicale*, 1892, et "Des causes morales de l'hystérie", Ibid., 1893.

[8] L'article de Calmette est par exemple cité dans T, LP, UM du 29 mars, et dans I du 30, celui du *Cri de Paris* de 1897 et la Lettre de Gibert à Le Roy sont reproduits dans JH du 29, T, A, UM, I, PH du 30, E du 31. A l'époque les journaux ont l'habitude de reprendre souvent in extenso les articles de leurs confrères.

[9] Reinach J., *Histoire de l'Affaire Dreyfus,II*, Paris, Charpentier et Fasquelle, 1903, p.172-173.

[10] Dreyfus M., *L'affaire telle que je l'ai vécue*, Paris, Grasset, 1978, p.48.

Magie



Mame Gibou, seule élève de Léonie. Trouve dans les lignes de n'importe quelle main les preuves indiscutables de la Grande Innocence.

Binet Observes His Daughters: Case Studies and French Experimental Psychology

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Abstract

This paper contrasts two styles of investigation common in French psychology at the turn of the century--the laboratory and the clinical--and analyzes the tensions that were perceived to exist between them. It argues that instead of viewing these approaches as fused into a distinctively French brand of psychology, it is more accurate to view them as constantly at odds, pulling their practioners toward different objects of investigation and different conclusions about the nature of human mind. Special attention is given to the work of Alfred Binet, who is portrayed as practicing both of these approaches and embodying some of the frictions that lay between them.

Binet Observes His Daughters: Case Studies and French Experimental Psychology

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In 1901, France's most renowned philosopher/psychologist, Henri Bergson, described the peculiarly French style of psychology as follows:

> Psychology has always aimed at being experimental "and" comparative, but for many centuries it had not had at its disposition any precise method of experimentation and measuring, any means of distinguishing what in thought is really elementary and what is really compound, and finally any firm procedure of analysis. This method came to it from two different sides, from the clinic and from the laboratory ...
>
> These two psychologies, the one founded on pathological observation and the other on direct experimentation, are now united: united with them as well is the ancient psychology of introspection. Today there is only one psychology, a true positive science, which has, like the other positive sciences, its own methods of investigation and also its own instruments and laboratories.[1]

On the surface, there was much to substantiate Bergson's remarks.[2] French scientific psychology of the late nineteenth and early twentieth centuries was, indeed, formed from the combination of two separate streams of intellectual development: the pathological/clinical style associated most closely with Théodule Ribot and his disciples, and the laboratory/experimental approach championed by the German physiological psychologist Wilhelm Wundt. But where Bergson found the two psychologies merged into one, I would like to suggest that, at least for most French practioners at the turn of the century, the situation was more complex and problematic. While many French psychologists practiced both strands of psychology, few were able to merge them seamlessly, and most felt an acute tension between the laboratory emphasis on the experimentally replicable and the clinical interest in the individual and particular. For them, illustrated best by the example of Alfred Binet, the laboratory and the clinic proved to be not just different settings, but the progenitors of different kinds of psychological phenomena and different sorts of psychological science.

French Experimental Psychology

Théodule Ribot initiated the process of developing experimental psychology in France when he declined an offer in 1889 to lead the new laboratory for physiological psychology being established at the Sorbonne, and suggested instead someone more experimentally oriented. As is well known, the choice of Henri Beaunis, a physiological psychologist teaching at Nancy, and especially Beaunis' decision to accept Alfred Binet as an assistant, marked the real beginnings of French experimental psychology. In the years immediately following the founding of the EPHE facility at the Sorbonne, psychological laboratories were established at the Salpêtrière for Pierre Janet in 1890, at the University of Rennes by Benjamin Bourdon in 1896, at the Asile Villejuif by Edouard Toulouse in 1900 or 1901, and at the University of Montpellier by Marcel Foucault in 1906.[3] Investigations into the basic intellectual processes, sensation, memory, and a range of other mental phenomena flourished, aided or made possible by the growing collection of instruments--including Hipp chronoscopes, d'Arsonval chronometers, myographs, audiometers, and dynamometers--that began to clutter the small spaces devoted to the new psychological laboratories.[4]

In much of the experimentation, the style was decidedly Wundtian.[5] As Kurt Danziger has characterized this approach, it emphasized laboratory investigation of the normal processes of cognition, primarily at the level of the perception of simple sensory stimulations, by experimentation with trained individual perceptors in order to illuminate universal, intersubjective psychological phenomena.[6] Introspective reports were relied on, as were a variety of instruments designed to produce precise time measurements for reaction-time studies or to record a range of physiological responses. The French version of experimental psychology, especially as defined by Binet, Toulouse, and their associates, maintained Wundt's emphasis on a laboratory setting and the use of precision instruments to help insure objective results. The goal of their investigations, however, was generally not so much to illuminate the universal characteristics of mind, but to explore the ways in which specific mental attributes were manifested in a collection of subjects.[7]

Individual variation was what drew their attention, with the extent of that variation increasingly attracting greater interest than the nature of the underlying process itself. Because of this focus on variation, many of the studies in the French style of experimentation were based on relatively large numbers of participants generally not known to the researchers rather than the one or two colleagues characteristic of Wundtian practice, and focussed on the higher mental processes, where individual differences were believed to be most acute.[8]

The quintessential example of the specifically French style of experimental psychology was the work of Binet and his colleague Victor Henri in "psychologie individuelle," a field that in many respects they invented during the mid 1890s.[9] In a series of papers appearing between 1894 and 1898, Binet and Henri both together and separately pressed for a new psychology that would, echoing Ribot's language of a quarter century before, "substitute for vague notions of man in general, of the archetypal man, precise observations of individuals considered in all the complexity and variety of their aptitudes."[10] The program was concretely manifested in Binet and Henri's studies of children; groups of primary-school students were presented with a series of "mental tests" and their responses recorded, either with verbal or laboratory instruments.[11] In addition to descriptions of the subjects' actions, the investigators obtained precise quantitative measurements wherever possible.[12] After the data had been collected, it was then analyzed, not so much to better understand the nature of the mental processes being tested, as to be able to rank the groups of children according to proficiency at the tests.[13]

Case-Studies and the Clinic

The development of this French experimental style, with its emphasis on laboratory-situated, instrument-based studies of individuals in the aggregate and its orientation toward classification, however, went neither unremarked nor unchallenged within the broader field of French psychology. A number of researchers bridled at what they took to be the arbitrary limitations, artificialities, and superficial nature of the investigations characteristic of the

experimental approach. Hewing much more closely to the clinical-pathological style of careful, often long-term observation of individual subjects, they celebrated the value of the case-study method of investigation and the richness and complexity of psychological phenomena. "We find already," one such researcher observed, "that the laboratory is a place too narrow, too artificial, and that it would be valuable to draw nearer to reality and make more studies from nature, taking man as he lives ... not as a subject."[14]

What is rather surprising, however, is that many of those most active in the construction and promotion of experimental psychology in France were simultaneously its most vociferous critics. It was Alfred Binet, for example, who uttered the disparaging remarks about the nature of the laboratory, and he was not alone among experimentalists in expressing misgivings about the very project for which they were proselytizing. Edouard Toulouse as well felt equally at home conducting aggregate experimental investigations in his laboratory at Villejuif and celebrating the advantages of the intensive study of a single individual.[15] Indeed, one of the peculiar features of French psychology at the turn of the century was the almost schizophrenic attitude of many of its most prominent practioners toward the methods they were employing. Binet, until his death, would vacillate wildly on the value to psychology of objective methods, statistical studies, and mechanically produced information, at one point lauding their scientific character and objectivity, at another condemning their shallowness. Equally well trained in the methods of clinical observation and experimental manipulation, and acutely aware of the different types of investigative programs implied by each, many among the first generation of French "scientific" psychologists found it difficult to abandon either, and so moved almost nervously back and forth between the two.

The divergence in approach was most visible in the study of intelligence. In one sense, clinical investigations of the higher mental processes looked markedly similar to those performed by avid experimentalists. Thus when Toulouse conducted a study of superior intellect in 1896, he employed many of the same instruments and tests used routinely in French psychological

laboratories.[16] The tools of the trade, however, were employed to a decidedly different purpose. Where experimentalists would typically keep the instrument constant and vary the subjects, as in Binet and Henri's examination of 300 children, Toulouse, following the case-study method, kept his subject, Emile Zola, constant and varied the instruments. What resulted was a much more complex portrait of Zola's intellect than would have been possible had Toulouse relied on a single investigative modality. It was an approach that Toulouse himself likened to that of the quintessential clinical practice, the physician's:

> In order to do truly useful work, it is necessary to employ as much as possible the same methods of examination used in the hospital, those of the doctor who is not content with posing a list of questions, but who interrogates very closely, examines, palpates, scrutinizes and verifies most often with his usual instrumentation.[17]

Like most of his fellow psychologists, Binet had initially been exposed to the clinical style of investigation by working with Charcot at the Salpêtrière. Although singed by the acrimonious debate between Charcot and his opponents over the proper interpretation of hypnotic effects, Binet emerged from his experience of close research on individual subjects with a deep appreciation for the importance of the pathological in illuminating the nature of the normal, and for the value of intensive observation and experimentation. It was this clinical emphasis on obtaining an intimate feeling for the organism, as historian Raymond Fancher has pointed out, that Binet manifested in the series of case studies he performed in the 1890s and early 1900s.[18] In investigations of chess players, calculating prodogies, and a number of authors, Binet sought through extended observation and careful questioning of selected individual subjects to understand the intricacies of the mental processes that underlay their talents.[19] Dubious of the value of quantitative results alone, Binet stressed the need for exacting observation and detailed description in order to achieve some deeper understanding of the phenomena under investigation:

> Mere numbers cannot bring out ... the intimate essence of the experiment. This conviction comes naturally when one watches a subject at work. ... What things can happen! What reflections, what remarks, what feelings, or, on the other hand, what blind automatism, what absence of ideas! ... The experimenter judges what may be going on in [the subject's] mind, and certainly feels difficulty in expressing all the oscillations of a thought in a simple, brutal number, which can

> have only a deceptive precision. How, in fact, could it sum up what would need several pages of description![20]

In addition, Binet's very choice of subjects--individuals manifesting exaggerated instances of the qualities he wished to investigate--reflected one of the fundamental tenets of the clinical-pathological method. As Binet himself explained, "it seems to me that people of talent and of genius serve better than average examples for making us understand the laws of character, because they present more extreme traits."[21]

Binet's most sustained use of the case-study approach, however, came not in his analysis of obviously extraordinary individuals, but in his incredibly painstaking and thorough research on two subjects with whom he was intimately familiar, his daughters Alice and Madeleine. Begun in 1890, when the children were respectively three and five years old, it culminated in his 1903 work, L'Etude expérimentale de l'intelligence deemed by many of Binet's contemporaries to be his psychological masterpiece.[22] The clinical study par excellence, L'Etude expérimentale contained the distillation of years of observation of Alice and Madeleine under myriad conditions, each precisely specified, with those observations employed to generate some general conclusions about the types of intellects that each of his daughters possessed.[23] In contrast to the craniometric studies he conducted during the very same period, which were full of instruments and quantities and fairly rapid measurements of a large number of individuals, L'Etude expérimentale concluded with a condemnation of "la méthode de la statistique" as generating nothing but mediocre results, and praised, especially for the study of the superior functions, the slow, intimate observation of a few well chosen subjects.[24] "If I have been able to arrive at any light by the attentive study of two subjects," Binet observed, "it is because I have watched them live and have scrutinized them over several years."[25]

With this close scrutiny and intimate familiarity came something else, a much different relation to the object of investigation, intelligence.[26] Binet the experimenter used intelligence as a classificatory tool, a way of arraying individuals in a single hierarchy according to global mental power. Binet the clinical observer, however, was much more concerned with the nature of

intelligence, with how the mind operated and how different people could manifest intelligence of different types. While Binet still wanted to classify, and indeed categorized one of his daughters as "l'observateur" [Madeleine] and the other as "l'imaginatif" [Alice], there was no attempt to hierarchize these categories. It was not better, according to Binet, to be an "observer" rather than an "imaginative," simply different. Each style of intelligence carried with it its own set of strengths and weaknesses. And this was not limited to the investigations of his children. In each of Binet's case studies, it was the style of mind that captured his interest and especially the wide variety of ways in which even the highly proficient achieved their skill. Examined not through a single lens, but through the multiplicity of instruments and tests and observation procedures that the clinical style of investigation carried with it, intelligence became a complex, multi-valent phenomenon. As an experimentalist, Binet looked through intelligence to see his subjects; as a clinician, Binet looked through his subjects to see intelligence. And what he found there was not one thing, but many.

Endnotes

1. "Assemblée des Professeurs, Chaire de Psychologie expérimentale et Comparée," 10 novembre 1901; F^{17} 13556 20bis, "Chaire de psychologie expérimentale et comparée," AN.

2. This paper is adapted from Chapter Four of my dissertation, Talents, Intelligence, and the Constructions of Human Difference in France and America, 175-1920 (Ph.D. diss: Princeton University, 1994).

3. There is no comprehensive history of French psychology for the late nineteenth or twentieth centuries. Even this information about the founding of the laboratories has been culled from a number of sources, including Jean Beuchet: "La vie et oeuvre de Benjamin Bourdon (1860-1943)," Psychologie française 6(1961): 173-181; and Paul Fraisse, "L'Evolution de la psychologie expérimentale," in Traité de psychologie expérimentale, vol. 1, Histoire et méthode (Paris: Presses Universitaires de France, 1963), esp. pp. 34-38. For a first-hand account of the nature of the French psychology laboratories at the turn of the century, see Nicholas Vaschide, "L'enseignement de la psychologie expérimentale en France," Revue internationale de l'enseignement 45(1903): 203-214. It should be noted that Toulouse conducted both experimental and psychiatric or psychopathological research, for which he is now better known, at his facility at Villejuif. The laboratory became part of the EPHE in 1901.

4. Henri Piéron provides a description of the Sorbonne laboratory as of 1894 in "Le laboratoire de psychologie de la Sorbonne." Edouard Toulouse and Piéron describe the outfittings of an ideal facility for psychological experimentation in Technique de psychologie expérimentale, 2nd edition (Paris: Octave Doin et Fils, 1911), pp. 6-16. See also Françoise Parot, "La psychologie scientifique française et ses instruments au début du XX^{e} siècle," in Studies in the History of Scientific Instruments, eds. Christine Blondel, Françoise Parot, Anthony Turner, and Mari Williams (London: Rogers Turner Books, 1989), pp. 259-264.

5. Parot points out not only this Wundtian emphasis, but the complete lack of interest in animal experimentation, quite prevalent in England and the United States, in a genetic psychology that sought to understand mind as it had evolved throughout the animal kingdom. See Parot, "La psychologie scientifique française," esp. pp. 261-262.

6. The analysis in this section is heavily indebted to Danziger's insightful observations on the nature of modern psychological research. See Danziger, Constructing the Subject, esp. pp. 27-39; Danziger, "The Origins of the Psychological Experiment as a Social Institution," American Psychologist 40(1985): 133-140; and Danziger, "The Positivist Repudiation of Wundt," Journal of the History of the Behavioral Sciences 15(1979): 205-230. The Wundtian style is also characterized in Mitchell G. Ash, "Academic Politics in the History of Science: Experimental Psychology in Germany, 1879-1941," Central European History 13(1980): 255-286; and R. Steven Turner, "Helmholtz, Sensory Physiology, and the Disciplinary Development of German Psychology," in The Problematic Science: Psychology in Nineteenth-Century Thought, eds. William R. Woodward and Mitchell G. Ash (New York: Praeger, 1982), pp. 147-166; and William R. Woodward, "Wundt's Program for the New Psychology: Vicissitudes of Experiment, Theory, and System," in The Problematic Science, pp. 167-197.

7. Binet in 1909 argued explicitly for the novelty of the Parisian approach to experimental psychology when claiming priority for the Parisians over a similar method being developed at Würzburg. See Binet, "Le bilan de la psychologie en 1908," L'Année psychologique 15(1909): v-xii.

8. "Le système français," Binet noted in 1899, "que V. Henri et moi-même nous nous trouvons représenter part d'un principe tout différent; nous admettons que c'est surtout par les processus les plus compliqués que les individus diffèrent, et nous croyons par conséquent que ce sont ces processus compliqués que la psychologie individuelle doit étudier; si on ne peut pas faire cette étude avec autant de précision que pour des processus plus simples, en revanche les différences qu'on peut relever sont autrement importantes." Binet, "SHARP.--**Individual Psychology. A Study in Psychological Method.**," L'Année psychologique 6(1899): 583-593, p. 583.

9. For general information on Binet's background, see Raymond E. Fancher, The Intelligence Men: Makers of the I.Q. Controversy (New York: W.W. Norton, 1985), esp. ch. 2; and Theta H. Wolf, Alfred Binet (Chicago: University of Chicago Press, 1973), esp. chs. 1-2. What little remains of Binet's correspondence is scattered in a number of collections, although most of these are located in the Salle des manuscrits, Bibliothèque Nationale (BN), Paris. See, for example, the papers of Edmond Goncourt, B.N. Mss. n.a.fr. 22452 ff. 266-270; Louis Havet, B.N. Mss. n.a.fr. 24488 ff. 55-63; and Gaston Paris, B.N. Mss. n.a.fr. 24432 ff. 125-129. In addition, see the papers of Jacques Larguier des Bancels, Bibliothèque Cantonale, Lausanne; and Marian Bejat, "Une correspondence inédite d'Alfred Binet," Revue roumaine des sciences sociales: Série de psychologie 10(1966): 199-212. Reminiscences about Binet can be found in M. Michotte, "Discours de M. Michotte"; Edouard Claparède, "Simples Souvenirs"; Jacques Larguier des Bancels, "L'Oeuvre d'Alfred Binet"; and Henri Piéron, "Le laboratoire de psychologie de la Sorbonne"; all in Centennaire de Th. Ribot: Jubilé de la psychologie scientifique française 1839-1889-1939 (Agen: Imprimerie Moderne, 1939); Claparède, "Alfred Binet," Archives de psychologie 11(1911): 37-88; Larguier des Bancels, "L'Oeuvre d'Alfred Binet," L'Année psychologique 18(1912): 15-32; Piéron, "Quelques souvenirs personnels," Psychologie française 3(1958): 89-95; Théodore Simon, "Alfred Binet," L'Année psychologique 18(1912): 1-14; Simon, "Alfred Binet," Bulletin de la Société Alfred Binet 365(1940): 29-36; and Simon, "Souvenirs sur Alfred Binet," Bulletin de la Société Alfred Binet 415(1954): 343-360. Other important secondary works on Binet include François-Louis Bertrand, Alfred Binet et son oeuvre (Paris: Alcan, 1930); Paul Fraisse, "L'Oeuvre d'Alfred Binet en psychologie expérimentale," Psychologie française 3(1958): 105-112; Pierre Pichot, "Alfred Binet et la psychopathologie," Psychologie française 3(1958): 96-104; Maurice Reuchlin, "La psychologie différentielle au XIX^{e} siècle et au début du XX^{e}: Métrique statistique et comparaison ordinale," Revue de synthèse, 3rd series, 89(1968): 383-400; Read D. Tuddenham, "The Nature & Measurement of Intelligence," in Psychology in the Making, ed. Leo Postman (New York: Alfred A. Knopf, 1963), esp. pp. 481-489; Edith J. Varon, "Alfred Binet's Concept of Intelligence," Psychological Review 43(1936): 32-58; Theta H. Wolf, "The Emergence of Binet's Conception and Measurement of Intelligence: A Case History of the Creative Process," Journal of the History of the Behavioral Sciences 5(1969): 113-134, 207-237; Wolf, "Intuition and Experiment: Alfred Binet's First Efforts in Child Psychology," Journal of the History of the Behavioral Sciences 2(1966): 233-239; René Zazzo, "Alfred Binet et la psychologie de l'enfant," Psychologie française 3(1958): 113-121.

10. Binet and Henri, "Le développement de la mémoire visuelle chez les enfants," Revue générale des sciences pures et appliquées 5(1894): 162-169, p. 167.

11. See Binet and Henri, "Le développement de la mémoire visuelle chez les enfants"; and "Recherches sur le développement de la mémoire visuelle des enfants," Revue philosophique 37(1894): 348-350.

12. "Il serait peu utile," Binet argued in 1898, "d'étudier les intelligences et les caractères si on devait se contenter de les décrire, sans leur appliquer une mesure quelconque, si grossière, si approximative qu'elle fût; l'immense utilité de la mesure est du reste si profondément sentie par tous les hommes de science qu'il est inutile d'insister." Binet, "La mesure en psychologie individuelle," Revue philosophique 46(1898): 113-123, p. 113.

13. Jules de la Vaissière in his 1912 book on experimental psychology caught this sense that individual psychology had classification and categorization as one of its primary goals. See Jules de la Vaissière, Eléments de la psychologie expérimentale (Paris: Beauchesne, 1912), esp. pp. 323-325.

14. Binet, Inédits d'Alfred Binet, p. 31.

15. See Edouard Toulouse, Enquête médico-psychologique sur les rapports de la supériorité intellectuelle avec la nérvopathie. Emile Zola (Paris: Société d'Editions Scientifique, 1896); and "Lettres de Edouard Toulouse à Emile Zola, 1895-1901," papers of Emile Zola, B.N. Mss. n.a.fr. 24524 ff. 116-154.

16. See Toulouse, Enquête médico-psychologique.

17. Toulouse, Enquête médico-psychologique, pp. x-xi.

18. See Fancher, The Intelligence Men, pp. 63-65.

19. See Alfred Binet and L. Henneguy, "Observations et expériences sur le calculateur J. Inaudi," Revue philosophique 34(1892): 204-220; "Les grandes mémoires: Résumé d'une enquête sur les joueurs d'échechs," Revue des deux mondes 117(1893): 826-859; Binet and Jean-Marie Charcot, "Un calculateur de type visuel," Revue philosophique 35(1893): 590-594; Binet and L. Henneguy, La psychologie des grandes calculateurs et joueurs d'échechs (Paris: Hachette, 1894); Binet and Jacques Passey, "La psychologie des auteurs dramatiques," Revue philosophique 37(1894): 228-240; Binet and Passey, "Etudes de psychologie sur les auteurs dramatiques," L'Année psychologique 1(1894): 60-118; Binet, "M. François de Curel (Notes psychologiques)," L'Année psychologique 1(1894): 119-173; and Binet, "La création littéraire. Portrait psychologique de M. Paul Hervieu," L'Année psychologique 10(1903): 1-62.

20. Alfred Binet, La suggestibilité (Paris: Schleicher, 1900), pp. 119-120, quoted in Fancher, Intelligence Men, pp. 62-63.

21. Alfred Binet, "La création littéraire," p. 3.

22. Alfred Binet, "The Perception of Lengths and Numbers in Some Small Children"; "Children's Perceptions," Revue philosophique 30(1890): 582-611; "Studies on Movements in Some Young Children," Revue philosophique 29(1890): 297-309; and "Imageless Thought," Revue philosophique 55(1903): 138-152; all in The Experimental Psychology of Alfred Binet, pp. 79-92, 93-126, and 207-221; and L'Etude expérimentale de l'intelligence (Paris: Schleicher Frères & Cie, 1903). On the high regard for L'Etude expérimentale, see Nikolai Kostyleff, La crise de la psychologie expérimentale (Paris: Félix Alcan, 1911), p. 64; Larguier des Bancels, "L'oeuvre d'Alfred Binet," p. 159; La Vaissière, Eléments de la psychologie expérimentale, p. 17; Michotte, "Discours de M. Michotte," p. 54; Henri Piéron, "Préface," in Bertrand, Alfred Binet et son oeuvre, p.iv; and Wolf, Alfred Binet, p. 117.

23. Alfred Binet, L'Etude expérimentale de l'intelligence (Paris: Schleicher Frères & Cie, 1903).

24. This tension between Binet the quantitative experimenter and Binet the qualitative observer would remain unresolved throughout Binet's career. In L'Etude expérimentale, however, Binet came down unequivocally on the side of observation: "I wish that one would be persuaded that psychological experiments, especially those on the complex functions, are not improved [by large studies]; the statistical method gives only mediocre results; some recent examples demonstrate that. The American authors, who love to do things big, often publish experiments that have been conducted on hundreds and thousands of people; they instinctively obey the prejudice that the persuasiveness of a work is proportional to the number of observations. This is only an illusion." Binet, L'Etude expérimentale de l'intelligence, p. 299.

25. Binet, L'Etude expérimentale de l'intelligence, pp. 299-300.

26. Both Theta Wolf and Raymond Fancher have argued that it is misleading to consider L'Etude expérimentale a study of intelligence, because it is much closer to what we would now call an examination of personality differences. While I agree that that is probably true for contemporary usage, it seems to me that it is somewhat anachronistic to apply this denotation retrospectively. Indeed, one of the points of this chapter is that "intelligence" at the turn of the century still could denote differences in kind rather than simply in degree, that it still possessed the possibility of indicating a variety of modalities of thought. For the postions of Wolf and Fancher, see Alfred Binet, p. 116; and The Intelligence Men, p. 65.

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Les questionnaires d'enquête en psychologie de l'enfant, le rôle des revues .

C'est dans la Revue Philosophique, que parait en 1894, la première enquête par questionnaire intéressant la psychologie de l'enfant, son auteur est A. Binet, son titre: Enquête sur le Caractère des Enfants.

Dès lors, et sur une période d'une quinzaine d'années, on recensera une vingtaine d'enquêtes portant sur des thématiques diverses comme les premiers souvenirs, la peur, la colère, le mensonge, les révélations de l'écriture, la concordance entre observateurs, les jeux scolaires ou l'écriture droite ou penchée [1] .

Ces travaux , qui témoignent d'un processus d'institutionnalisation de la psychologie en France, touchent des populations quantitativement très inégales :

200 réponses pour l'enquête sur le mensonge de Buisson, plus de 6000 pour l'enquête relative au vocabulaire de Belot, et 40000 pour l'enquête de Cornuel sur les récompenses.

A l'exception des trois premières _ sur le caractère, sur les premiers souvenirs et sur la peur qui émanent du laboratoire de psychologie physiologique de la Sorbonne que dirige Binet _ ces enquêtes sont toutes issues de la Société Libre pour l'Etude Psychologique de l'Enfant, fondée par Buisson en 1899, et trouvent leur public, au sein du milieu scolaire, par l'intermédiaire des inspecteurs de l'enseignement.

Dans un article du Manuel Général des Instituteurs dont il est directeur, Buisson en précise ainsi le projet ... "créer à Paris une petite société libre d'études pédagogiques, en vue de recherches et d'observations pratiques se rapportant à la psychologie de l'enfance", en référence aux "essais" américains, "pour organiser une série méthodique d'études expérimentales sur la psychologie de l'enfant".

Dans le même esprit, un éditorial non signé du Tome VI de l'Année Psychologique, dont l'auteur probable est Binet, reprend cette formulation, dans ces termes : "La Société a pensé que le premier emploi de

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[1] on pourra consulter à la fin la liste des enquêtes citées .

son activité devait consister à composer et à distribuer des questionnaires sur un certain nombre de problèmes importants pour la psychologie de l'enfant. En entrant dans cette voie, la Société se borne à suivre l'exemple qui lui est si brillamment donné par les Sociétés américaines de Child Study".

Les enquêtes des laboratoires américains, celui notamment de Stanley Hall, portant sur de vastes populations d'enfants, sont connues partout grâce à des revues à fort tirage comme American Journal of Psychologie, Pedagogical Seminary ou Psychological Review . Elles constituent donc la référence majeure au départ du mouvement français d'enquêtes en psychologie de l'enfant .

En France, les revues joueront un rôle déterminant tant dans la diffusion des enquêtes auprès du public auxquelles elles s'adressent, que dans la présentation critique de leurs résultats , ouvrant leurs colonnes à toutes les controverses .

Ainsi, en 1904, le numéro inaugural du Journal de Psychologie Normale et Pathologique publie l'article célèbre de Ribot, "Sur la valeur des questionnaires en psychologie", où celui-ci dénonce dans des termes implacables, le caractère illusoire des enquêtes sur les grands nombres, pour quiconque cherche à saisir des faits authentiques.

Dès lors, en France, sous l'effet de ce coup de semonce, redoublant un sentiment général de déception chez leurs utilisateurs , un déclin s'amorce dans le recours aux questionnaires. Ceux-ci se feront de plus en rares dans les revues, et les critiques à l'égard des productions anglo-saxonnes, de plus en plus acerbes.

L'Année Psychologique qui avait relayé la Revue Philosophique dans la diffusion de questionnaires de 1894 à 1899, cesse en effet d'en éditer, laissant ce soin au Bulletin de la Société Libre, qui ne s'adresse qu'à ses propres membres. Jusqu'en 1904, L'Année Psychologique présentera toutefois les résultats de quelques enquêtes dûment choisies, comme celle de M.Fuster, sur le degré d'accord entre deux observateurs .

Un petit nombre de travaux de ce type, seront encore menés par la Société Libre, jusqu'en 1909, où paraît, dans le Bulletin, l'enquête de Bocquillon sur la paresse qui semble être la dernière du genre, avant la guerre de 1914.

Ribot, on l'a vu, mais simultanément Binet, ont contribué au déclin de cet instrument .

Binet, pourtant, avait fondé de grands espoirs sur l'utilisation du questionnaire comme instrument de connaissance scientifique, ce qui explique pourquoi il intervient toujours, à un moment ou à un autre, dans le déroulement des travaux, soit au titre de "conseiller scientifique" auprès de Buisson, soit au titre de président de la Société Libre quand il prend sa succession à ce poste.

Initiateur de l'enquête dans le domaine de l'enfance, il en reste un certain temps le partisan prudent, participant à l'élaboration des questionnaires , recommandant leur essai préalable sur de petits échantillons , incitant à la critique systématique des résultats.

Qu'attend Binet de la méthode du questionnaire ?

Selon lui, l'enquête par questionnaire, si les conditions d'application en sont bien contrôlées, pourrait fournir à la psychologie scientifique, un instrument privilégié pour l'observation des processus supérieurs du psychisme. Et cela en particulier, pour l'étude de la vie émotionnelle et de l'imagination dont, dit-il, on ne sait encore presque rien, du fait que, par nature, elles échappent à l'expérimentation en laboratoire. Malapert, dans des conseils pratiques aux destinataires du questionnaire sur le sentiment de la colère (Année Psychologique,T.VI,1899), évoque dans le même sens, "l'importance de la vie émotionnelle dans la constitution du caractère", et il ajoute, "c'est donc par des observations prises sur le vif et réunies au moyen d'enquêtes de la nature de la nôtre qu'on pourra arriver à connaître le mécanisme des sentiments, leurs variétés, leurs antécédents, leurs effets".

Ainsi, deux grands domaines d'application de l'enquête par questionnaires semblent pouvoir être dégagés :

L'un concerne l'observation d'enfants.

Par le jeu de questions touchant à leur vie quotidienne, on espère obtenir, en quelque sorte, l'enregistrement sur le vif, d'une multitude de petits faits, non altérés dans leur spontanéité. Ce serait là, l'occasion de recueillir le matériel "expérimental" dont se nourrit la psychologie positive .

Le second permet de provoquer l'introspection dans des conditions contrôlées.

En effet, malgré l'opprobre suscitée par la condamnation positiviste, Binet n'a jamais renoncé à l'introspection. Qu'on se souvienne à ce sujet, de ses déclarations de 1894, dans l'Introduction à la psychologie expérimentale, ..."l'introspection(...) est à la base de la psychologie"... ou, celles de 1908,

dans l'Avant-propos du tome XVI de l'Année Psychologique, ..." l'introspection, c'est à dire l'âme de la psychologie"...
Le questionnaire écrit pourrait, pense-t-il, en renouveler l'utilisation, dans le sens d'une plus grande objectivité puisque, l'introspection n'étant plus le fait du sujet réflexif livré à lui-même, est induite chez autrui, et, de cette manière, peur être observée de l'extérieur. Ce procédé devant autoriser par ailleurs, contrairement à l'introspection classique, une application aisée à de nombreux sujets .
Nous aurons ainsi deux types de questionnaires d'enquête:
1°_ les questionnaires d'enquête fournissant une codification de l'observation (ce qu'on appellerait aujourd'hui " une grille d'observation") et qui s'adressent "à tous ceux qui ont l'occasion d'observer quotidiennement des enfants" comme il est écrit dans le préambule aux premiers questionnaires de la Société Libre, ce qui, de fait, se limitera aux instituteurs, faute de pouvoir enquêter dans le milieu familial, du fait du manque général d'information de la population.
2°_ les questionnaires du type "introspection provoquée", s'adressant, pour les uns, à des adultes, comme c'est le cas de l'enquête sur les premiers souvenirs de l'enfance, menée en 1894 par les Henri, ou, de l'enquête très voisine de G.Dumesnil, en 1903, sur le premier souvenir de l'enfance, et pour les autres, à des élèves, généralement sous la forme d'une rédaction faite en classe comme c'est le cas des enquêtes sur la gronderie en 1901 (Kuhn), sur le souvenir le plus ancien en 1901_1902 (Dumesnil), sur la plus belle action en 1901(Roussel) ou sur les récompenses en 1903 (Cornuel).

De qualité variable selon la nature de l'enquête, les résultats sont dans l'ensemble jugés décevants, voire même tout à fait dérisoires comme c'est le cas par exemple, pour l'enquête publiée en 1904 par Belot, sur les révélations de l'écriture courante chez les écoliers...
"Nous avons manipulé beaucoup de feuilles; nous avons fait beaucoup de totaux, calculé beaucoup de tant pour cent; et vous allez nous reprocher d'arriver à vous les mains vides", constate-t-il avec accablement .

Dans ce cas, c'est à dire l'application d'une "grille d'observation", dont l'enfant fait l'objet , un essai préalable, mieux réfléchi, aurait permis de faire l'économie d'une coûteuse enquête. Cependant , même décevante par rapport aux résultats escomptés, une enquête peut fournir des indications imprévues par exemple sur les moeurs éducatives du milieu familial, la

fréquence des punitions corporelles, l'ignorance des notions morales ou des principes élémentaires de l'hygiène .
Lorsque l'échec n'est pas imputable à une question mal posée, l'irréductible "optimisme pédagogique" des instituteurs colore toutes leurs réponses, aucun crédit d'objectivité ne peut leur être accordé dans les descriptions qu'ils font de leurs élèves, comme le démontre l'enquête sur la concordance entre observateurs, où, de façon systématique, les maîtres éliminent, dans les listes de qualificatifs qu'ils doivent attribuer à leurs élèves, tous les traits qui leurs seraient défavorables, au prix d'une invraisemblance criante, que souligne le rapporteur.

Quant aux questionnaires d'introspection adressés à des adultes, et à travers eux à l'enfant qu'ils ont été, ils montrent qu'on ne peut, ni quantifier, ni analyser aisément les développements aussi protéiformes qu'inattendus qu'ils suscitent parfois, de même, qu'on ne peut comptabiliser l'implicite, tout ce qui, entre les lignes, laisse entendre ou suggère, au détriment de celui qui écrit. Tout cela, pour être interprété, nécessiterait, en complément, une biographie détaillée du sujet. Le grand nombre échappant, de toute façon, à ce type d'interrogation, là encore, l'usage du questionnaire, paraît de peu d'utilité .

Les questionnaires sous forme de rédactions écrites en classe, sous le regard des maîtres, présentent, outre les difficultés de traitement signalés pour les adultes, des déformations inhérentes aux conditions mêmes de leur application. La situation scolaire n'est pas neutre, même dans le cas où le rôle de l'instituteur est supposé se borner à une pure surveillance. Les auteurs d'enquête en font l'amère expérience au fil des années, relevant l'influence des maîtres sur les réponses des élèves, corrigeant ou sélectionnant les copies, en dépit des recommandations de neutralité réitérées en toutes occasions .
L'écueil majeur dans le cas des enfants tient au fait que ceux-ci ne peuvent jamais être tenus pour des interlocuteurs directs puisqu'ils dépendent d'adultes qui les encadrent.
D'ailleurs, on l'a vu, l'interrogation ne s'adresse que rarement à l'enfant. Celui-ci est bien d'avantage objet du questionnement, que sujet à qui on l'adresse. C'est l'adulte, l'éducateur, censé le connaître, qui, promu observateur, édulcore le trait et qui, plus secrètement, sortant de la neutralité du surveillant, répond en quelque sorte à la place de l'interrogé.

Binet, qui guide et écoute avec bienveillance ses collaborateurs de la Société Libre peinant à poursuivre de lourdes enquêtes, ne se prive pas d'en condamner le principe même, sous le couvert des notices bibliographiques, régulièrement publiées dans la rubrique Psychologie des Enfants de l'Année Psychologique . Cette rubrique se rapporte à tout ce qui vient d'être publié dans ce domaine, ouvrages français et étrangers, comptes-rendus de recherches et d'enquêtes, et notamment, celles venues d'outre-Atlantique, que Binet éreinte pour leur caractère vain et illusoire. Ainsi, à propos d'une enquête sur la morale chez l'enfant, relatée dans "Pedagogical Seminary", VII, 1900, écrit-il : "1245 enfants ont été questionnés, mais on ne sait rien de ces enfants, ni le sexe ni l'âge. Il vaudrait beaucoup mieux se limiter à un très petit nombre d'enfants, faire sur chacun d'eux beaucoup d'expériences, et les comparer avec les informations qu'on peut obtenir d'autres sources, sur la moralité de ces mêmes enfants" .

Position ambiguë de Binet, reflétant peut-être sa déception de devoir abandonner ce procédé d'investigation, et témoignant peut-être aussi de ses hésitations personnelles.
Ayant besoin du concours des milieux enseignants, très présents au sein de la Société Libre, et qu'il sait favorables à l'enquête scolaire, Binet les ménage, en leur mesurant soigneusement ses critiques, comme on peut le lire dans les comptes rendus des débats rapportés par le Bulletin . Mais, soucieux de vérité scientifique, il ne peut laisser se répandre, de l'étranger, une méthode aux procédures mal contrôlées et aux résultats incertains.
C'est sans doute pourquoi, à propos du même objet, il peut tenir un discours conciliant, dans le cadre d'une publication à usage interne, et, simultanément, adopter un ton mordant pour polémiquer avec des adversaires scientifiques, dans une publication de renom international, dont il est le directeur .

Le questionnaire d'enquête en psychologie de l'enfant, ne sera guère utilisé par la suite; pour recueillir des faits, on lui préférera toujours l'observation intensive de petits groupes ou l'examen de sujets particuliers. Et, sans doute, l'intérêt majeur de ces enquêtes pionnières, est-il d'avoir suscité et nourri, à partir des ratés et des erreurs rencontrées en chemin, une réflexion à la fois méthodologique et épistémologique sur l'observation en psychologie.

Liste des enquêtes [2] :

- Sur le caractère des enfants, Binet, laboratoire de psychologie physiologique de la Sorbonne, 1893 . R.Ph. , I, 1894.
- Sur les premiers souvenirs de l'enfance, V. et C. Henri, laboratoire de psychologie physiologique de la Sorbonne, A.P. I, 1894, R.Ph. I, 1895.
- Sur la peur chez les enfants, laboratoire de psychologie physiologique de la Sorbonne, Binet, A.P. II, 1895.
- Sur le sentiment de la colère chez l'enfant, Malapert, Société Libre, 1899, A.P. 1899 et 1902, Bull. N°1 et N°10.
- Sur le souvenir le plus ancien, Dumesnil et Henri, Société Libre, Bull. N°8.
- Sur les altérations volontaires ou involontaires de la vérité (ou enquête sur le mensonge), Buisson puis Duprat, Société Libre, 1900 et 1902, Bull. N°1 et 6, Archives de Psychologie, III, 1904.
- Sur les enfants indisciplinés et rebelles, Fuster et Fiévet, Société Libre, 1899, A.P. VI, 1899, Bull.N°1, 7 et 8 .
- Sur les enfants grondés, Kuhn, Société Libre, 1901, Bull.N°8 et N°9.
- Sur la plus belle action, Roussel , Société Libre, 1901, Bull.N°10.
- Sur les récompenses, Cornuel, Société Libre, 1903, Bull.N°8, 12, 14.
- Sur l'évolution du sens de la probité, Société Libre,1903, Aurivel, Bull.N°12.
- Sur le premier souvenir de l'enfance, Dumesnil, Société Libre, 1903, Bull.N°12 et 14.
- Sur les révélations de l'écriture courante chez les écoliers, 1904, Belot, Société Libre, Bull. N° 15 et 18 .
- Etude de la concordance pouvant exister entre deux observateurs, de 1903 à 1905, Fuster, Société Libre, Bull. N°14 , 16 , 21 , 24 et A.P. X, 1903.
- Sur les jeux scolaires, de 1903 à 1909, Tarde, Briquet, Cousinet, Société Libre, Bull. N° 16, 19, 21, 22 et 52.
- Sur l'influence de l'école sur le langage des enfants, 1905, ßelot, Société Libre, Bull. N° 24, 25, 27, 35.
- Sur l'écriture droite ou penchée, 1905, Mutelet, Société Libre, Bull. N° 22, 23, 25, 30 et 31.

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[2] abréviations : A.P. pour Année Psychologique, R.Ph. pour Revue Philosophique, Bull. pour Bulletin de la Société Libre pour l'Etude Psychologique de l'Enfant .

- Sur le vocabulaire connu des enfants, 1906, Belot et Anfray, Société Libre, Bull. 29, 30, 35, Journal de psychologie Normale et Pathologique, 1907.
- Sur la paresse, 1909, Bocquillon, Bull. N° 55 et 56.

Bibliographie [3]:

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Archives de Psychologie .
Avanzini G. Alfred Binet et la Pédagogie scientifique, Vrin, Paris, 1969.
Binet A. Introduction à la Psychologie Expérimentale, Alcan, Paris, 1894.
Bulletin de la Société Libre pour l'Etude Psychologique de l'Enfant .
Carroy J. Les Questionnaires dans la psychologie française, à la fin du XIX°siècle et au début du XX°siècle, communication faite au colloque de la SFHSH , "Les questionnaires et guides d'enquête dans les sciences de l'homme: objets, objectivations, objectivités", en mars 1991 .
Journal de Psychologie Normale et Pathologique .
Manuel Général des Instituteurs, 1899.
Paicheler G. L'Invention de la psychologie moderne, L'Harmattan, Paris, 1992.
Revue Philosophique .

[3] Les revues citées ont été consultées de leur première parution jusqu'à la guerre de 1914 .

THE PROFESSIONAL PSYCHOLOGY THROUGHT THEIR PROFESSIONALS JOURNALS: THE CASE OF SPAIN

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SUMMARY

We considerer the professional framework of Spanish Psychology adequately defined by the Colegio Oficial de Psicólogos (Spanish Official Association of Psychologists) and its activity. This professional Association is protectionist to a great extent, and dedicated to the profession it represents. The aim of the work in hand is to offer a picture of the structure, their professionals journals. Journals to build a image of the spanish psychology from this point of view.

INTRODUCTION AND AIMS

Psychology as a science has evolved during the past century, as an independent knowledge in regard to its philosophical source and apt to promote applications in many fields of social and educational interests.

Spain appears as a country receptive to a great extent toward the new psychological ideas. With a certain tradition of reflection upon man and its mind in the modern times (exemplified by the names of Luis Vives and Juan Huarte, in the Renaissance period), it may be said that the beginnings of modern Spanish psychology are to be found in the last decades of past century (Carpintero, 1982).

After many years of political unrest and civil wars, a great effort to bring the country into the European mainstream was launched by an important group of intellectuals and men of science in the final decades of the past century. Greatly interested in pedagogical issues, and viewing education as the main way for social reforms in the country, they approached the new psychology as an instrument for those purposes. Among its leading figures Francisco Giner and Luis Simarro are to be found. Close to them is to be placed S. Ramón y Cajal, the great neurohistologist that discovered the neuronal cell as a real unity, also deeply interested in psychological issues (Carpintero, 1989).

Liberal-minded psychologists, of the republican ideal, supported the development of the first real-life psychological applications. The Civil War and the distatorship brought with them a swing towards the opposite pole, submerging psychology in a scholasticism wich flooded the universities of the official past-war scene, and attempted to do the same with spanish minds. Gradually, due to a strong legal pressure and rigid state control on education and research, a movement of escape was to begin, especially strong from the 1950´ onwards. This flight was away from the scholastic philosophy of the 1940´, towards a more valid knowledge, or even more creative, and made possible the take-off of Spanish Psychology as a natural and positive science (Pérez y cols., 1989; Tortosa y cols., 1991).

Nonetheless, the majority of activity and iniciative was produced as from the 1970´s, when spanish psychology began to receive specialised attention in Universities, after the explosive increase in the demand for this subject. At the end of

the above decade, there was a "big boom" in psychology in the academic world, soon to be followed by a similar "boom" in the social world, provided by the creation of an independent university degree with a specialised curriculum (Bañuls, 1990). The escalating implantation of the subject, first in the Faculty of Philosophy, and then in the Faculty of Psychology, the increase in research, the advent of specialised professors and the first graduates, and the growing number of students, all contributed to increase the interest of the new professionals. This culminated in the passing of the Law of Creation of the Colegio Oficial de Psicólogos (Official Associaton of Psychologists) in 1979.

We considerer the professional framework of Spanish Psychology adequately defined by the Colegio Oficial de Psicólogos (Spanish Official Association of Psychologists) and its activity. This professional Association is protectionist to a great extent, and dedicated to the profession it represents (Civera, 1991). The aim of the work in hand is to offer a picture of the structure, the functioning and the main events for wich this institution is responsible, and to build a image of the spanish psychology from this point of view.

METHODOLOGY and SOURCES

In order to carry out the analysis, a sociohistoriometric model ha been designed which is backed up by the objective and quatitative methodology, but does not reject the most classical critical Analysis of documental sources. This model leans upon the use of bibliometric techniques, the perception of science as a complex organisation, and the conviction that scientific knowledge is public and communicated. Consequently, the analysis of the channels of ditribution of this communication allow the reproduction of the welfare of a science at any determined point in time, or throgouth a particular period of time (Carpintero and Tortosa, 1990).

The professional ambience has effectively been defined in terms of material published in the Official Programms of General Congresses and in professional journals dependent upon the Colegio Oficial de Psicólogos (Official Association of Psychologists), the professional institution which legally represents this field, created by the Law of the 31st december 1979. The journal selected for the analysis is actually "Papeles del Colegio, Psicólogos, the basic body of expression of the scientific and professional policy of the Society, which has been published non-stop since 1981, and prints some 27.000 copies. Also selected are the programms of the General Congresses of Madrid (1984) and Valencia (1990). The content of all the works has been studied independently of the thematic spectrum, as well as Guest Conferences, general reports, and sessions of free communication and posters.

DISCUSSION AND CONCLUSSIONS

The Spanish Civil War (1936-39) impaired the normal development of psychology in our country. A massive emigration of professionals followed the end of the war, and deep changes took place in Spanish culture, now under the ruthless control of a very conservative-minded government (Carpintero, 1989).

The restoration of a scientific psychology was slow. An active group gathered around the person of José Germain (1897-1986). He was charged with the organization of a Department of Experimental psychology in the Higher Council for the Scientific Research. In the new Department, the research in psychotechnics was soon reassumed and the interest for scientific psychology grew continuously among his collaborators. He also founded a journal, Revista de Psicología General y Aplicada

(1946-) , and a few years later, he promoted the creation of a Spanish Psychological Society (Martí, 1990).

Germain also faced the problem of rising professionals that could develop and manage psychological applications to social problems and needs. The teaching of psychology was first planned as a program at a postgraduate level, and the task was carried out at two Psychology Schools, that were established by the universities of Madrid (1953) and Barcelona (1964) (Encinas y Rosa, 1990).

Substantial progress was achieved when the Schools were replaced by university departments in forming psychologists. The change took place in 1968, and this marked the turning point of the development of Spanish psychology. Thousands of students came to universities seeking a degree in psychology; psychology departments were created in many universities, research and teaching jobs were offered, new scientific journals were founded, and the development of the field grew. Between 1971, the date of the first graduation, and 1988, around 30.000 psychologists graduated (with the high proportion of 528 psychologists per million inhabitants) (Alcaín, 1990), of which more than 21.000 became collegiates in front the decreasing evolution of the membership in Spanish Psychological Society. The professional market has been analyzed this same year, offering a interesting picture of the psychological work (Diaz & Quintanilla, 1992)

New scientific societies were founded in recent years, giving support to further specialization. Let us mention here the Spanish Society for Psychological Assessment, the Spanish Association for Behavior Therapy, the Iberoamerican Association for Transactional Analysis, the Spanish Society for Rorschach and Projective Techniques, and last but not least, the Spanish Society for the History of Psychology.

Interestingly is the foundation of an organization directly concerned with professsional and labour problems of psychologists. This is the Professional Association of Psychologists, first established in 1979 as an independent entity, now gathering about 30.000 members, distributed in various regional divisions. Another Professional Union for Psychologists, restricted to the Catalonia area, was recently created, well backed by the political decentralizative trends dominating the Spanish arena after the arrival of the democratic regime,that gave place to the organization of several so-called autonomous regional governments in the country. As their main target, such Unions are interested in the consolidation of the role of psychologist in our society, and in giving support to practical activity , but they are also backing some scientific and theoretical activity, as we said elsewhere.

A large part of the merit for the recovery of the scientific tradition in psychology must be given to some Germain's pupils and collaborators, that occupied in the 50s and 60s university chairs from which they first promoted scientific psychology among their students. One of these Germain's students was Mariano Yela (born in Madrid, 1921), his colleague José Luis Pinillos (born in Bilbao, 1919), Miguel Siguán (born in Barcelona, 1918). A large number of present-day researchers and professors are , in one way or another, related to those names. This means that a deep unity spreads all over the scene of the Spanish psychology, although a great theoretical freedom dominates everywhere. Nevertheless the group of more productive authors in the activities organized by the Professional Association of Psychologists there are a certain number of well-known researchers behind the universities, and also researchers without any vinculation to the universty.

Some interesting clues related to the recent evolution of psychological theorizing in the country may be obtained from the thematic analysis of those contributions presented to the various congresses that have taken place under the sponsorship of the Spanish Professional Association of Psychologists. Such meetings, covering all theoretical specialties and all geographical areas, give a picture of the main interests and orientations that characterize the various groups working in the country. A perspective that is accomplished by the subject-matters of the journal Papeles del Colegio, Psicólogos.

Let us see a concise analysis of the subject-matters of the papers presented to both meetings (Fdez.Ballesteros, 1990; Civera, 1991).

The first one took place in Madrid in 1984, only four years after the foundation of the Spanish Association, and professional and applied topics dominated its debates. Clinical -health- and educational problems explained nearly two thirds of all the presented papers. The second, that has met in Valencia very recently (1990), has mantained the commonly accepted applied sections (educational, clinical and social-industrial ones), but has also added section for more basic and theoretical research - basic psychology and assessment, in what amounts to nearly one third of the whole- as well as for very specific sub-fields such as traffic safety, sport-, forensic and military psychology. Cognitive psychology dominated most of the theoretical symposia, but behaviorally- oriented points of view are still in force in therapy and in various forms of social interventions.

Data coming from congresses may be confronted with another source of information that may serve to validate our findings, the journal Papeles del Colegio, Psicólogos. This is the most important survey of the documents published by the Association. In the data basis formed with the articles coming mainly from the journal are now dominating clinical problems (personality and intervention), professional problems, developmental and educational subjects, work psychology, communication and social topics. Meanwhile, we are lacking substantial research on experimental areas (human, social and animal subfields), psychophysiology, methodology and statistics, and general and theoretical questions.

It is easy to see that in recent years a twofold tendency seems to dominate the field of Spanish psychology: first, there is a large interest invested in clinical and/or health psychology, treatment included, and second, there is a certain lack of support and development for experimental work , a subfield that still is in need for academic and governmental backing.

Spanish psychology shows also in its recent evolution some qualitative changes clearly related to the variations affecting the theoretical orientations dominating the field. These changes have paralleled (not without a certain time-lag) similar variations that have taken place in the still very influential model of American psychology. Perhaps the things are now starting to change, as the scientific relations with European groups - through the newly organized European meetings - may pave the way for the development of a new frame of reference , just now at its beginnings.

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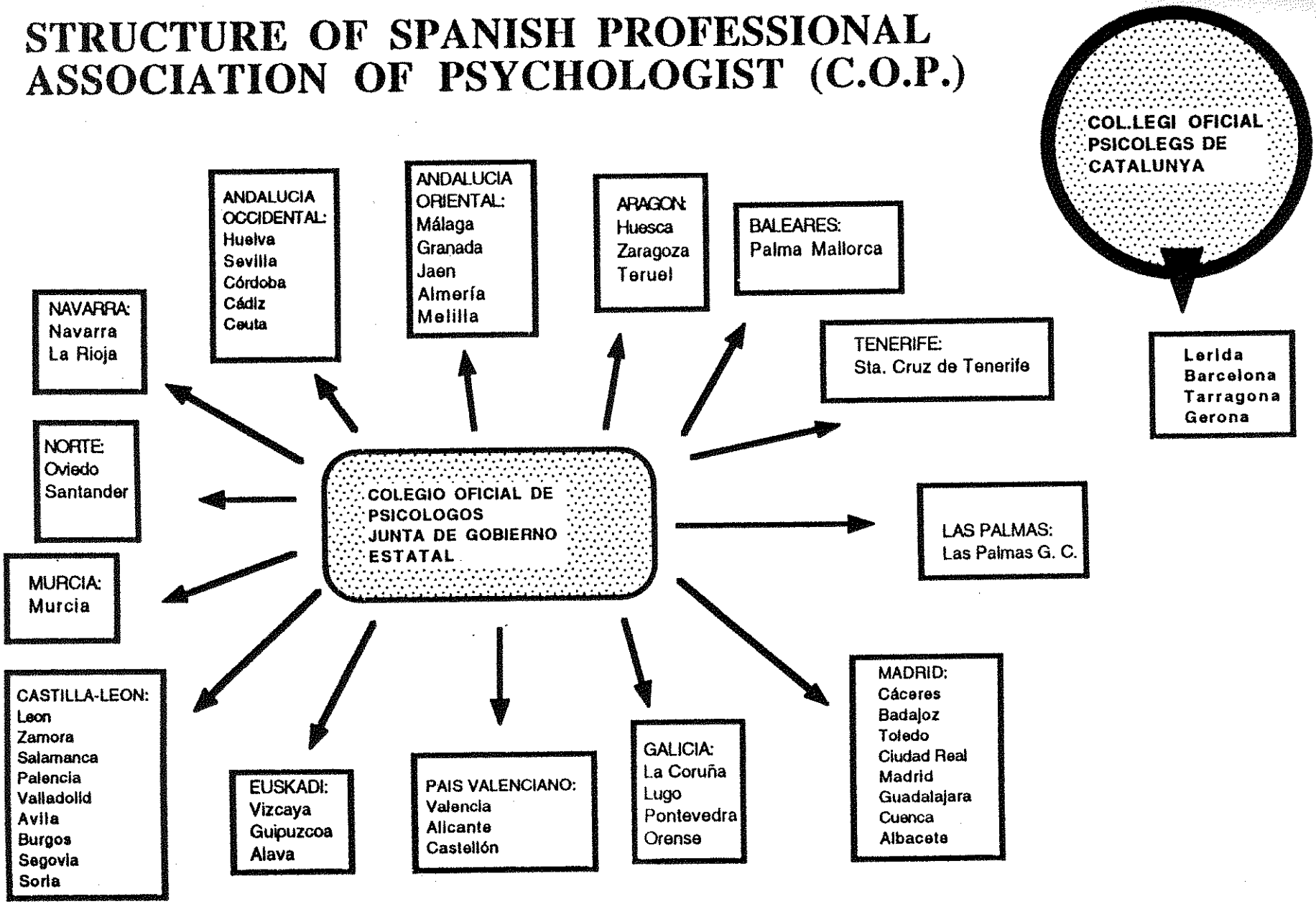
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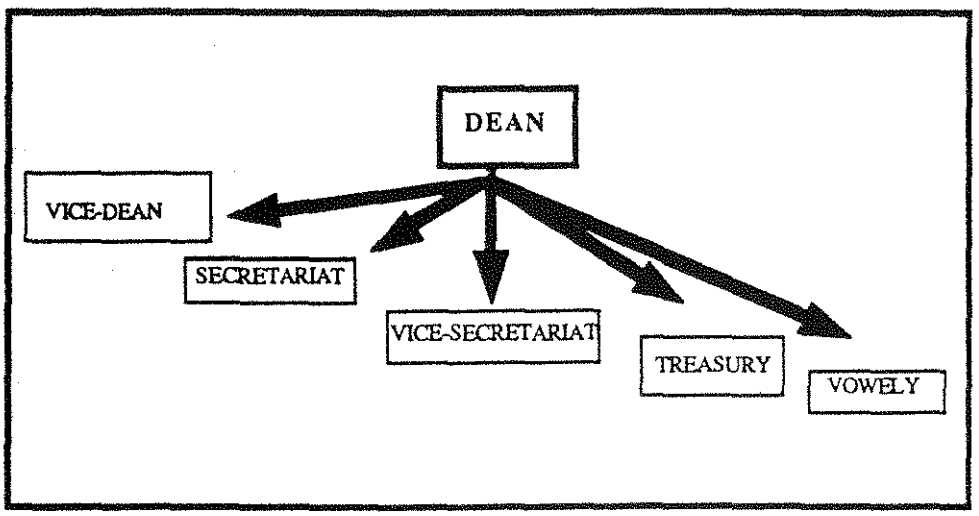


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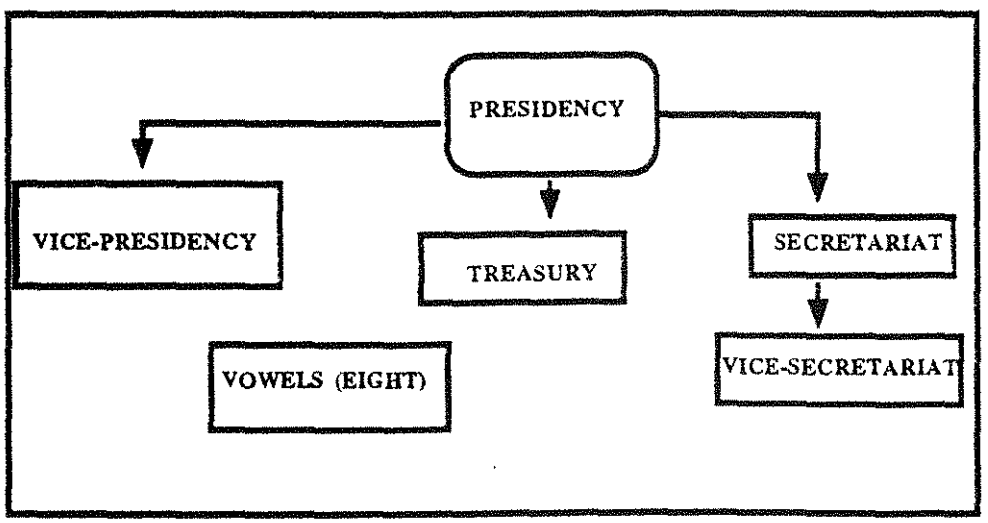
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REVISTA DE PSICOLOGIA DEL TRABAJO Y DE LAS ORGANIZACIONES	C.O.P./ DELEGACION DE MADRID
SELECCIONES DE PRENSA	C.O.P./ DELEGACION DE MADRID
SINTESIS	C.O.P./ DELEGACION DE TENERIFE

FLOW CHART OF PROFESSIONAL ASSOCIATION OF PSYCHOLOGIST

STATAL MEMBERSHIP



DELEGATION MEMBERSHIP



Abstract

The contextual study of memory formed a central theme of Binet's experimental psychology. This paper describes some of the research which represented this focus, attempts to explain some of Binet's concerns and orientations which underlay his interest in this topic, and discusses the implications of his work for guiding contemporary approaches to understanding memory reconstruction. The latter issue considers the relevance of recent neurophysiological (e.g., Freeman, 1990, 1991) models of memory for providing the biological evidence that memory is a function of changes in mental state rather than the reenactment of fixed and changeless traces, a conclusion upheld by Binet over a century ago.

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Binet's Contextual Study of Memory

The contextual study of memory formed a central theme of Binet's research in psychology. This paper describes some of the activities which represented this focus, attempts to explain Binet's concerns and orientations which underlay his interest in this area, and discusses the implications of this work for guiding contemporary approaches to understanding memory reconstruction.

The Contextual Study of Memory as a Research Theme

Binet's interest in the role context plays in memory supported his more dominant need to understand complex mental functioning. This interest was represented in naturalistic observations of his two daughters' approaches to solving memory tasks, analogue studies of the eyewitness testimony of school children and group conformity, analyses of the cognitive strategies used by extraordinary chess players and mental calculators, laboratory investigations of memory for prose passages, as well as other systematic investigations (Wolf, 1976). A reevaluation of this work by several reviewers has found it surprisingly modern and of greater importance than was generally recognized in the 100 years following its origins (Siegler, 1992).

For example, Binet's work on prose memory has been lauded by Thieman and Brewer (1978) as "one of the most

impressive and original experiments in the development of experimental psychology" (p. 262). The work led Binet to the prescient conclusion that the memory of a sentence is quite distinct from the memory of the words because it does not result from the simple addition of the meanings of the words (Binet & Henri, 1894). As another example, Binet's conclusions on the eyewitness testimony of children has been recognized as foreshadowing present-day interpretations of children's testimony (Cunningham, 1988). As Binet (1900) was able to show, children's suggestibility is now also seen as a phenomenon that varies across situations and children rather than viewed as a reflection of their static traits (Goodman, 1984).

In the eyewitness testimony work and that of memory for prose passages, as well as in multiple other investigations of memory, a theme of the instability of meaning pervaded Binet's research. The next sections of this paper will discuss how Binet came to adopt this constructionist stance.

The Background to Binet's Constructionism

Eulogies written by Théodore Simon (1912) and J. Larguier des Bancels (1912) in the 18th edition of L'Année Psychologique provide intimate accounts of the concerns and orientation which guided their colleague's career-long quest for understanding complex psychological phenomena. The contributions emphasized by the two colleagues included Binet's ability to support and extend associationism through empirical observations, his

ability to depart from German experimentation in the Wundtian laboratory tradition, and his ability to retain a physiological conceptualization of mental activity.

Binet's associationism was strongly built upon that of Taine. However, in his first book (Binet, 1886), he took issue with Taine's reductionism by proposing a phenomenological explanation of thought. Binet agreed with Taine that thought resulted from the activity of mental images but he also believed that such images functioned holistically, as the activity of the beehive recapitulates that of the bees (cited in des Bancels, 1912).

An organismic interpretation of associationism led Binet to his well-known stand on imageless thought and his clear departure from German structuralism. Further, belief in the plasticity of thought underlay his interest in memory reconstruction for which he found ample evidence in numerous investigations, including those on prose memory and on eyewitness testimony. The importance of this work was well understood by Simon (1912), who prefaced his eulogy with a signature page featuring a photograph of Binet below which was Binet's handwritten inscription:

> Thought evolves from a vague state toward
> a fixed state. (p. 1)

Despite such recognition, Binet's ideas on the reconstruction of memory remained virtually unnoticed until

recently (Cunningham, 1988).

Implications of Binet's Work on Memory

Reasons given for the obscurity of Binet's pioneering work on memory have included the dominance of behaviorism during the first half of this century which eclipsed competing paradigms (Siegler, 1992). A less obvious reason is that Binet's contextualism was misunderstood by those who might have promoted it (Cunningham, 1992). More specifically, William James bitterly criticized Binet's approaches to observing the cognitive functioning of schoolchildren when he failed to realize that Binet's pluralism and pragmatism were not unlike his own (Cunningham, 1992). Binet's inability to formulate an integrated theory which would explain the mechanisms underlaying cognitive functioning has been given as another reason (Siegler, 1992). However, Binet may have been all the more insightful for tolerating the limitations upon the ability to discover such mechanisms. This was proposed by des Bancels (1912), who prefaced his eulogy to Binet with Binet's own inscription:

> One only needs to experiment and observe, this is true; but how difficult it is to find the true formula of an experiment (p. 15)

If any of these reasons validly identifies Binet's shortcomings, they also point to his strength in his willingness to abandon certainty when it was unwarranted.

Binet took original departures to studying psychological phenomena and arrived at important understandings of memory reconstruction. This work is all the more timely as advanced technology is now able to reveal the biological evidence that memory really is a function of changes in mental state rather than the reenactment of fixed and changeless traces (Barton, 1994; Freeman, 1990, 1991). In their applicability to contemporary paradigms, Binet's ideas transcend the limitations imposed by the timeframe in which they originated. The concluding sections of this paper will discuss implications of renewed interest in those ideas for the future development of memory research.

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1868 - 1918

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The concept of unconsciousness in Edward Abramowski's psychological works

E. Abramowski was one of the first Poles to initiate research aimed at a psychological description of human life. He lived between 1868 and 1918. Initially he studied biology and physics at the Jagiellonian University in Cracow. In 1886-89 he attended lectures at the Department of Natural Sciences in Geneva. His academic interests were focused upon problems from the area of psychology, philosophy and sociology. In 1895 he writes his first psychological work entitled "Theories of Psychic Entities", which contains the term "unconsciousness" used in the aspect of epistemology. In 1908-1912 E. Abramowski conducts research into memory in psychological laboratories of Brussels and Paris. This work results in a three-volume publication of "Experimental Studies of Memory". Abramowski's other psychological works include : "Sources of Subconsciousness", "Double-faced Character of Perceptions", and "Experimental Metaphysics". On his return to the country he runs the Laboratory of Psychology of the Polish Psychological Society. In 1915 he becomes head of the Faculty of Psychology at Warsaw

University. In his psychological works Abramowski creates a concept of psyche based upon a division of psychic life into two spheres: consciousness and subconsciousness. Thus he rejects the then-accepted concept of psyche as manifestation of consciousness. Any representation of inner psychic mechanisms cannot take the form only of intellectualized thoughts or judgements. Should it be so, Abramowski's psychology would belong to abstract sciences, such as mathematics or logic, where result is achieved through abstracting from real data. Yet perceptible spontaneity, variability of a subject's reactions do not allow psychology as a science to lock psyche in a framework of objective scientific truth. In a sense Abramowski's procedure resembles Edmund Husserl's phenomenological method. According to the author of "Metaphysics of Experience" a study of human psyche should be done without prejudice, i.e. without assumptions. All statements, theories, concepts should be put aside so as to enable us to grasp the psyche in a pure descriptive experience. This requires an observation free from defined concepts since psychic phenomena transgress the notional or intellectual form, their scope of meaning covering that which cannot be identified with the concept.

An assumption that human psyche has a layered structure was a crucial moment in E. Abramowski's theory. He claims that psyche consists of the two spheres: consciousness and subconsciousness. Consciousness is a realm of intellect, an

area where judgements on reality and on inner psychic states are formulated. It enables a subject's cognition of reality. Perception constitutes a foundation for gaining knowledge of reality. Memory is an element which synthesises the whole of our psyche. One can distinguish two kinds of memory. One is the active memory, related with the focused attention and intellectual in character. The other one, latent, includes all intuitive experiences resulting from perceptions, i.e. emotional equivalents of their intellectual meanings. Abramowski labels it subconsciousness. It is a groundwork for our psychic life since it covers all individual experiences generated by perceptions on their leaving of the area of consciousness. These perceptions create a "forgotten psychic life" which is a measure of a subject's individuality. Each fact and event occurring in an intellectual form in consciousness and passing on to the sphere of latent memory retain their psychic existence, although not in the form of an image or representation. This pre-conceptual multiplicity of emotional traces constitutes an unconscious background for all states of consciousness. It is a generalized emotional state, an objectless experience. According to Abramowski accumulation of these unintellectual perceptions creates an inner environment which, like the external reality, provides a subject with material for perception. This notionless-emotional state of our psyche is actualized in every psychic act and thus builds a continuum of thoughts and feelings. Notionlessness

means only certain potentiality which has not been yet categorized intellectually and which does not have its position in a series of thoughts but which evokes certain state of experience. This does not mean a defined kind of emotions but a general state of stimulation of organism with its distinguishable physiological components. Affection is its psychological equivalent. Devoid of any representational element, it is a state unknown to our perceiving consciousness. Abramowski emphasizes that "Affection is not at all a casual quality but a permanent feature of our spiritual life, an indispensable factor present in every moment of consciousness" [Abramowski p.391]. Every perception of consciousness after leaving the sphere of attention acquires a pre-conceptual nature and as an emotional trace remains in the layer of the latent memory - the unconsciousness - and thus creates a psychological continuity of an individual. It constitutes a foundation for psychic reality of life, no fresh perception misses its aim, not making up a new quality of psychic life. It always finds support in the unconscious sphere of our psyche which is an emotional record of an individual's whole life. In this way it is a measure of an individual "I", of the identity of one's own person. Temperament and character as possibilities of description of human behaviour are also founded upon the unconscious layer. We can observe their impact on behaviour and thinking, although they are only their manifestations in consciousness, their true manifestations not being subject

to laws of introspection. These are hidden in the unconscious layer, inaccessible to intellect. Man's individuality is also marked with hereditary features which according to Abramowski constitute a "generic memory". He says that every man's character retains not only contents of his own experience in a notionless, or pre-conceptual, form but also an extra-intellectual content of experience of previous generations. This memory cannot be made actual in the form of an image or representation, it cannot be developed in memories. It remains only as an intuitive source of religious and aesthetic experience where intellectual experience proves ineffective. Dreams, remote memories, states of mind in which emotionality overruns rational action are the form of representation of our intuitive experiences coming from the latent memory or subconsciousness. In agnostic states we are able to experience subjective values, and so an artist will experience beauty, in a religious experience a believer - a sense of presence, a mystic - a mystery.

An analysis of the potentialities of intuitive cognition which addresses no familiar concepts, and what is more, inexpressible linguistically brought E. Abramowski to an expansion of a range of psychological studies. Transgression of the framework of intellectual consciousness enables cognition of the deepest layers of personality which by no means can cross the threshold of thoughts. In this way they are inaccessible in introspection to a subject. Yet

they are present in the act of cognition of the "thing in itself", in a direct contact with reality where suspension of the conceptual sphere of our psyche enables elimination of a division into subject and object of cognition. The opposition "I" - "non-I" disappears, instead there appears a unity unknown to intellect. Such states are most distinctly manifested in mystic experiences, they are defined as "Union" or "Nirvana". Thus intuitive cognition is a foundation for agnostic states. Abramowski distinguishes several reasons which determine this extra-rational experience. First of all they are states of affection, of suspended attention provoked by surprise or freshness of events. They may also occur in dreams, hypnosis and religious, ethical and aesthetical experiences. They are also present in erotic experiences. At the basis of agnosis lies a state of stimulation arising only from the layer of subconsciousness. Wherever we have to do with intuitive cognition there appear subjective experiences typical of such a type of epistemological structure. They are manifested by an impossibility of representation in the form of a concept or an image, by a sense of truth free from any justification, as well as by a feeling of a unique value of such cognition. It is noteworthy that Abramowski discriminated features of intuitive cognition on the basis of laboratory experiments. These experiments were aimed at evoking agnostic states through restricting intellectual functions in tested persons. Such a procedure was supposed

to justify a basic theoretical assumption which saw the range of human experience as larger than intellectual possibilities. Such an opposition to the positivist understanding of experience resembles E. Husserl's programme of phenomenology with its form of eidetic cognition based on intuition. Yet individual experience in the phenomenological sense is a total experience which covers not only elements of perceptions but also intuitive acts of cognition. In Abramowski's approach experience is a layered accumulation of knowledge of the world, the source of which lies in the notionless-emotional subconsciousness. According to the founder of phenomenology experience is abstracted from the psychological "I", it is a manifestation of transcendence from a subject of cognition. Bordering on a philosophical description as it is, Abramowski's theory remains a psychological analysis. It is an attempt at answering the question about the function of man's psyche in relation to the whole of his life.

STANFORD, COLUMBIA AND THE IDEAL EXPERIMENT

A complement to Hacking (1988)

Trudy Dehue

Summary

The origination of randomized controlled design is generally ascribed to the famous methodologist and statistician Ronald A. Fisher, who in the twenties and thirties developed it for agricultural research. The historian and philosopher of science Ian Hacking, however, presented an alternative reconstruction. Hacking (*Isis*, 1988) argues that the ideal experiment originates in *psychical research* of the last decades of the 19th century and the first decades of the 20th. However, to my conviction Hacking is at most only partially right. I have made yet another reconstruction, in which *learning psychology* plays the main role.

This paper is submitted to the program committee of the 13th annual conference of Cheiron Europe, to be held in Paris, September 1994. Trudy Dehue is associate Professor at the Section Foundations and History of Psychology, University of Groningen, Grote Kruisstraat 2/1, 9712 TS Groningen. Phone: +31 50 636354. Fax: +31 50 636304. Email: <G.C.G.Dehue@PPSW.RUG.NL>

STANFORD, COLUMBIA AND THE IDEAL EXPERIMENT

A complement to Hacking (1988)[1]

Trudy Dehue[2]

To contemporary students of the life sciences and the social sciences, controlled randomized design is the standard against which all other research designs are measured. Experiments without control condition and without random allocation, are at most granted the label of 'quasi experiments.'[3]

The development of this randomized controlled design is generally ascribed to the famous methodologist and statistician Ronald A. Fisher, who in the twenties and thirties developed it for agricultural research. Mainly via his methodological handbook *The Design of Experiments* of 1935, the randomized controlled experiment gained the status of *ideal* experiment in many scientific disciplines.[4]

The historian and philosopher of science Ian Hacking, however, presented an alternative reconstruction of the origins of the design. In a beautifully written article, with plenty of entertaining additional information, Hacking argues in the historical journal *Isis* that the ideal experiment originates in *psychical research* (parapsychology) of the last decades of the 19th century and the first decades of the 20th. One *wishes* that Hacking was right. If only because the idea is so amusing that it was parapsychology -always looked at by academic psychologists with much methodological suspicion- that bequeathed psychology with its own received rules.[5]

However, to my conviction Hacking is at most only partially right. I have made yet another reconstruction, in which *learning psychology* plays the main role. Psychical research only has a supporting part.[6] In the 20 minutes of a conference paper, I can only tell a portion of the whole story, and even that in a summarized way. For those who afterward want to know 'all of it,' I have an extended written version, with more nice anecdotes too.[7]

Hacking's Coover: controlling and randomizing in parapsychology

Let me begin by recounting the main historical events Hacking builds his argument on. Hacking starts in 1885 with a research project on the discrimination of minimal differences in sensory stimuli, by Charles S. Peirce and his student Joseph Jastrow. In a sequence of experiments it was tested whether sensory differences below a minimal threshold of perception, nevertheless are registrated in a *subliminal* way. In this case, the stimuli were weights that were made heavier or lighter. To make sure that the subject, by turns Peirce or Jastrow, would not be able to involuntarily deduce whether he was offered more or less pressure on his finger through the systematic order of the trials, Peirce carefully randomized this order. He did this, using a pack of playing cards, in such a smart way that Hacking is enthusiastic about 'the canny Peirce.' Here for the first time in history, the idea of control was combined with randomization in a systematic way.[8]

Peirce and Jastrow did not themselves present their experiments as psychical research. However, Hacking argues that they can rightfully be considered as such. He does that based on a contemplation that was added to the report by the authors. At the end of their article Peirce and Jastrow wrote that their positive outcome 'has highly important practical bearings, since it gives new reason for believing that we gather what is passing in one another's minds in large measure from sensations so faint that we are not fairly aware of having them, and can give no account of how we reach our conclusions about such matters.' And they added: 'The insight of females as well as certain "telepathic" phenomena may be explained in this way.'[9]

To Hacking, Peirce was the first to develop randomized experimental design. However, Peirce and Jastrow's article largely gained negative comments and no epigones. Randomized experiments weren't published until decades later. This was to take place at Stanford University, Palo Alto, California.

The background to this event was the remarkable and sad occasion leading to the foundation of Stanford University. The full name of this university is 'Leland Stanford Junior University.' Leland jr was the son of Mr and Mrs Stanford, who had died at the age of 15.[10] In 1885, the Stanfords had founded the university, as they expressed it, 'to make all children of California our children.' Besides Leland's decease had motivated Mrs Stanfords to take an interest in spiritism. She hoped that

this would get her into contact with her son again. Also Mr Stanford's brother was an active member of the spiritist movement. Some years after the foundation of the university, this Thomas Welton Stanford had donated a sum of $10.000 to the university as an endowment for psychical research. The university board, slightly embarrassed decided to place the money at the disposal of the psychology department, which, no less embarrassed, after some consideration decided to use it for an appointment of its assistant Coover who recently had finished his PhD.

Unabashed by the esoteric connotations of his subject, John Edgar Coover undertook his new task with much methodological precision. To him, this implied that control-experiments as well as randomization were indispensable. His experiments are reported of in a 641-page book *Experiments in Psychical Research at Leland Stanford Junior University*, published in 1917 by J.E. Coover.[11] Mainly through this book Hacking substantiates his proposition that it was in parapsychological research that randomized controlled design was developed for the first time in history. And I must admit I have not found information refuting that here for the first time the notions of control and randomization were combined in a systematic way. Why then do I claim to have an alternative story?

Control-groups: from psychophysics to 'transfer of training'

To begin with, consider Peirce and Jastrow's report. All in all, those two sentences at the end of their article are a rather narrow basis for the claim that the ideal experimental design originates in *parapsychology*. At most, they give an indication of a possible underlying interest. The background to their research was also described by Jastrow in his autobiography.[12] There, Jastrow says nothing about a parapsychological stake. As is done in the original article, he refers to inspiration from psychophysics by Fechner and Wundt. It was that field of research in which his teacher Peirce wanted to intervene. If it has to depend on this article, there would have been at least as much reason to mention *psychophysics* as the context in which the ideal design originated.

There are more arguments for at least a partial background in psychophysics. In experimental designs of 19th century German psychophysics from before Peirce and Jastrow, so called *Vexirversuche* or 'deception trials' were included as a way of

control.[13] So, in this respect Peirce nor Coover did something new. Besides, by the end of the century in Germany and subsequently in America, several psychophysical experiments were conducted to investigate whether *training* of a sensory or motor function has a positive effect on other, untrained, capacities. For instance, it was investigated whether subjects trained in tapping their *right* big toe, had also improved their *left* big toe.[14] They had (I fantasize finding a picture of the scene: look-a-likes of Wundt, sitting on those old wooden laboratory chairs, socks and shoes off, tapping their right or left big toe. It must have been great fun in psychological laboratories of the time).

As far as I have been able to check, in these training experiments no randomizing or controlling was included. Nevertheless, they were of great importance to the origination of the kind of learning psychology, in which context the ideal experiment originated. This was mediated by the so called 'cult of efficiency,'[15] which in America of the Progressive Era was also settling into education. The generally posed question was how to get the highest yields with the lowest costs. Historians have amply documented this movement and the eugenic inspiration that was often connected to it.[16] In these histories it is told how researchers at Teachers College of New York's Columbia University together with researchers at Stanford University, Palo Alto, formed a firm web or even an 'Educational Trust' in their pursuit to hold all the strings of American education and educational administration.[17] In this 'hot bed of reform for education' many research projects and methodological treatises were written.[18]

In the Stanford-Columbia circle, the psychophysical experiments on the effect of training were placed in the context of a discussion on the doctrine of 'formal discipline.' Formal discipline was the name for the controversial theory stating that education must concentrate on strengthening general mental capacities, rather than on teaching practical knowledge and professional skills.[19]

The question educational psychologists posed in this context, was whether there is a 'transfer' from trained to non-trained mental capacities. Early examples of this kind of 'transfer of training' research, inspired by psychophysics, were given in a series of articles by the Columbia psychologists Thorndike and Woodworth in 1901.[20] Thorndike and Woodworth concluded that formal training was a myth after

experiments in which they first tested a function, next gave a training of another one, and after that tested the first one again. This implies that they used, what was later to be called the 'One-Group Pretest-Posttest Design,' and which nowadays is used in methodology handbooks as an example to explain all kinds of 'threats to validity.' Since the sixties, it has simply been denied the status of real experimental design.[21]

This is the moment Coover, who was saved from oblivion by Hacking, in fact already enters the origination-story of the ideal experimental design.[22] When Coover in 1912 was put to work on parapsychological research, he already had gained some experience of life. Having been raised on a farm in Kansas, it had not been a first option for him to go to university. He had finished a study of the Common Branches, had respectively become a telegraphist with the Union Pacific Railway, and next a journeyman, printer and publisher of a country newspaper. After that he had finished a State Normal School and had become a school principal. In 1899, at the age of 27, he had registered at the newly founded Stanford University. There, the skills he had acquired before not only ensured supplementary income, but they also guided his academic interests.[23]

Coover began studying with Frank Angell, who had still learned to do the job with Wundt himself.[24] However, as befitted a man of Coover's background, he soon gave Angell's psychophysics an applied swing. Already as a student, he started experimenting on the problem of 'transfer of training,' which in 1905 resulted in a Masters Thesis entitled *Formal Discipline from the standpoint of experimental psychology*. In the article on the same topic, which he subsequently published together with Angell, Thorndike and Woodworth are severely criticized. Subsequently some experiments are described on differences between the achievements of 'reagents' who had been given a training and 'control reagents' who had not. Both groups were subjected to a pretest and a posttest, and the results were compared.[25]

Historically speaking, this 1907 report in *The American Journal of Psychology* is eminently important. For, in contrast to what was done in the parapsychological projects Hacking discusses, in this educational article the idea of control was realized by deploying a control-*group*. Here, the procedure was not that one and the same person was subjected to different conditions. Rather in these experiments groups of reagents who had been subjected to a particular treatment were compared to groups

who had not.

Modern evaluation researchers also are well aware that giving different treatments to *different* subjects has an advantage compared to making a distinction by giving different treatments to the same subjects. For, about the latter method, the objection can easily be raised that the treatment of the first experiment might still have worked after the second experiment. It also has an advantage compared to 'transfer experiments' like those of Thorndike and Woodworth, where comparison was brought about by a 'pretest' rather than by control. When only the performances of one and the same group are scrutinized before and after a treatment, the objection can easily be made that most likely the pretest has functioned as a part of the training, or that the pretest has independently affected the results on the posttest.[26]

Unlike in educational experiments on 'transfer of training,' in psychical research on 'transfer of thoughts' it was not seen as a problem to subject the same reagents to both conditions 'transference and no transference.' Pretests did not have to be included in the design, because here no investigations of the 'before-and-after-treatment' type were at issue. From educational transfer experiments in particular, however, it was but *a small step* to the consideration that maybe between the variable treatments or between the pretest and the posttest there had been a transfer *too*. This occasioned designs with a control-group. It seems that it was Coover who was the first to take this step. At least, as far as the notion of control is concerned, the learning psychologist Coover could exploit more of his methodological ingenuity than Hackings parapsychologist Coover in 1917.

With this, Coover (1907) also appears to offer a correction of a much cited history of the control-group by Solomon in the *Psychological Review* of 1949 and a republication of Solomon's data by Boring in *The American Journal of Psychology* of 1954. Ever since these two articles, the British educationalist Winch is pointed out as the first one to have described control-groups in 1908. And the American psychophysicist Ewert in 1926 is mentioned as the first one to have also used the *word* control in relation to groups.[27] Actually, Coover was as much as nineteen years ahead of the latter.[28] And, spicy detail, Solomon on his turn appears not to have gone out gathering historical data himself. He simply took them from Woodworth's so called 'Columbia Bible' of 1938.[29] Woodworth remained silent about Coover and

Angell (1970) too. So, it actually was Woodworth *-of whose own work Coover's introduction of the control-group had been a correction at the time-* who denied Coover his rightful place in history.[30]

But that is only an aside. What really is at issue is that the notion of the non-(or differently) treated control-group was of great importance to the ideal experimental design. It was of more importance than the other notions of control that figured in the psychical experiments described by Hacking. Remarkably enough, it is precisely control via an untreated group, which Hacking himself mentions when he gives a *definition* of the ideal design. At the beginning of his story he states that it is a matter of control in the sense of 'untreated plots or persons.' Further on he also regularly indicates that it particularly is this kind of control which was highlighted by Fisher, and that it is this kind of control of which he himself will present an alternative origination-account. However, in his examples of concrete experiments, Hacking seems to have lost track of this description.

Controlling and randomizing in educational research

This neglect of the control-group causes the most important kind of *randomizing*, Hacking's main topic, to be kept out of the historical part of his article too. After some time, in the history of experimental research, the question arose of how it could be guaranteed that the experimental and the control-groups were equivalent in all other respects but the experimental condition.[31] This of course, to meet the objection that differences of results on a posttest can be ascribed to other systematic discrepancies between the groups than the experimental variable. After some more time -and it must be granted, mainly via Fisher's work-, the common solution to this problem became to assign individuals, mice, plots, plants or whatever, *by chance*. Whereas in psychophysical experiments randomization served to create chance-sequences, and in population research it served to construct representativeness, in comparative group experiments it served to construct equivalence.[32]

As to Hacking, here the same applies as to the idea of the control-group. Whereas this last kind of randomization is the core of his *definition* of the ideal experiment -he speaks of 'a notion of random assignment of treatment to a subset of the plots or persons, leaving the rest as controls'- this type of randomizing does not

return in the parapsychological material which he refers to in describing the early history of the ideal experiment. As there *were* no groups to compare, randomization of groups neither occurs in Peirce and Jastrow's article nor in that of the parapsychologist Coover.

However, it was not mentioned in the work of the learning psychologist Coover either. In the more extended version of my account, I report on the emergence of the 'groups equated by chance' in the same Stanford-Columbia circle, which some 15 years earlier provided the setting for the 'control reagent' to come on stage.

Notes:

1. I. Hacking (1988). Telepathy: Origins of Randomization in Experimental Design. *Isis, 7*, 427-451.

2. This paper is submitted to the program committee of the 13th annual conference of Cheiron Europe, to be held in Paris, September 1994. Trudy Dehue is associate Professor at the Section Foundations and History of Psychology, University of Groningen, Grote Kruisstraat 2/1, 9712 TS Groningen. Phone: +31 50 636354. Fax: +31 50 636304. Email: <G.C.G.Dehue@PPSW.RUG.NL>

3. The expression 'quasi experiment' has been introduced in the social sciences by the influential little book: D.T. Campbell and J.C. Stanley (1963). *Experimental and Quasi-experimental Designs for Research*. Chicago: Rand McNally.

4. R.A. Fisher (1935). *The Design of Experiments*. Edinburgh en London: Oliver and Boyd.

5. As to this methodological suspicion of psychologists, see the recent discussion in the *Psychological Bulletin* on parapsychology. These articles also offer an instructive image of the technical and conceptual complexities of nowaday's randomization (D.J. Bem and C. Honorton (1994). Does Psi Exist? Replicable Evidence for an Anomalous Process of Information Transfer. *Psychological Bulletin, 115*, 4-18; R. Hyman, *ibid.* Anomaly or Artifact? Comments on Bem and Honorton, 19-24; D.J. Bem, *ibid.* Response to Hyman, 25-27.

6. Nevertheless, it was Hacking who put me on the track, and part of my alternative picture is composed using jigsaw pieces he provided. Only, in my account these pieces contribute to another representation.

7. I confine myself to the development of designs for data *collection*. For the simultaneous history of data analysis in psychology see A.D. Lovie (1919) The analysis of variance in experimental psychology: 1934-1945. *British Journal of Mathematical and Statistical Psychology, 32*, 151-178; A.J Rucci and R.D. Tweney (1980). Analysis of Variance and the 'Second Discipline' of Scientific Psychology: A Historical Account. *Psychological Bulletin, 87*, 166-184.

8. Hacking also quotes the historian of statistics Stephen Stigler who wrote about this article that 'The Peirce-Jastrow experiment is the first of which I am aware where the experimentation was performed according to a precise, mathematically sound randomization scheme' [(S.M. Stigler (1978). Mathematical Statistics in the Early States. *Annals of Statistics, 6*, 239-265 (p. 248)].

9. In Hacking's quotation some mistakes have slipped in, which have been corrected here [C.S. Peirce and J. Jastrow (1885). On small differences of sensation. In: *Memoirs of the National Academy of Sciences, Vol III, Part I, 1884*. Washington: Government Printing Office, pp. 75-83 (p. 83)].

10. Not that it makes any difference, but it was in fact the fifteen year old and not the seventeen year old son, as Hacking notes. At least that is the age mentioned by Mrs Stanford's private secretary [B. Berner (1935). *Mrs Leland Stanford: An Intimate Account*. Stanford: Stanford University Press].

11. J.E. Coover (1917). *Experiments in Psychical Research at Leland Stanford Junior University*. Stanford: Stanford University Press. The book was reprinted with Arno Press, New York in 1975.

12. J. Jastrow (1930, repr 1961). Autobiography. In: C. Murchison, ed., *A History of Psychology in Autobiography, vol. I*. New York: Russell and Russell, pp. 135-163.

13. For instance: A. Camerer (1883). Versuche über den Raumsinn der Haut nach der Methode der richtigen und falschen Fälle. *Zeitschrift für Biologie, Neue Folge 1*, 280-300.

14. W.W. Davis (1898). Researches in Cross-education. *Studies of the Yale Psychological Laboratory, 6*, 6-50.

15. R.E. Callahan (1962, repr 1964). *Education and the Cult of Efficiency*. Chicago: The University of Chicago Press.

16. G.M. Joncich (1962). Science: New Age in Education. In: G.M. Goncich (ed.), *Psychology and the Science of Education; Selected writings of Edward L. Thorndike*. New York: Teachers College, pp. 1-26; Callahan. *Education and the Cult of Efficiency*; Tyack, D. and E. Hansot (1982). *Managers of Virtue: Public School Leadership in America, 1820-1980*. New York: Basic Books; D.A. Stout (1987). *Statistics in American Psychology; The Social Construction of Experimental Psychology, 1900-1930* (unpublished PhD Thesis, University of Edinburgh); K. Danziger (1990). *Constructing the Subject*. New York: Cambridge University Press; J. Brown (1992). *The Definition of a Profession; The authority of metaphor in the history of intelligence testing, 1900-1930*. Princeton: Princeton University Press.

17. Tyack and Hansot. *op cit*.

18. Stout. *op cit*.

19. W. Kolesnik (1958). *Mental discipline in modern education*. Madison: The University of Wisconsin Press.

20. E.L. Thorndike and R.S. Woodworth (1901). Influence of Improvement in One Mental Function upon the Efficiency of other Functions. *Psychological Review, 8*, 247-262; 384-395; 553-564.

21. Mainly since Campbell and Stanley, *Experimental and Quasi-experimental designs*.

22. Concerning this oblivion: Coover and Angell (1907) is shortly discussed in Kolesnik. *op cit*, p. 35 and p. 50. And Coover (1917) in E.R. Hilgard (1987). *Psychology in America: A Historical Survey*. San Diego etc: Harcourt Brace Jovanovich, p. 277 and p. 794.

23. J.E. Coover (1912). *Formal Discipline from the Standpoint of Experimental Psychology*. Stanford: Stanford University Press, p.308.

24. E.G. Boring (1929, repr 1950). *History of Experimental Psychology*. New York: Appleton-Century-Crofts, p. 324.

25. J.E. Coover and F. Angell (1907). General practice effect of special exercise. *American Journal of Psychology, 18*, 328-340.

26. In the vocabulary introduced by Campbell en Stanley, these objections respectively are called the 'threat to internal validity' named 'testing' and the 'threat to external validity' named 'interaction of testing and treatment.'

27. R.L. Solomon (1949). An Extension of Control Group Design. *Psychological Bulletin, 46*, 137-149; E.G. Boring (1954). The Nature and History of Experimental Control. *American Journal of Psychology, 67*, 573-589.

28. Coover (*op cit*, 1912, p. 18/19) refers to a book in German published in 1907, by a pupil of Meumann's, in which 'control reagents' figure also [P.R. Radossawljewitsch (1907). *Das Behalten und Vergessen bei Kindern und Erwachsenen nach experimentellen Untersuchungen*. Leipzig: Otto Nemnich]. So, as to this, Coover shares his first place with Radossawljewitsch. Because Coover does not literally use the term 'control group' (but 'control reagents' and 'control experiment') the medal for that can remain awarded to L.W. Kline (1914). Some experimental evidence in regard to formal discipline. *Journal of Educational Psychology, 5*, 259-266 (this reference stems from Danziger, *Constructing the Subject*, p. 226 note 42).

29. R.S Woodworth (1938). *Experimental Psychology*. New York: Holt, pp. 178-181. In this book the origin of the control-group is reported in exactly the same way. Whereas Solomon does not say that his account is completely based on Woodworth's, he does refer to the Columbia Bible and he specifies the pages involved in his bibliography.

30. In fact, Woodworth mentions himself as the inventor of the control-group: 'The control group in transfer experiments was employed in a small way by Thorndike and Woodworth (1901)' (*Experimental Psychology*, p. 178). Solomon repeated this. However, as Boring remarks, Thorndike and Woodworth 'did not fully appreciate its importance, for what they describe is only an incidental control experiment, to which they gave eight lines of description in a paper of 1400 lines' (Boring, Nature and History, p. 584). Woodworth must already have been working on the book at the time Coover and Angell published their critique. A mimeographed version of Woodworth's was already used for teaching at Columbia in 1909. There it also was given the nickname of 'Columbia Bible' [See A.S. Winston (1990). Robert Session Woodworth and the "Columbia Bible": How the psychological experiment was redefined. *American Journal of Psychology, 103*, 391-401]. Woodworth had an extra reason to just forget about Coover and Angell. When the first issue of his book was published in 1938, thousands of teachers had been convinced by references to 'Thorndike and Woodworth (1901)' that the doctrine of formal discipline was a myth; in 1930 the series of articles was still described as 'a veritable bombshell into the educational camp' (Kolesnik, *op cit*, p. 7 en 34). In another section than the one on the history of the control-group, this first issue of the book does mention an article by Coover from 1916. After some of Coover's data are presented, the snappy comment is added: 'Do these results indicate transfer?' (...) The results have been sometimes cited as evidence for positive transfer effect (...), but the reader must use his own judgement (Woodworth, *Experimental Psychology*, p. 198).

31. For that matter, soon multi-variate designs were used, comparing more than two groups. It is just for convenience that I mostly only speak of just two groups.

32. Constructing representativeness by chance has not been brought up in this paper yet. For a historical account: A. Desrosières (1991) The part in relation to the whole: How to Generalize? The prehistory of representative sampling. In: M. Bulmer, et al. (eds.). *The social survey in historical perspective*. Cambridge: Cambridge University Press; W.C Kruskal and F. Mosteller (1980). IV: The history of the concept. *Statistical Review, 48*, 169-195.

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Alfred Binet, and the theatre as a laboratory for metaphysics

ABSTRACT

In a short text published in 1897 in l'*Année psychologique*, Alfred Binet intended to criticize Diderot's *Paradox of acting*. His enquiry among actors during three years led to the same conclusions as those of William Archer, a dramatic critic in London. Binet knew Archer's works from William James' *Principles of psychology*. Binet's interest for theatre is well documented, but no details have been found about the conditions of this enquiry among actors. It is possible to refer to other enquiries made by him among other artists. More generally one can include this research into Binet's interest for enquiry methodolgy, and introspection. Concerning the theoretical aspect, Binet tried to develop a reflection about emotion, and wanted to prove that actors couldn't play without feeling some emotion. In his enquiry he emphasized the answers which confirmed this conception, and he looked for evidences of a tight link between mind and brain in the James-Lange theory of emotion, which he kept in high esteem though he found it two "intellectualist". He refered to Aristotle's philosophy of soul though he didn't agree with all its tenets. Binet's love for metaphysic was one of a well informed lover. Even his passion for theatre is somehow related to metaphysics, for the link between mind and body is constantly experimented on the stage, so that theatre is like a laboratory for metaphysics.

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Alfred Binet, et le théâtre comme laboratoire de métaphysique.

Dans un bref article publié en 1897 dans l'*Année psychologique*, Alfred Binet se propose de réfuter la thèse du *Paradoxe sur le comédien* de Diderot à partir d'une enquête menée pendant trois ans[1]. Guy Avanzini a montré que l'intérêt de Binet pour le théâtre est loin d'être marginal dans son oeuvre[2], et Jacqueline Carroy a souligné récemment comment Binet établit dans cet article, un parallèle entre les effets de l'hypnose et le jeu de l'acteur[3]. Concluant sur un rapprochement entre l'acteur et le "sujet suggestionné", Binet veut en finir avec l'idée de Diderot, selon laquelle l'acteur construit son jeu sur une pure simulation, teintée d'indifférence et de détachement.

Dans la problématique de cet article d'autres interrogations sont sous-jacentes. Binet se fait l'écho d'une enquête du même type réalisée par un critique de théâtre britannique, William Archer, publiée en 1888, à la suite d'une réflexion menée quatre ans plus tôt sur le *Paradoxe* de Diderot [4]. Ami de Bernard Shaw et traducteur de Ibsen, Archer a eu une grande influence sur le théâtre anglais de la fin du XIX° et du début du XX° siècles[5] . Son étude sur le jeu des acteurs est connue de Binet par William James qui en fait état dans les *Principles of Psychology* en 1890[6]. La double référence à Archer et à James peut servir de fil directeur pour comprendre l'enjeu de cet article polémique où Binet expose, sur un mode assez libre dans la construction et l'écriture, des préoccupations culturelles, théoriques et méthodologiques .

Binet ne connaît Archer qu'à travers ce qu'en dit James, mais il pense "*être arrivé aux mêmes conclusions que lui*". Les résultats en effet présentent une certaine convergence[7], mais le choix méthodologique de l'enquête

mérite aussi quelque intérêt. Il se trouve que Binet donne moins d'informations dans son article sur la façon dont il a procédé pour interroger les acteurs que ne le fait Archer, et le nombre de personnes interrogées est bien plus grand chez Archer, qui fait état de plusieurs dizaines de comédiens alors que Binet n'a que neuf témoignages. Archer donne la liste de ses questions, et commente longuement les réponses, sur près de soixante pages. Binet, lui, se contente, en seize pages, d'une synthèse de réponses à des questions dont on n'a pas la formulation exacte.

On peut situer vers 1893-1894 le moment où Binet a commencé son enquête, or on n'en trouve aucune trace dans les diverses revues scientifiques où il aurait pu publier son questionnaire[8], ni dans ce qui reste des archives familiales[9]. Malgré l'absence de tout document écrit concernant le questionnaire aux acteurs plusieurs éléments peuvent intervenir pour éclairer la démarche de Binet.

Tout d'abord, par analogie, on peut se référer à d'autres questionnaires. Par exemple celui sur la mémoire, adressé aux peintres, signé H. Beaunis et A. Binet, et publié dans la *Revue scientifique* en 1892[10]. Les 31 questions sont classées en catégories (distinction des mémoires visuelle, motrice et verbale, distinction entre mémoire des formes et des couleurs, etc.) . Il est demandé par exemple si l'on peut se représenter facilement et nettement un objet usuel (question 1), et si l'effort de mémorisation fait apparaître les objets sous forme visuelle (question 2) ou à travers les mouvements de la main pour dessiner (question 3). De même, en 1909, paraît dans l'*Année psychologique* le résultat d'entretiens avec le peintre Tade Styka, qui comporte des questions sur la mémoire visuelle, sur ses goûts en littérature, sur le choix des techniques etc[11]. Tout ce type de questions est en fait un appel à l'analyse interne des sujets par eux-mêmes, c'est-à-dire reposent sur l'introspection.

Or la position de Binet à l'égard de l'introspection est prudente et nuancée. Dans le chapitre VIII de l'*Introduction à la psychologie expérimentale* Binet analyse les mérites et les dangers des méthodes d'observation en général. Elles sont une source d'information indispensable, mais doivent cependant obéir à des règles d'interprétation, tant dans l'introspection que le psychologue exerce sur lui-même, et où il est à la fois juge et partie, que dans l'introspection collective et comparée, où l'on invite d'autres personnes à s'analyser. Il faut alors compenser la difficulté du contrôle par une bonne évaluation des réponses, les unes rares et accidentelles, les autres susceptibles de généralisation.

> "*Lorsqu'on ne fait pas de psychologie avec des appareils, qui par leur disposition même apportent un contrôle aux expériences, la garantie se trouve dans l'accord des observations*".[12]

En se référant aux résultats de Archer, Binet redouble en quelque sorte l'effet comparatif des réponses, et justifie, par la convergence des résultats les mérites de la méthode[13].

Enfin, son désir de comprendre la psychologie humaine se traduit par un vif intérêt pour la réflexion interne des artistes, acteurs et peintres comme nous l'avons vu, musiciens[14] , mais aussi écrivains et auteurs dramatiques. L'article co-signé avec Passy dans l'*Année Psychologique* en 1895 est le résultat d'entretiens personnels, comme l'attestent quelques lettres échangées avec l'un des frères Goncourt en automne 1893 à propos de l'imagination.

> "*Vous dîtes que <u>l'imagination est un être raisonné</u> (je cite de mémoire n'ayant pu retrouver le passage). Cette définition, vous serait-il possible de la reprendre et de la développer ? La question est extrêmement importante pour nous, psychologues, car jusqu'ici on ne sait rien sur le mécanisme de l'imagination artistique[...] Si vous pouviez nous dire votre pensée là-dessus, vous qui avez pratiqué pendant 40 ans l'observation de vous-même, comme nous vous en serions reconnaissants !*"[15]

Ainsi, quelles que soient les modalités précises par lesquelles Binet a effectué son enquête auprès des acteurs, sa démarche entre dans un ensemble de recherches où se révèle son attachement aux témoignages individuels, et à leur complémentarité à l'égard de l'expérimentation en laboratoire, au point qu'il s'ouvre à toute source d'information. L'enquête d'un non professionnel de la psychologie comme Archer peut ainsi avoir quelque mérite à ses yeux.

Mais le destin de l'enquête de Binet demeure étrange. Non seulement rien n'est dit sur les conditions méthodologiques, mais Binet annonce une suite, confiée à Courtier, l'un des co-auteurs de l'*Introduction à la psychologie expérimentale*, et qui n'a sans doute pas été réalisée[16]. Pourtant il ne semble pas que Binet ait renoncé aux réflexions qui le portaient à s'intéresser aux acteurs. Sans même parler de sa propre création théâtrale en collaboration avec André de Lorde, qui se prolonge jusqu'à la veille de sa mort [17], des questions telles que nature de l'émotion et le rapport entre l'âme et le corps, bien présentes dans sa réflexion sur le *Paradoxe* de Diderot, continuent à le préoccuper dans d'autres textes[18].

Les pages de William James où Binet découvre William Archer contiennent précisément de quoi alimenter une réflexion sur ces questions[19]. James y développe une longue citation de Burke à propos de Campanella: on raconte que l'auteur de *La cité du soleil*, lorsqu'il désirait savoir ce qui se passait dans l'esprit d'une autre personne, imitait son attitude et sa physionomie en concentrant en même temps son attention sur ses propres émotions[20]. Binet revient sur ce passage dans l'*Année psychologique* de 1902, à l'occasion d'un compte rendu de la traduction par Georges Dumas des trois textes du psychologue américain - parmi lesquels le chapitre XXV des *Principles* [21] - rassemblés sous le titre *La théorie des émotions* [22]. A

cette occasion Binet reproduit intégralement la citation de Burke et toute la problématique développée par James sur le jeu des acteurs, avec bien sûr la référence à Archer. Binet évoque alors sa propre enquête sur les comédiens. Il cite les réponses faites à Archer sur lesquelles James s'étend quelque peu, et où l'on voit les acteurs décrire leur émotion à travers les effets physiques qu'ils éprouvent en jouant: pâleur, transpiration, déssèchement de la bouche. Mais comme certaines réponses vont dans le sens d'une dissociation entre le jeu expressif et l'émotion ressentie, Binet s'émerveille devant l'ingéniosité de James qui parvient à "*ramener l'exception sous la règle*" . James dit en effet:

> "*L'explication de la contradiction entre acteurs est probablement celle-là même que ces citations* [il s'agit des réponses à l'enquête de Archer] *suggèrent. La partie viscérale et organique de l'expression peut se supprimer chez certains hommes, mais non pas chez d'autres, et c'est de là que dépend probablement la partie essentielle de l'émotion ressentie.*"[23]

Dans sa propre enquête, Binet s'attache d'ailleurs lui aussi aux réponses qui vont dans le sens de sa critique de Diderot, au risque de ne pas respecter les règles qu'il s'est données dans sa réflexion méthodologique. Par exemple M. Worms, grand premier rôle de la Comédie Française dit que certains acteurs "*soutiennent qu'on doit jouer sans rien sentir*" . Binet se plaît à rapporter la restriction qui suit : "*mais j'ai remarqué que les partisans de cette thèse sont en général des natures très sèches, incapables de sentir pour leur propre compte*".[24] Quant à M. Mounet-Sully[25] et son frère M. Paul Mounet, ils disent que "*l'art du comédien consiste précisément à réaliser toutes les émotions d'un rôle avec l'intensité de la vie réelle*", et ils ajoutent qu'on n'y arrive pas toujours, et que"*les jours où l'émotion est absente, [...] on joue avec son talent, c'est-à-dire avec son raisonnement et son métier, et on cherche à reproduire artificiellement ce qu'on faisait le soir où on avait le feu sacré*"[26]

Ces nuances sont intéressantes, mais Binet ne s'y attarde pas. Il ne retient que les déclarations qui montrent qu'un bon jeu repose sur le ressenti

du comédien, il ne retient pas qu'il est *possible* de jouer *artificiellement.*

Derrière ce refus catégorique de dissocier le sentiment et l'expression se profile une question qui lui tient à coeur, et qui fait l'objet d'un ouvrage publié en 1905, *L'âme et le corps*[27]. Sa volonté de réfuter Diderot peut être éclairée rétrospectivement par l'intérêt qu'il y marque pour la théorie dite de James -Lange[28]. Puisque selon cette nouvelle théorie, on ne doit plus dire "nous rencontrons un ours, nous avons peur, et nous nous enfuyons"etc., mais "nous sommes effrayés parce que nous tremblons"etc.[29], le lien entre l'expression physique et le sentiment psychologique demeure indissociable. Binet va jusqu'à dire que "*la définition de l'émotion telle qu'elle est préconisée par W.James semble faite exprès*" pour lui qui cherche "*à résoudre tous les états intellectuels en impressions physiques accompagnées de conscience*"[30]. Pourtant, Binet ne suit pas "*le vraiment génial W.James*" jusqu'au bout[31]. Il déclare déjà dans son commentaire de 1902 que dans la théorie de James, l'émotion se réduit finalement à n'être que la conscience de certaines sensations, et donc un acte intellectuel[32]. Or, pour ne pas oblitérer la nature spécifique de l'émotion il faut préserver "*un quelque chose qui n'appartient pas au domaine physique, et qui forme le monde moral*"[33]. Pour éviter les pièges du dualisme et du parallélisme, il faut considérer que l'esprit et la matière sont "*aussi inséparables que le mouvement et l'objet qui se meut*"[34]. Cette conception aristotélicienne, empruntée à Alexander Bain[35] - Binet se dit non "*assez érudit*" pour consulter les sources lui-même"[36] - ne résout pourtant pas la question de ce que peut signifier un "*monde moral*" . Binet conclut: "*C'est une question que je laisse sans réponse*"[37]. Il y a un contraste étonnant entre cette ardeur à réfuter la dissociation de l'âme et du corps, et cet aveu d'impuissance métaphysique. Aveu qui va de pair avec le souci de rigueur qu'il affiche en maintes circonstances. Par exemple, lorsqu'il soumet sa thèse sur l'esprit et la matière aux membres de la Société française

de philosophie en 1904, en présence notamment de Bergson et de Lachelier il s'expose honnêtement à leurs critiques[38].

Tout au long de sa vie Binet se livre ainsi avec passion à des activités pour lesquelles il n'est pas vraiment spécialiste, le théâtre et la métaphysique. Dominique Hocquart a montré que le théâtre et la science ne devraient pas chez Binet être conçus comme dissociés, mais complémentaires[39]. Theta H. Wolf a salué les envolées métaphysiques de Binet, au point d'en faire l'appendice de son ouvrage[40]. J'ajouterai que la passion pour le théâtre, ce lieu où le corps concentre les émotions, où s'expérimente indéfiniment le lien entre l'âme et le corps, comme en un véritable laboratoire de métaphysique, n'est pas étrangère à ce goût pour les mystères de la pensée.

NOTES

1 Alfred BINET, "Réflexions sur le paradoxe de Diderot", *Année psychologique*, 1897, pp.279-295.
Denis DIDEROT, *Le paradoxe sur le comédien*, [écrit en 1770, Ed. posthume 1830], Paris, Garnier-Flammarion, 1980.

2 Guy AVANZINI, *Alfred BINET, Ecrits psychologiques et pédagogiques*. Toulouse , Privat, 1974. On trouvera dans cet ouvrage la liste des pièces écrites par Binet, en collaboration avec le dramaturge André de Lorde, p. 173.
Voir aussi des articles théoriques de Binet sur le monde du théâtre, vu d'un point de vue psychologique:
A. BINET & J. PASSY, "Etudes psychologiques sur les auteurs dramatiques", *L'année psychologique*, 1° année, 1895 (1895) , pp. 61-118.
A. BINET, "M. François de Curel, note psychologique", *L'année psychologique*, 1° année, 1895 (1895) , pp. 118-173.
Voir aussi la thèse d'une complémentarité entre l'oeuvre psychologique et l'oeuvre dramatique de Binet, dans Dominique HOCQUART, "Alfred Binet ou les coulisses de la psychologie expérimentale", *in* Antoine SAVOYE et Rémi HESS (Dir.), *Perspectives de l'analyse institutionnelle*, Paris, Méridiens Klincsieck, 1988, pp.13-31. (Je remercie Jacqueline Carroy d'avoir attiré mon attention sur ce texte).

3Jacqueline CARROY, *Les personnalités doubles et multiples. Entre science et fiction*, Paris, PUF, 1993.
Voir également Jacqueline CARROY, *Hypnose, suggestion et psychologie. L'invention de sujets*, Paris, PUF, 1991.

4 William ARCHER, "Diderot's paradox of Acting", *The Theatre*, march 1884, pp. 117-126.
William ARCHER, "The Anatomy of Acting", *Longman's review*, Vol XII, 1888, pp.266-281, 375-395, 498-516.

Voir aussi William ARCHER, *English Dramatists of Today*, London. Sampson Low, Maroton, Searle &Ruvington. 1882.
William ARCHER, *About the Theatre. Essays and Studies*, London. T. Fisher Unwin. 1886.

[5] On trouvera des indications biographiques sur William Archer (1856-1924) dans le *Dictionary of national biography*. 1937, ainsi que dans l'*Enciclopedia dello spectacolo*, Roma, 1954, ou l'*Enciclopedia universal ilustrada europeo americana*, Bilbao, Madrid, Barcelona, 1930, ou encore le *Who was who* de Adam& Charles Black, London. [1929], 1962, Vol II.

[6] William JAMES, *Principles of Psychology*, [1890], London, Macmillan and Co. Ltd., 1901(Ch. XXV). C'est dans ce même chapitre que James développe sa théorie des émotions, parallèle à celle du danois Carl Georg Lange (1834-1900). Lange publie ses travaux sur les émotions en 1885, traduits en 1887 en allemand, et en 1895 en français par Georges Dumas.

[7]Voir à ce sujet Anne-Marie DROUIN-HANS, "Sémiologie du geste, jeu théâtral et simulation: l'artifice au service de l'explication du vivant", Communication au colloque *El juego de la vida, la vida en juego (Les jeux de la vie, la vie en jeux)* 25-29 octobre 1993, Acapulco, Mexique. A paraître dans *Ludus Vitalis*, revue de philosophie des sciences de la vie, Mexico et Barcelone.

[8]Dans son *Introduction à la psychologie expérimentale*, Binet signale que ses travaux du Laboratoire de psychologie physiologique "*ont été publiés en partie dans les* Bulletins de la Société de psychologie physiologique, *dans la* Revue scientifique, *la* Revue Générale des sciences, *et dans le* Bulletin des travaux du Laboratoire. " Alfred BINET, *Introduction à la psychologie expérimentale*. Paris, Alcan, 1894, p. 6.

[9]Je remercie vivement mesdemoiselles Géraldine et Georgette Binet, petites filles d'Alfred Binet, de m'avoir donné tous les renseigements qu'elles avaient à leur disposition, et il semble bien que si un questionnaire écrit a existé, il ait disparu avec d'autres archives.

[10] H. BEAUNIS et A. BINET, Laboratoire de psychologie physiologique de la Sorbonne (Hautes Etudes), "Questionnaire de psychologie adressé aux peintres", Extrait de la *Revue scientifique*. Paris, administration des deux revues, 111 boulevard St Germain, 1892, 14 p.

[11]Alfred BINET (avec la collaboration de Alice BINET), "La psychologie artistique de Tade Styka", *L'année psychologique*, Tome XII, 1909, pp. 316-356.

[12] Alfred BINET *Introduction à la psychologie expérimentale*, Paris. Alcan. 1894, p. 141.

[13] Il faut préciser que Binet, outre l'introspection par le psychologue lui-même, et l'introspection comparée, se réfère à un troisième type de d'observation, les observations des moeurs, modes et coutumes, qui se rattachent à "*l'étude des peuples désignée en Allemagne sous le nom de Völkerpsychologie*".Alfred BINET *Introduction à la psychologie expérimentale*. Paris, Alcan. 1894, p. 133.
Voir aussi pour l'analyse de l'introspection: François-Louis BERTRAND. *Alfred Binet et son oeuvre*, Paris. Alcan. 1930, pp. 116-122.

[14]Des questions destinées à étudier la mémoire musicale illustrent ses réflexions sur les méthodes d'observation qui viennent d'être évoquées .

[15]Lettre de Alfred Binet à, Edmond Huot de GONCOURT, 20 novembre 1893, manuscrit conservé à la Bibliothèque Nationale. Une autre lettre datée du 30 novembre fait état d'une réponse de Goncourt à propos du sens qu'il donne au mot "raisonné", et demande des précisions sur le rôle du rêve dans l'imagination.

[16] On ne trouve trace de ce travail annoncé , ni dans l'*Année psychologique*, ni dans la *Revue générale des sciences*, ni dans la *Revue scientifique*, ni dans le *Journal de psychologie normale et pathologique*. Les *Archives de psychologie* de 1909 et de 1910 signalent encore deux articles de Courtier mais sur de tout autres sujets. Jules Courtier (1860-1938), un des premiers collaborateurs de Binet, devint en 1911 directeur adjoint du Laboratoire de

physiologie des sensations dirigé par Charles Henry. (Je remercie Serge Nicolas pour les renseignements biographiques sur Courtier.)

17 *L'homme mystérieux* , drame en deux actes, par exemple, est joué au théâtre Sarah Bernhardt en novembre 1910 . D'autres pièces sont jouées après la mort de Binet, comme *Les invisibles* (1912, Ambigu) ou *Crime dans une maison de fou* (1925, Grand Guignol). Voir le répertoire théâtral d'Alfred Binet dans Guy AVANZINI, *Alfred BINET, Ecrits psychologiques et pédagogiques*. Toulouse , Privat, 1974, p.173.

18 Par exemple, L'année même de sa mort il fait une communication sur "La nature des émotions" devant la Société de psychologie. Alfred BINET. "La nature des émotions". Société de psychologie, séance de mars 1911, *Journal de psychologie normale et pathologique*, 1911, pp. 258-259.

19 "*Psychologie, II, p. 464*," ce qui correspond à l'édition anglaise.

20 Voir Edmund BURKE, *Recherches philosophiques sur l'origine des idées que nous avons du beau et du sublime*, Londres, Hochereau, 1765, pp.107-109.

21 William JAMES, *La théorie de l'émotion*, Trd. et intr. Georges DUMAS, Paris, Alcan, 1903. Les trois textes de William James, rassemblés et traduits par Georges Dumas sont:
-"What is an Emotion ?", *Mind*, IX, 1884
- *Principles of psychologt*, New York, 1890, ch. XXV (L'édition de Dumas donne une référence fautive en indiquant le ch. XXIV)
-"The physical Basis of Emotion", *Psychological review*, I, 5, 1894.

22Alfred BINET, compte rendu de l'ouvrage de William James, *La théorie des émotions*, Trad. avec une introduction de G. Dumas. Paris, Alcan, 1902, in *L'année Psychologique*, 1902, pp. 388-401.

23Alfred BINET, compte rendu de l'ouvrage de William James, *La théorie des émotions*, Trad. fr. avec une introduction de G. Dumas, Paris, Alcan, 1902, in *L'année Psychologique*, 1902, p. 398.

24Alfred BINET, "Réflexions sur le paradoxe de Diderot", *Année psychologique*, 1897, p. 285.

25Mounet-Sully fut le second mari de la mère de André de Lorde. (Je dois à Mesdemoiselles Binet ce renseignement qu'elles ont trouvé dans François RIVIERE et Gabrielle WITTKOP, *Grand Guignol*, Ed. Henri Veyrier, 1979.)

26Alfred BINET, "Réflexions sur le paradoxe de Diderot". *Année psychologique*, 1897, p. 285.

27Alfred BINET, *L'âme et le corps*, Paris, Ernest Flammarion, 1905.

28Alfred BINET, *L'âme et le corps*, 1905, pp.93-97.

29William JAMES, *La théorie des émotions*, 1903, p.60-61.

30Alfred BINET, *L'âme et le corps*,1905, p. 95.

31Alfred BINET, *L'âme et le corps*,1905, p. 95.

32 *"D'après la définition même de James - et on m'excusera, on me le dira si je l'ai mal compris - l'émotion serait une conscience de certaines sensations:[...] la conscience est un acte intellectuel: éprouver des sensations c'est faire fonctionner son entendement. Je ne vois pas bien en quoi cela ressemble à éprouver une émotion."* Alfred BINET, compte rendu de l'ouvrage de William James, *La théorie des émotions*, Trad. fr. avec une introduction de G. Dumas, Paris, Alcan, 1902, in *L'année Psychologique*, 1902, p. 401.

[33]Alfred BINET, *L'âme et le corps*,1905, p. 97

[34]Alfred BINET, *L'âme et le corps*,1905, p. 189.

[35]Le recours à Aristote amène Binet à définir l'intelligence et l'émotion comme "*des phénomènes non de l'organisme corporel simplement, ni du Noûs simplement, mais de la communauté ou de l'association dont ils sont membres*". Alfred BINET, *L'âme et le corps*,1905, p. 194.
Alfred Binet dit avoir repris les termes mêmes dans lesquels Bain analyse la psychologie d'Aristote dans*Les sens et l'intelligence*. Dans *L'esprit et le corps*, Bain reprend cette analyse d'un point de vue un peu plus distancé.

[36]Alfred BINET, *L'âme et le corps*,1905, p. 191. On peut préciser que cette modestie de Binet s'explique sans doute par la difficulté de trouver une traduction française du *De anima* d'Aristote: la seule traduction française existante à l'époque où Binet écrit, date de 1846: ARISTOTE. *Psychologie d'Aristote. Traité de l'âme*, Traduit en français pour la première fois et accompagnée de notes perpétuelles. par J. Barthélemy Saint-Hilaire. Paris, Ladrange. 1846. (Catalogue B.N., t.IV. 1900)
Par ailleurs, dans son hommage à Binet, J. Larguier des Bancels commente en ces termes *L'âme et le corps* et les articles qui sont venus compléter cet ouvrage : "*Ils possèdent [...] cette saveur originale que l'on goûte avec tant de plaisir dans les ouvrages philosophiques des savants demeurés longtemps étrangers aux traditions de l'école. Nul verbalisme chez Binet; nulle jonglerie. L'auteur n'esquive jamais la difficulté, et quand il désespère de pouvoir la résoudre, il reconnaît les limitres de son hypothèse avec cette bonne foi qui forme l'un des traits caractéristique de ce rare esprit*". J. LARGUIER DES BANCELS, "L'oeuvre d'Alfred Binet", *L'année psychologique*, 1912, pp. 15-32 (p.28).
Voir aussi dans ce même numéro: Th. SIMON, "Alfred Binet", pp.1-14

[37]Alfred BINET, *L'âme et le corps*,1905, p. 97

[38]*Bulletin de la Société française de philosophie*, Tome V, 1905. Séance du 22 décembre 1904 (pp. 73-101). Dans un article sur "Cerveau et pensée", en 1906, il reprend la réflexion sur la base d'une critique du parallélisme et du matérialisme. Alfred BINET, "Cerveau et pensée", *Archives de psychologie*, tome VI, 1907, pp. 1-26.

[39]Dominique HOCQUART, "Alfred Binet ou les coulisses de la psychologie expérimentale", *in* Antoine SAVOYE et Rémi HESS (Dir.), *Perspectives de l'analyse institutionnelle*, Paris, Méridiens Klincsieck, 1988, p.14.

[40]Elle termine son ouvrage sur "Binet's flight into metaphysics". Voir Theta H. WOLF. *Alfred Binet*, Chicago, London, The University of Chicago Press, 1973, pp. 339-347.
Voir aussi Theta H. WOLF, "Alfred Binet et le théâtre", texte traduit de l'américain par Germain Paquet, *Bulletin Alfred Binet*, n° 598, 1984, pp. 121-137. (Article paru dans *The psychological record*, 1982, 32, pp. 397-407)

THE SOCIAL PSYCHOLOGY OF ALFRED BINET (1857-1911)

ABSTRACT

In this paper it is proposed that Binet developed a social psychology which is often overlooked. In his work at the Salpêtrière he witnessed interpersonal influence through suggestion. Unlike LeBon and Tarde, Binet did not use the metaphor of suggestion, but investigated suggestion directly and experimentally. He went on to study suggestibility mainly in school children. From experiments he reported the "introspections provoquées" which set up dialogues or interviews. In a key work, La Suggestibilité, Binet also examined the dynamics of group behaviour. Finally, he developed a social psychology within testing situations. His death in 1911 deprived him of the opportunity of pursuing a social psychology of which we see the beginnings in some of his published work.

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THE SOCIAL PSYCHOLOGY OF ALFRED BINET (1857-1911)

Binet (1857-1911) worked in various domains within the developing discipline of psychology, but saw himself primarily as an experimental psychologist, and his fame undoubtedly rests on his construction of the first Scales of Intelligence. (1905,1908 and 1911). Nevertheless, I shall argue here that in spite of the seeming invisibility of Binet's social psychology, a closer look at even his experimental work and his intelligence tests leads to a recognition of this little examined aspect of his work.

In 1909, in his stocktaking or "bilan" of psychology of the year 1908 Binet made a plea for social psychology. Erica Apfelbaum (1986) referred to Binet's nascent interest in social problems, and his interest was certainly stimulated by his membership of, and work within the pedagogical society, La Société Libre pour L'Etude Psychologique de L'Enfant. In the laboratory of the "show school" in the Rue Grange-aux-Belles Binet and Vaney collected data on the children's social and economic backgrounds. However, it is not just through his interest in social problems that we can claim to find the social psychology of Binet. We may examine his experimental work and indeed find it there.

The period between 1887 and 1890 when Binet worked at the Salpetriere hospital under the direction and influence of Charcot has been well documented. It is also known that Binet turned his back on Charcot's methods and assumptions, and from about 1892 renounced his allegiance to the Paris School of hypnotism. However,

the two rival schools of hypnotism, namely the latter and the Nancy school provided the popular writer, LeBon and the sociologist, Gabriel Tarde with the metaphor of suggestion. The language of suggestion was that of the Bernheim school of Nancy and the strong pathological associations came from the practice of the Salpetriere. Social phenomena were described by Tarde mainly in terms of imitation, and crowd behaviour by LeBon in terms of pathology; and in their descriptions this model became the dominant one to explain interpersonal influence. The two main works of these men, "Les Lois de L'Imitation" (1890) and "La Psychologie des Foules" (1895) respectively, are recognised as pivotal in the development of early social psychology in France. Therefore an enquiry into Binet's social psychology might well take as its starting point an examination of his position with regard to the general lines of thought of LeBon and Tarde.

Binet differed from both of them in that he had a reluctance to theorise and did not produce any well formulated theory of social relations. He therefore had little use for the suggestion metaphor as used particularly by LeBon. Another reason for his non-use of this metaphor was that he had a literal and <u>direct experience</u> with the mechanism of suggestion; and the evolution of Binet's conception and use of suggestion relates to his psychological practice. At the Salpetriere he witnessed the power of suggestion exercised by the hypnotist, and hinted at a connivance between the hypnotist and subject for the benefit of the audience of Charcot's lessons. In his experiments on "Animal Magnetism" (1887) Binet saw

suggestion as the variable for which he had to control in order to show the effects of transfer. When he abandoned his studies of animal magnetism and his work under Charcot this marked the end of his studies of the pathological. He had thereafter little use for the suggestion model in its pathological connotations. He went on to investigate the role suggestion in "normal" psychology.

Binet's entry into normal psychology began with "De la Suggestibilité Naturelle chez les Enfants" (1894). This work reported experiments which he made on school children. He manipulated the experimental stimuli in such a way that produced an "idée directrice" or "routine" which led the subjects to make an inaccurate reproduction of the stimuli. This study was later expanded and published under the title of "La Suggestibilité" in 1900. Three crucial aspects of Binet's social psychology can be found in this key work. Firstly, Binet observed that it was not just the "idée directrice" which influenced perceptions of the task, but often and more importantly the personal influence which resulted from the experimental situation which he called "la suggestion morale". Secondly, Binet identified suggestibility as a personality trait, which in some cases he termed docility or even educability. Thirdly, Binet produced the introspections of his experimental subjects and these were of an innovatory and even radical nature. It was Binet's belief that the use of "introspections provoquées" (ie. introspections evoked or elicited by the experimenter) extended the experiment and contributed to its interpretation. Binet questioned the children about their

performances and errors of judgement in order to identify the mechanism of suggestion. These introspections revealed the mind of the child who had been misled. Among the reasons were: performing for approval; a belief that the teacher is right, and therefore should be believed even if his statements contradicted the child's own judgement. As Binet reported his questions and probings, together with the children's replies, we witness dialogues, but of the formal kind - ie. interviews. Binet's aim was to gain the child's viewpoint, and as Farr (1988) has proposed, the social encounters between two persons with different perspectives create what he terms an "inter-view" - social psychology at the micro level.

Mainly through his experiences in schools Binet came to believe that the strongest social pressure came through language particularly in that of an authority figure. He became increasingly aware of the effects of the imbalance of power in the experimenter/subject and teacher/child situation. In the one-to-one testing situation Binet noted how the presence of the headteacher (proviseur) could detrimentally affect the child's performance by introducing an element of fear. Deference and fear were not confined to the teacher/pupil situation, for similarly he noted the performance of soldiers when one of their superiors was present. The key to all Binet's observations lay in his attention to detail or his finesse and his sensitivity. He observed the child's behaviour during a task or test: he became aware of discomfort or embarrassment in their silences (mutisme). He allowed

subjects to save face and to be spared a sense of failure. In all of these observations he was producing data for social psychology, observations from experimentation and in other situations - the home, school, hospital, asylum and barracks.

But as he claimed, he was not inspired by theoretical ideas as nothing could be gained except through observation and experimentation. He identified two main effects attributable to group participation. The one weakened both suggestibility and shyness; the other reinforced suggestibility while relaxing discipline and attention. The latter effect overrode the first, thus resulting in increased suggestibility. His experiments with groups of different sizes led Binet to propose three main group characteristics: in chance grouping there was no solidarity; grouping increased suggestibility laughter and imitation (contagion de l'exemple). In comparing the behaviour of five different groups he found that they varied in terms of imitation and emergence and quality of leadership. Finally he proposed three group characteristics, namely division of functions, increased suggestibility and a tendency to imitate. He also noted that groups formed an internal organisation and produced their own hierarchies. In his identification of imitation and his proposal that social imitation requires little effort, Binet comes close to the thesis of Tarde. The difference, however was that while Tarde spoke of the "chaîne de magnétisations" Binet emancipated himself from the theoretical unidirectional model of influence. Binet's implicit model of influence, drawn as it was from his observations, showed

influence to be shifting and relative according to the composition of the group. At this point Binet was conscious of the nature of his psychological work: he recognised it as part of "la Psychologie des Foules".

Finally we may discern the social psychology of Binet's intelligence testing. It is to be found in the method of administering the items of the Scale, the method being inseparable from the test items. A brief comparison with mental testing prior to 1905 illustrates this fact, for one of the assumptions which that underlay the testing movement was that intelligence was inherited and fixed. This meant that social factors were irrelevant to the testing situation; and as the performance of the individual was to be related to a norm, impersonality was often its hallmark. The individual testing procedure devised and prescribed by Binet and Simon was in strong contrast for they gave detailed advice not only for the standardization but also for the conduct of this social interaction or encounter.

In conclusion we may propose that experimentation did not preclude social psychology. Binet's relatively short life meant that his embryonic social psychology did not come to term. Had it developed earlier and before the death of Tarde, their proposed collaboration in the pursuit of an "inter-psychologie" may have added another dimension to French social psychology as it developed in the last decades of the nineteenth century.

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POUR UNE ETUDE DE L'HISTORICITE DES COMPORTEMENTS

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Une contribution importante de l'histoire de la psychologie à l'épistémologie interne de la discipline est la mise en évidence de l'historicité des théories psychologiques, qui s'enracinent dans une époque et en portent la trace. On s'est efforcé de montrer, par exemple, le rôle du contexte socio-historique dans la formation de la psychanalyse, du behaviorisme, de la théorie des aptitudes intellectuelles ou de la théorie piagétienne du développement cognitif. La lumière ainsi jetée sur l'influence de l'environnement social autorise un questionnement critique des théories concernées et dissipe les illusions d'une approche purement internaliste de la discipline (Mengal, 1988). Cependant, pour éviter les insuffisances d'une position relativiste (Matalon, 1986) menant à des apories (Rée, 1992), l'histoire de la psychologie doit tenir compte aussi bien des déterminants internes que des déterminants externes (Apfelbaum, 1988; Watson; 1977).

Mais il existe une deuxième forme d'historicité de la psychologie, et c'est elle qui sera au centre de notre communication. Le comportement étudié par la psychologie est celui d'un homme qui vit à une époque historique précise (phénomène souligné à juste titre par Parot, 1992). Si, comme on a quelques raisons de le supposer, le comportement est partiellement déterminé par le contexte historique, les lois psychologiques sont en partie historico-dépendantes. L'historicité de la psychologie tient donc aux variations temporelles de son objet d'étude, autant qu'à la manière dont le psychologue conceptualise cet objet. Bien que ce point de vue ait été exprimé de façon récurrente par un certain nombre d'auteurs, il reste très minoritaire. La plupart des psychologues sont convaincus de l'intemporalité de leurs observations et de leurs lois. La variabilité historique des conduites exige donc à la fois des justifications théoriques et des preuves empiriques.

Le temps modifie, plus ou moins profondément, plus ou moins largement, la culture d'un groupe humain. Selon le groupe et l'intervalle entre les périodes comparées ces changements constituent des variations d'une même culture ou des différences beaucoup plus importantes, assimilables à des différences de culture. L'étude des variations historiques et et celle des variations culturelles ont donc le même fondement théorique. Les mêmes raisons qui expliquent qu'un comportement varie dans l'espace justifient qu'il puisse varier également dans le temps. Parmi ces raisons, la principale est sans doute l'existence de cadres organisateurs de la pensée et de la conduite, variables selon les cultures.

Deux objections méritent d'être considérées. La première consiste à faire valoir que l'unité neurophysiologique de l'espèce humaine rend vraisemblable l'universalité de certaines opérations mentales basiques. On a montré (Rosch, 1973) par exemple que la reconnaissance des couleurs ne subissait pas de variation interculturelle, alors que le découpage du spectre lumineux en couleurs varie considérablement selon les époques et les cultures[1]. Mais le système nerveux ne fait qu'imposer des limites aux variations historico-culturelles, et l'identification des invariants psychiques nécessite des comparaisons dans l'espace et le temps. La deuxième objection repose sur la distinction entre contenus et processus: les premiers varieraient selon les époques et les cultures, tandis que les seconds constitueraient des universaux. La psychologie aurait pour tâche de formuler des théories ne faisant pas intervenir les contenus et contextes historico-culturels (Schlenker, 1974). Mais la séparabilité des contenus et des processus est mise en doute par de nombreux auteurs. D'autre part, la mise à l'écart des contenus peut nuire à la compréhension des processus. S'agissant des processus cognitifs par exemple, des cognitivistes, et non des moindres (e.g. Neisser, 1976; Morton, 1981), considèrent que l'évacuation des contenus stérilise la psychologie, tandis que des psychologues sociaux estiment que les limites des modèles cognitivistes « incitent à une réflexion sur le caratère social et historique de la cognition» (Jodelet, 1989). On peut se demander également avec Petitone et Triandis (1987) si l'apparente universalité de certaines théories (celle de la dissonnace cognitive par exemple) n'est pas illusoire, car elle se fonde sur des mécanismes intrapsychiques dont la généralité apparente pourrait ne reposer que sur des présupposés culturels discutables.

Le manque d'attention aux variations temporelles des comportements s'explique sans probablement en partie par le peu de recul historique dont dispose la psychologie. Il n'est pas très étonnant que des lois établies il y a un siècle au plus paraissent éternelles. Cependant, un doute sur l'immutabilité de ces lois est apparu dans trois secteurs de la psychologie.

Le premier est celui de la psychologie du développement. Les comparaisons du développement de cohortes différentes ont été stimulées par les travaux de Schaie. Cet auteur a montré (Schaie & Strother, 1968) que la prétendue baisse des performances cognitives à partir de vingt-cinq ans était due à des différences intergénérations, confirmées par de nombreux travaux ultérieurs. Les comparaisons inter-cohortes portant sur des enfants paraissent indiquer que le développement cognitif a subi une accélération au cours des dernières décennies (Flieller, 1989), qui n'est pas sans rappeler celle que l'on observe pour la croissance physique (stature moyenne plus élevée, puberté plus précoce). Plus

[1] Des travaux plus récents, synthétisés par Varela, Thompson et Rosch (1993), amènent à penser que les catégories de couleurs sont engendrées d'une part par des réponses neuronales et des opérations cognitives propres à l'espèce, et donc universelles, et d'autre part par des opérations cognitives variables selon les cultures.

généralement, l'importance de la dimension historique pour l'étude du développement est soulignée avec force par les chercheurs du courant *life span* (McCluskey & Reese, 1984; Sørensen, Weinert & Sherrod, 1986).

Le deuxième secteur où l'immutabilité des lois psychologiques a été contestée est la psychologie sociale. Dans un article théorique, Gergen (1973) est allé jusqu'à présenter la psychologie sociale comme une discipline historique. Son argumentation a été contestée. Mais il existe au moins un domaine, celui de l'influence sociale, où l'historicité d'une loi a été empiriquement prouvée. Dupliquant l'expérience de Asch, plusieurs auteurs (Flieller, en soumission; Larsen, 1974; Perrin & Spencer, 1981) ont montré que le conformisme était un phénomène dépendant du contexte historico-culturel, qui s'est considérablement réduit au cours du temps, jusqu'à pratiquement disparaître au cours de la période récente. Selon Deconchy (1989), c'est l'épuration des situations expérimentales qui maintient la fiction de lois immuables en psychologie sociale.

Le troisième secteur où le postulat d'immutabilité est mis en question est celui de la psychologie différentielle des sexes. A partir de la fin des années soixante-dix, un nombre croissant de chercheurs s'est demandé si les changements intervenus dans le statut des femmes occidentales étaient susceptibles d'avoir modifié les différences psychologiques intersexe observées dans nos sociétés (par ex. Hoffman, 1977). De fait, en comparant des recherches récentes à des recherches plus anciennes, on a constaté une homogénéisation des conduites dans plusieurs domaines (Flieller, 1991), en particulier dans celui des aptitudes cognitives (Feingold, 1988). Bien que cette réduction des différences paraisse liée aux situations expérimentales considérées (Eagly, 1987; Masters & Sanders, 1993), elle montre qu'il est fort imprudent de discourir sur des différences psychologiques intergroupes sans tenir compte ni de la position relative de ceux-ci au sein de la société (Unger & Crawford, 1992, p. 101) ni des changements historiques de cette position. Il est intéressant de noter que la question d'une modification des différences intersexe au cours du temps s'est posée en même temps que le statut des femmes changeait. Le contexte historique peut donc simultanément modifier l'objet étudié et le regard qui y est porté.

Quel que soit le domaine considéré, la méthode la plus sûre pour établir l'historicité d'un phénomène est de dupliquer une recherche antérieure. Les duplications de l'expérience de Asch évoquées plus haut en fournissent un exemple. Pour que la comparaison soit valide, il est impératif que la reproduction soit la plus fidèle possible. Ceci implique que l'échantillon soit défini de la même manière et que le matériel et les consignes soient repris sans changement. Divers problèmes méthodologiques se posent, qui, en général, sont d'autant plus aigus que la recherche originale est éloignée dans le temps. Nous mentionnerons seulement ici le problème posé par une éventuel changement de la dimension mesurée, dont l'identité formelle du matériel et des consignes ne garantit malheureusement pas l'invariance

temporelle. Par exemple, une question qui à époque donnée constitue une bonne opérationnalisation d'un concept peut changer de signification au cours du temps et n'être plus faiblement relié à la dimension que l'on cherche à mesurer. Il existe toutefois des solutions à ces problèmes. En revanche, les imperfections de la recherche originale, ou l'absence d'informations suffisantes sur les conditions de sa réalisation, constituent des obstacles majeurs et souvent rédhibitoires. C'est ainsi que nous avons dû renoncer à dupliquer certaines expériences de Binet (1899) sur la suggestibilité des enfants, faute de précisions indispensables, sur l'échantillonnage des sujets en particulier.

Ces remarques sur la méthode permettent de situer les recherches évoquées par rapport à la psychologie historique de Meyerson (1948, 1987). On sait que cet auteur a défendu, il y a longtemps et avec beaucoup de vigueur, des idées proches de celles exprimées ici. Meyerson critique «le dogmatisme de la permanence» (1948, p. 120) et invite les chercheurs à «considérer les "objets" psychologiques sous leur aspect historique» (p. 121). La mission assignée à «l'historien des faits psychiques» (p. 136) est d'étudier la genèse des diverses fonctions psychiques. Ses données sont les «oeuvres» d'une époque (langues, mythes, religions, arts, sciences, etc.), à partir desquelles il doit inférer le fonctionnement psychologique des hommes qui les ont produites. La perspective que nous proposons s'apparente, comme on le voit, à celle ouverte par Meyerson. Toutefois, deux différences importantes les séparent. La première concerne la période que se propose de considérer l'historien des faits psychiques: toute l'histoire de l'humanité dans un cas, une partie de l'histoire contemporaine seulement dans l'autre. Il est clair que le programme de Meyerson est beaucoup plus riche potentiellement. La deuxième différence entre les perspectives est méthodologique. Meyerson croit possible de retrouver le fonctionnement psychique sous l'oeuvre. Cette rétrodiction paraît cependant hasardeuse, comme tend à le montrer l'exemple de la perception des couleurs. Pour son exemplarité sans doute, Meyerson a choisi cette perception comme thème du premier colloque de psychologie historique (1954). Mais, comme nous l'avons rappelé précédemment, la perception des couleurs apparaît à présent beaucoup plus contrainte par des universaux neurophysiologiques et beaucoup moins sujette à des variations culturelles qu'on ne l'a longtemps cru. Les duplications d'expérience ou d'enquête appréhendent les variations temporelles du comportement d'une manière certes plus limitée, mais plus sûre et plus objective que la psychologie historique meyersonienne.

Mais quelle est la portée et l'utilité de l'étude des variations historiques du comportement par la méthode des duplications? Ces variations ne sont-elles pas simplement quantitatives? L'accélération du développement cognitif, la diminution des réponses conformistes dans l'expérience de Asch, la réduction des différences comportementales intersexe ne sont-elles pas autant d'exemples de variations quantitatives, qui laissent

inchangées les lois du développement, de l'influence sociale et de la différenciation des groupes?

Dans sa réplique à Gergen, Schlenker (1974) fait observer qu'une science est nécessairement anhistorique parce qu'elle cherche à subsumer la variabilité des choses sous des lois invariantes («species change, but a theory of evolution is tanshistorical», p.7). Nous partageons ce point de vue. Les variations historiques du comportement constituent des faits à expliquer par des lois générales. Mais comme l'indique Reuchlin (1990), une théorie n'est vraiment générale que dans la mesure où elle est capable de rendre compte des variations observées. Les comparaisons temporelles peuvent ainsi utilement contribuer à l'édification d'une science psychologique générale: d'une part en permettant d'éprouver l'universalité des lois et des théories et d'autre part en fournissant des observations qui permettent de reformuler les lois et les théories dans le sens d'une généralité accrue.

Ainsi la mise en évidence de l'historicité de certains comportements n'a pas pour objectif de légitimer une épistémologie relativiste. En revanche, elle assure une fonction critique nécessaire à l'égard d'une conception de la psychologie où cette dernière est assimilée aux sciences physiques ou biologiques. La psychologie humaine n'est pas une science naturelle, mais une science sociale qui, comme telle, ne peut ignorer sa dimension historique.

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TOWARDS THE STUDY OF THE HISTORICITY OF BEHAVIOR

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ABSTRACT

Psychology strives to develop universal theories but usually neglects to consider the influence of the historical context on the behavior of the subjects observed. From a theoretical point of view, historical variations in behavior can be accounted for in the same manner as cross-cultural variations. The neurophysiological unity of the human species and the distinction between processes (assumed to be general) and contents (where variability is acceptable) are insufficient for guaranteeing the universality and the immutability of the laws of behavior. Empirical research is needed to verify their stability over time. Examples of historical variations have been drawn from three fields: developmental psychology, social psychology, and differential psychology. Evidence of such variations is obtained by duplicating experiments or surveys dating back several decades. The methodology of this form of study of the historicity of behavior seems to permit surer conclusions than does Meyerson's historical psychology. The nature and theoretical utility of the studies considered is discussed briefly. Although the crucial role played by these studies is obvious, they are not based on a relativist epistemology. Their objective is to contribute to the construction of theories which, being more tuned to the historical dimension of their object of study, gain some generality.

TOWARDS THE STUDY OF THE HISTORICITY OF BEHAVIOR

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An important contribution of the history of psychology to the internal epistemology of the discipline is the evidence it offers of the historicity of psychological theories, which are rooted in an era and bear its mark. There have been a number of attempts, for example, to demonstrate the role of the social and historical context in the elaboration of psychoanalysis, behaviorism, the theory of intellectual aptitudes, and Piaget's theory of cognitive development. The light such studies have shed on the influence of the social environment prompts us to take a more critical look at the theories in question, and shatters all illusions of a purely internalist approach to the discipline (Mengal, 1988). Nevertheless, in order to avoid the shortcomings of a relativist position (Matalon, 1986) leading to aporias (Rée, 1992), the history of psychology must take both internal and external factors into account (Apfelbaum, 1988; Watson; 1977).

But there exists a second kind of historicity of psychology, which will be the focus of this paper. The behavior studied by psychology is the behavior of a human being living in a specific historical era (a phenomenon stressed by Parot, 1992). If, as we have reason to believe, behavior is indeed partially determined by historical context, then psychological laws are partly history-dependent. The historicity of psychology thus stems from time-related variations in its object of study and in the way in which that object is conceptualized by psychologists. While this viewpoint has been expressed repeatedly by a certain number of authors, it remains the position of a small minority. Most psychologists are convinced of the timeless nature of their observations and their laws. Thus, the historical variability of behavior must not only be justified theoretically but proven empirically as well.

To varying depths and breadths, time changes the culture of a human group. Depending on the group and the amount of time separating the periods being compared, these changes either constitute variations within a given culture or much larger changes which can be classified as cross-cultural differences. Thus, the study of historical variations and the study of cultural variations have the same theoretical foundations. The same reasons why a behavior varies in space can also be used to explain how it varies across time. The most important among these reasons is no doubt the fact that some frameworks and principles used to organize thought and behavior differ across cultures.

Two objections deserve consideration here. The first consists of pointing out that the neurophysiological unity of the human species lends credibility to the universality of certain basic mental operations. It has been shown (Rosch, 1973), for example, that color recognition is not subject to cross-cultural variation, whereas the decomposition of the light spectrum into colors varies considerably by era and culture[1]. But the nervous system only sets the limits of these historical and cultural variations, and the psychological invariants can only be identified by making comparisons across space and time. The second objection concerns the distinction between contents and processes -- while the former are assumed to vary with time and culture, the latter are thought to be universal. Psychology's task, then, would be to formulate theories that do no take historical and cultural context and content into account (Schlenker, 1974). But many authors have questioned the separability of contents and processes. Moreover, disregarding contents may hinder the comprehension of the processes. Take the case of cognitive processes, for example. Some cognitivists -- and renowned ones at that (e.g. Neisser, 1976; Morton, 1981) -- believe that the withdrawal of content makes psychology sterile, while social psychologists feel that the limitations of cognitivist models "lead us to reflect upon the social and historical nature of cognition" (Jodelet, 1989). One can also wonder, along with Petitone and Triandis (1987), whether the apparent universality of certain theories (cognitive dissonance theory, for instance) is not illusory, for it relies on intrapsychic mechanisms whose apparent generality may be based solely on arguable cultural assumptions.

The little attention paid to variations in behavior over time can probably be partially accounted for by the fact that psychology has not had enough time to discover those variations. It is not surprising that laws established no more than a century ago seem eternal. However, some doubt about the immutability of those laws has appeared in three areas of psychology.

The first area is developmental psychology. Schaie's work inspired a number of studies comparing the development of different cohorts. Schaie showed (Schaie & Strother, 1968) that the alleged lowering of cognitive performance starting at the age of twenty-five was due to cohort differences, a finding which was confirmed by many subsequent studies. Cross-cohort comparisons of children appear to indicate an acceleration in cognitive development within the past few decades (Flieller, 1989), which is anologous to the accelerated patterns observed for physical growth (increase in mean height, and early

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[1] Some more recent studies reviewed by Varela, Thompson and Rosch (1993) lead us to believe that the color categories are generated (i) by neural responses and cognitive operations which are species-specific and thus universal, and (ii) by cognitive operations which vary across cultures.

puberty). At a more general level, the importance of the historical dimension in the study of development has been highly stressed in research on life span (McCluskey & Reese, 1984; Sørensen, Weinert & Sherrod, 1986).

The second area in which the immutability of psychological laws has been contested is social psychology. In a theoretical article, Gergen (1973) went so far as to present social psychology as a historical discipline. His argumentation was challenged. But there is at least one domain, that of social influence, where the historicity of one law has been proven empirically. Duplicating Asch's experiment, several authors (Flieller, submitted; Larsen, 1974; Perrin & Spencer, 1981) have shown that conformity is a historical and cultural context-dependent phenomenon which has diminished considerably with time, to the point of practically disappearing in recent years. According to Deconchy (1989), it is the oversimplification of experimental situations which has maintained the illusion of immutable laws in social psychology.

The third area where the postulate of immutability has been questioned is the psychology of sex differences. Near the end of the seventies, an increasing number of researchers began to wonder whether the status changes undergone by western women had modified the psychological differences observed between the sexes in our societies (e.g. Hoffman, 1977). In comparisons of recent research to some older studies, behavior in several domains was in fact found to be more homogeneous (Flieller, 1991), particularly in the area of cognitive capacities (Feingold, 1988). Although this reduction in differences appears to be linked to the experimental situation used (Eagly, 1987; Masters & Sanders, 1993), it indicates that it is indeed risky to talk about psychological differences between groups without considering both the position of those groups within the society (Unger & Crawford, 1992, p. 101) and any historical changes that position might have undergone. It is interesting to note that the issue of potential changes in gender differences over time was raised at the same time as the status of women was evolving. It is clear then that historical context can simultaneously modify the object that is studied and the way it is viewed.

No matter what dimension is being considered, the surest method for establishing the historicity of a phenomenon is to duplicate an earlier study. The replications of Asch's experiment mentioned above illustrate this well. In order for a comparison to be valid, the reproduction must be as accurate as possible. This means that the sample must be defined in the same manner, and exactly the same materials and instructions must be used. This poses various methodological problems which generally increase in acuteness as the time lapse between the studies increases. I will simply mention the problem posed by a change in the dimension being measured, whose constancy over time cannot be ensured by identical materials and instructions. For instance, a question which at one time afforded a good operationalization of a concept may change meanings with time, becoming only weakly

related to the dimension one is attempting to measure. However, there are solutions to these problems. On the other hand, imperfections in the original study, or insufficient information about the conditions under which it was run, are major, often crippling obstacles. For example, due to missing information (particularly concerning the subject sample), we were forced to abandon the idea of duplicating some of Binet's (1899) experiments on the suggestibility of children.

These remarks on experimental methods allow us to situate the studies in question with respect to Meyerson's (1948, 1987) historical psychology. We know that very many years ago, Meyerson fervently defended a certain number of ideas which are similar to those expressed here. He criticized "the dogmatism of permanence" (1948, p.120) and encouraged researchers to "consider the historical aspect of psychological 'objects'" (p. 121). The assignment given to the "historian of psychic facts" (p.136) was to study the genesis of the various mental functions. The historian's data are the "works" of an era (languages, myths, religions, arts, sciences, etc.), from which the psychological functioning of the humans who produced them must be inferred. As you can see, the perspective that I am proposing can obviously be likened to the one opened up by Meyerson. Nevertheless, two important differences distinguish them. The first concerns the period considered by the historian of behavior: the entire history of humanity in one case, and only part of its contemporary history in the other. It is clear that Meyerson's project is potentially much richer. The second difference between the perspectives is a methodological one. Meyerson thinks it is possible to find mental functioning behind the works. This retrodiction appears hazardous, however, as suggested by the above illustration of color perception. Most likely for its exemplarity, Meyerson chose this type of perception as the theme of the first conference on historical psychology (1954). But, as mentioned above, color perception now appears to be much more highly constrained by neurophysiological universals, and much less subject to cultural variations than it was long believed. Granted, duplication of experiments and surveys may be a much more limited way of assessing temporal variations in behavior, but it is surer and more objective than Meyerson's historical psychology.

But what is the scope and utility of the study of historical variations in behavior using the replication approach? Aren't these variations simply quantitative? The acceleration of cognitive development, the decline in conformist responses in Asch's experiment, the reduction of gender differences in behavior -- aren't all of these examples of quantitative variations which do not change the laws of development, social influence, or group differentiation?

In his reply to Gergen, Schlenker (1974) pointed out that any science is necessarily ahistorical because it seeks to subsume the variability of things under invariable laws

("Species change, but a theory of evolution is transhistorical", p.7). I share this point of view. Historical variations in behavior are facts to be explained by general laws. But as Reuchlin (1990) indicated, a theory is only truly general insofar as it is capable of accounting for observed variations. Comparisons across time can indeed make a useful contribution to the edification of a general psychological science, because they allow us to test the universality of the laws and theories, and they supply observations which can be used to reformulate the laws and theories in order to grant them greater generality.

Thus, the search for evidence of the historicity of certain behaviors is not aimed at legitimatizing relativist epistemology. Instead, it performs a necessary and critical function relative to a view that psychology is treated as a physical or biological science. Human psychology is not a natural science but a social science which, as such, cannot ignore its historical dimension.

A FLECKIAN APPROACH TO THE HISTORY OF THE MENTAL MEASUREMENT MOVEMENT.

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ABSTRACT:

In this paper the historiographic model of Ludwik Fleck will be applied to the development of a new scientific fact in the mental measurement movement of the 1920's in America: the idea that defective intelligence was the cause of anti-social conduct.

The advantage of Fleck's historiographic model is that it avoides the pittfals of a strict social-constructivist epistemological view on the development of science by introducing a realist element: the proto idea. According to Fleck it is not fruitful to denote old scientific insights as pseudo-scientific fairy tales. On the contrary these insights play a mayor role in the development of new scientific facts. They determine the direction of new research of a given scientific thought collective.

Because of this Fleck's historiographic model was way ahead of his time. Only recently studies appear in wich a social-constructivist *cum* realist option is defended. A good example is John D. Greenwood (1994) who states that a social construction always refers to an existing social reality.

Fleck's epistemology starts from three assumptions:

1. Knowledge may not be considered apart from the people- who under specific social/historic and culturele preconditions- aquire, produce and use this knowledge.

2. When a thought collective explanes one of its ideas to another thought-collective, this idea undergoes a transformation. Circulation of ideas neccesary implies their transformation.

3. Certain historical developments of particular knowledge are not the product of logical developments of specific thoughts/insights. they're also not the product of the increase of specific information. They are the product of the interplay between certain thought-collectives and their thought styles.

These startingpoints show that scientific facts are constructed. A construction based upon proto-ideas wich have their origine in a specific thought style of a specific thought collective at a certain time. Because these thought collectives belong to a given culture, scientific progress is always culture bound. Therefore a culture-theoretical analysis of science is an important condition for a historical insight in the development of a scientific fact.

To demonstrate the usefullness of Flecks historiographic model for

the history of psychology is chosen a particular development in American differential psychology: the development of the idea that intelligence and feblemindedness were causaly related in a hereditarian model of mind.

This idea was rather succesfully propagated by the American psychologist Henry H. Goddard who in his book: 'The kallikak Family' (1912) stated that modern psychology could protect society against the dangers of feeblemindedness. Intelligence tests could be used to select feebleminded persons who subsequently could be segregated from the rest of society.

Working as a research psychologist at the 'Vineland Training School For The Feebleminded' Goddard didn't belong to the mainstream psychology of his day. Because of his active membership of 'The American association for the Study of the Feebleminded' he came into contact with psychiatrists and other medically trained professionals who introduced him to a pathological approach to feeblemindedness.

Having aquainted himself with the French Simon-Binet intelligence test with ideas from recent scientific developments such as the new biology, biometrics and the new psychology, Goddard became convinced that it was possible to find a solution to the problems concerning 'psycho-asthenics'. The findings of the application of his version of the Simon-Binet test to the pupils from the Vineland Trainingschool for the Feebleminded, combined with the new insights of biometrics and the new biology, led to his publication in 1910 in wich he presented his classfication system for the feebleminded.

In this paper it is shown that Goddard's idea of the causal relatedness of intellingence and feeblemindedness was the result of a rather specific dynamics in the psychological science of that time. The main thrust of the dynamics can be relegated to specific interactions between the several different thought collectives to wich Goddard and his adherents belonged. Another important factor was the incorporation of transformed ideas from other thought collectives in his own theory. It will also be shown that other thought collectives in which other cultural approaches were tried, did not enter sufficiently into the fact-construction dynamics.

With a Fleckian approach to the development of a new scientific fact it is possible to supercede externalistic approaches such as used by Hamilton Cravens in his book:' The triumph of Evolution' (1988).

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On the disciplining of social psychology: A mutualist approach to the history of boundary relations between social psychology and sociology

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Introduction and Summary

This paper is in part a sequel to the one I presented at last year's Poznan conference. In that paper I presented an outline of a mutualist approach to the objects of historical knowledge, emphasising the mutuality of the historian and the object known. In this paper I apply this approach to the history of boundary relations between social psychology and sociology. I begin by noting the importance of work on boundary relations in recent philosophy of science. I then consider the singular importance of social psychology as a discipline for the analysis of boundary relations. In order to help resolve difficulties about the disciplinary status of social psychology I then briefly consider the question of how scientific disciplines are to be distinguished. The main body of the paper is concerned with supporting my view that the boundaries between social psychology and its parent discipline(s) have to be seen as shifting: the preoccupations of social psychologists reflecting developments within these parent

disciplines and in the relations between these parent disciplines and the society in which they are embedded.

The nature and functions of disciplinary boundaries

In recent discussions in philosophy and social studies of science, 'boundary work' has been recognised as playing an important part in the maintenance, development and reproduction of disciplines (Gieryn, 1993; Löwy, 1992). Although the initial concern in much of this literature was a Popperian one, about the demarcation of science from non-science, boundary work can also be seen to be important for an understanding of differentiation within disciplines, where matters of the monopolization of professional authority and the protection of autonomy are involved. Thus we need to examine intradisciplinary relations as well as the relationships between a discipline and the wider community. The rhetorical dimension of disciplinary boundaries has also been examined recently (Fuller, 1994; Klein, 1991; Good & Roberts, 1993). A focus on the rhetorical dimension of boundary work draws attention to how scientific knowledge represents the world and away from the fact that it also interferes in the world (Fuller, 1991).

Disciplining social psychology

With such considerations in mind, how may the disciplining of social psychology be seen? The links between social psychology and sociology represent a particularly rich site for a case study of boundary relations in science. In the division of labour that currently is to be found in the human sciences, a potentially central place is occupied by the academic field of social psychology. An influential contemporary definition of social psychology sees its task as the 'integration of the psychological functioning of

individuals with the social settings, small and large, in which this functioning takes place' (Tajfel & Fraser, 1978). Insofar as the disciplines of psychology and sociology provide repositories of knowledge about psychological functioning and about social settings, respectively, then the task of social psychology would seem to require at least two sets of disciplinary resources. Indeed, this might suggest that social psychology is an inter-disciplinary field. Yet the name of the discipline - social psychology (the noun *psychology* qualified by the adjective *social*) suggests that the field is a branch of psychology. Although references can be found to the term *psychological sociology*, these are relatively rare in Anglo-American contexts. Nonetheless, some sociologists also practice social psychology. In the United States, social psychology has long been (and continues to be) one of the most important areas of specialisation within sociology. Social psychology is probably unique, then, among scientific disciplines in having developed independently in two disciplines - those of psychology and sociology. Although what may appear to be involved is a simple two-way set of relationships - between a branch of psychology and the discipline of sociology, the situation is thus far more complicated. But what is a discipline?

On the nature of disciplines

On the basis of what criteria can it be determined what demarcates social psychology from other branches of psychology or indeed sociology. The systematic study of disciplinarity is still in its infancy (Shumway & Messer-Davidow, 1991). A wide variety of criteria for disciplines have been suggested, ranging from the objects studied, the cognitive activities involved, and social and institutional organisation (Bechtel, 1986). While such criteria capture many of the features of disciplines, I argue that they fail to do justice to the temporal or historical aspects of disciplines and of the

ways in which the knowledge produced by disciplines can be seen as embodied in disciplinary practices. I shall adopt the suggestion of David Hull (following Ghiselin) that it is profitable to consider disciplines as historical *individuals*: members of the disciplines of social psychology share common pasts, rather than sets of defining characteristics, though they may have to be described in terms of characteristics in order to be defined ostensively (Hull, 1988). Over time, disciplines (like species) interact with their environments and with each other. They may grow or decline, have offspring and engage in interbreeding.

Writing the history of boundary relations between the social psychologies and sociology

It should be clear from the above that an adequate history of social psychology must also, in part, be a history of sociology and of psychology. Until recently histories of social psychology have been written by either sociologists (Karpf, 1932) or psychologists (G. Allport, 1958; Jones, 1985) and have reflected their own disciplinary preoccupations. In these disciplinary-bound histories, the boundaries of social psychology are drawn in such a way as to exclude or marginalise work in the other disciplinary field of social psychology. A recent text by two psychologists and a historian has gone some way to redress this imbalance (Collier, Minton & Reynolds, 1991).

In the remainder of this paper I provide examples drawn from the past five decades of social psychology of how the location of the boundaries of social psychology is dependent upon a number of factors. I shall first demonstrate the influence of the disciplinary affiliation of the social psychologist and of developments

within the parent discipline. Thus it will be shown how sociological social psychologists have at various times emphasised both the micro and the macro aspects of social life, depending on developments within sociology (e.g. to rectify 'oversocialised' conceptions of the person, or the neglect of social structure, respectively). Psychological social psychologists, likewise, can be seen to have variously emphasised 'situational' factors in their studies of conformity and social influence, or the neglect of the 'social' and the 'cultural' dimensions of human life. Further examples of the influence of prevailing (parental) disciplinary pressures will be provided in terms of the same social psychologists proselytising at different times in the development of their disciplines on behalf of quite diverse (and sometimes conflicting) developments. The paper ends with some reflexive considerations concerning the rhetorical features of this social psychological analysis of boundary relations by a social psychologist committed to an interdisciplinary view of the enterprise of social psychology.

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Eric Haas

Keep the customer satisfied ...
The psychological report and its functions: the case of the Dutch Foundation for Psychotechnics.
Paper presented at the 13th Annual Cheiron-Conference, Paris, 7-11 September 1994.

Summary

In this paper I discussed some functions of the psychological report. It can be concluded that the extensive psychological report was a means to inform the client of the results of the psychological investigation. It met the demands of the customer for understandable information about the candidate. Another important function of the report was to legitimize the final advice of the psychologist, because it conveyed the backgrounds and motivation of the psychologist's advice of the candidate's suitability.

The extensive report constructed an image of the candidate's personality and herewith his suitability for the job. In this way a framework was created for the customer to compare the professional psychological views with his own lay views. Thereby it created a sense of recognition and familiarity yielding feelings of confidence and trustfulness with the psychological investigation. The report itself added new information to the pre-scientific knowledge of the client. The message was transferred that eventually, with the help of the report, the customer was able to to come to a more secure and responsible judgement and to decide on more solid grounds to engage or refuse the candidate.

In giving the customer the feeling of obtaining value for money, the report functioned as a strategic device to maintain the relationship between the psychologist and his customer. In this sense the psychological report kept both the customer and the psychologist satisfied.

Eric Haas[1]

Keep the customer satisfied ...

The psychological report and its functions: the case of the Dutch Foundation for Psychotechnics.

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Introduction

Historically, psychological testing was one of the important tasks of psychologists. They used tests to investigate the suitability of candidates for certain jobs. After testing, the results were presented to the client, in most cases in the form of a report.

In this paper I want to illuminate some of the functions the psychological report had. This will be done by the case of the Dutch Foundation for Psychotechnics, one of the leading external psychological bureaus in the Netherlands. Its reports were an example for many psychologists in our country. First, I give information about the testing method of the Foundation and the growing interest in diagnostics in general in the Netherlands which eventually influenced the outlook of the psychological report. Secondly, I will to focus on the aspect of 'reality-construction' established by the report. Thirdly, I take a brief look on the legitimating force of the psychological report.

The psychological report

Both resident psychologists (internal psychological departments) and non-resident psychologists (external psychological bureaus) constructed the report differently. Variations can be found in form as well as in contents and extensivity. Sometimes the reports only consisted of the testresults in the form of profiles or simple enumerations. Other reports contained more elaborate presentations of the findings. Mr. D.J. van Lennep, the founder and one of the directors of the Dutch Foundation for Psychotechnics in Utrecht, was the first protagonist of the so-called extensive report.

The form and the contents of the reports of the Foundation strongly depended on the point of view about psychological testing of Van Lennep (Van Lennep, 1949, a; b). According to the so-called phenomenological-personalistic approach (Dehue, 1990, p. 83-91) Van Lennep based himself on the principle that the examination should embrace the whole personality of the candidate. A human being wasn't seen as a bundle of independent capacities (elementarism), but was conceived as a 'Gestalt'. Not only a capacity such as intelligence, but also temperament, motivation and character should be investigated. For this purpose a wide variety of methods was used, for example traditional intelligence-tests, projective techniques (the Four Picture Test developed) by Van Lennep), graphology and physiognomy and even astrology (Van Lennep, 1935, p. 48).

Van Lennep's investigations contained three methods (Van Lennep, 1949 b; c). The first was the testing of intelligence, the reductionistic method traditionally used by psychologists.

The second was the observation of the candidate performing certain tasks. The psychologist tried to emphatize with the candidate creating a picture of the testee in relation to the tasks and functions. In the third, the candidate no longer was approached as an object but was 'encountered' equally as a subjective being in order to get to know him. Psychological tests were only a means for activating the candidate to get acqainted with the candidate, his motives, temperament and worldview. In this way a deeper understanding into his personality was gained (Van Lennep, 1949c, p. 29). The actual results were less important than the exact way in which the candidate tried to solve the questions. In Van Lennep's intuitive approach not the test but the psychologist himself was the instrument to infer the candidate's suitability in relation to the future job.

After the examination the test-results together with the impressions of the different test-leaders were collected. After discussion a common conclusion was drawn. To construct a total image of the candidate the psychological reporter combined the test-results and the personal impressions in his report. He should be fully aware of the meaning of every single datum in relation with the demands of the new function the candidate was tested for. To understand the psychological structure of the candidate the reporter used his ability of 'psychological imagination' in creating his image of the testee. In the so-called 'freely' descriptive report, introduced by Van Lennep, the psychologist was able to paint a more personal impression of the candidate which he called a 'painting in words' (Van Lennep, 1949c, p. 4; Van Strien, 1964, p. 77).

The report of the Foundation should never be a mere enumeration of test-results. Instead, a coherent meaningful image of the candidate had to be conveyed to the client.

In the 1930's and approximately the first ten years after the Second World War Dutch psychologists in general symphathized with a more interpretative, impressionistic approach of selecting personnel. A shift took place from the 'measuring' psychotechnical (apparatus) and intelligence testing towards psychodiagnostics (Van Drunen, 1993, p. 254-256; Van Strien, 1993, p. 77-80). Psychodiagnostics was a more comprehensive way of psychological investigation in which different testing methods were combined (intelligence, personality, assignments under observation, etc) to gain deeper insight into the psychological equipment of the candidate as an entire person. As a consequence of this shift the presentation of psychological profiles or enumerations as a report was no longer considered sufficient to describe the testee.

The reports of the Dutch Foundation for Psychotechnics served as models for many other psychologists in the Netherlands. They were attracted to the new form of psychological reportation and began to follow the example set by Van Lennep (Cf. Van Strien, 1964, p. 75; 1966, p. 40).[2]

The report as 'reality-construction'

The main function of the psychological report was to inform the client about the results of the psychological investigation of the candidate. With the growing interest in psychodia-

gnostics, the extensive report became "a medium to transfer a more personal impression" of the testee to the client (Van Strien, 1964, p. 77).[3] Especially for the non-resident psychologist the report was an important device to inform his customer about the suitability of the candidate.

Moreover, the report prestructured the client's outlook on the candidate with respect to the future perspectives of the candidate in the organization. The vivid and eloquent description of the candidate's character molded the discussion about the candidate into a distinct framework. The importance of the report layed in the newly given information, filling in gaps in the previously existing ideas of the client about the candidate (Van Lennep, 1948c, p. 5). Generally, the psychological report acknowledged the pre-existing impressions which were arranged into a wider framework in relation the demands of the future job. It was important that the client could recognize his own ideas of the candidate, yielding feelings of familiarity and confidentiality with the psychological data presented (Van Strien, 1964, p. 163). Thus both the 'pre-scientific' view of the client as well as the professional psychological report led to a more coherent image of the candidate (Van Lennep, 1949c, p. 4). The fluent and distinct description contributed to the client's recognition of the candidate and gave the impression of plausibility or rightness to the report. Hence, the client could more adequately decide on the candidate's suitability.

The psychological report as a legitimizing means

External agencies used their psychological reports as a strategic device to show that psychology and the psychologist had been there. By means of the report the psychological investigation was legitimized to the client. It presented an account of the results of the psychological investigation in common language. As a legitimatizing force it was a justification for the advice about the candidate's suitability. To answer complicated questions about the candidate for present and future situations, for example reviewing staff and promotions (Blaauw, 1951, p. 7) the extensive report was best suited to meet the needs of the client.

In the intensive and long-established contacts with many of its clients the Foundation had become aware of their needs for adequate information. By means of the extensive report the customer was able to decide on more solid grounds. He could compare his own views about the candidate's suitability with the professional conclusions of the psychologist. The report gave the client the feeling of obtaining value for money. Besides, it created goodwill for the non-resident psychologist and for the prolongation of the relationship between psychologist and customer.

Conclusion

It can be concluded from this paper that the extensive psychological report was a means to inform the client of the results of the psychological investigation. It met the demands of the customer for understandable information about the candidate. Another important function of the report was to legitimize the

final advice of the psychologist, because it conveyed the backgrounds and motivation of the psychologist's advice of the candidate's suitability.

The extensive report constructed an image of the candidate's personality and herewith his suitability for the job. In this way a framework was created for the customer to compare the professional psychological views with his own lay views. Thereby it created a sense of recognition and familiarity yielding feelings of confidence and trustfulness with the psychological investigation. The report itself added new information to the pre-scientific knowledge of the client. The message was transferred that eventually, with the help of the report, the customer was able to to come to a more secure and responsible judgement and to decide on more solid grounds to engage or refuse the candidate.

In giving the customer the feeling of obtaining value for money, it functioned as a strategic device to maintain the relationship between the psychologist and his customer. In this sense the psychological report kept both the customer and the psychologist satisfied.

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Notes

1. Eric Haas is working in the History and Arts Department of the Erasmus University in Rotterdam. He is preparing a dissertation about the development of the psychological practice in the Netherlands.
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The author would like to thank Mr. Stephan Pas for his patient help in preparing the English version.

2. Towards the end of the 1950's, together with a more business-like approach in trade and industry and the increasing scientific and social critique on the subjective approach the interest for the intuitive, impressionistic methods declined. Even Van Lennep himself eventually admitted the subjective pitfalls of his methods (Van Lennep, 1957, p. 275-276; 1962, p. 16). Influenced by the social-psychological concept of role he proposed a shift towards the analysis of the functional roles, objective norms of job-functions which were considered more independent of the particularities of personality of the candidate.

3. Contrary to the reports of non-resident psychologists the reports of the residents kept their condensed form. The results were still presented in the form of simple profiles or enumerations. This is due to the function of these internal reports as research documentation instead of being an infor-

mational and legitimating tool towards a client. Moreover, for a resident psychologist it was easy to enlighten the interested party orally on the suitability of the candidate. For external bureaus this was more complicated, costing time and money.

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PSYCHOLOGY AS A SCIENCE

On defining a science of the mind and a social technology from Kant to Galton

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The subject matter of psychology

Before and after its emergence as a science in mid 19th C. Germany, France and Great Britain, Psychology has been haunted by the quest for defining its subject matter:

- Wundt: conscious mental processes
- Charcot/ Freud: unconscious mental processes
- Galton/Watson: behavior
- Brentano/Dilthey/Spranger: human *Erlebnisse*

The question ***why***, in Western Europe, there was no *scientific* psychology of great significance before, let us say, 1800, has been answered in various ways. One of the most commonly held viewpoints is that due to an underdeveloped methodology, psychology *could* not be a science from Aristotle to Herbart. Only when scientific experimentation, statistics, applied mathematics and brass instrumentation had sufficiently been improved during the first half of the 19th C., could psychology become a science, worthy of this name.(In parentheses it may be remarked that authors of textbooks with titles such as: "The great psychologists. From Aristotle to Piaget", have not digged enough into the ***primary*** texts and their cultural contexts, of e.g. Aristotle, Thomas, Descartes, Locke, Hume *e tutti quanti* before Kant (1786), to be able to arrive at the insight (to their own big surprise, we may assume), that how great these 'biologists', 'philosophers' and 'theologians' may have been, they were no psychologists in any decent sense of the term. History mainly advances transformationally and discontinuously and cultural and social history are no exception to this peculiar course of time's flow.

Gordana Jovancvic

(Belgrade)

FREUD AND DESCARTES: THE CARTESIANISM OF PSYCHOANALYSIS

Both Descartes and Freud have been understood as thinkers who radically changed the previous paradigm of theorizing. They both followed an inward turn - sharing a concentration on subjectivity. By this turn they determined the way their theorizing was relevant for psychology. Academic psychology has indeed adopted a Cartesian heritage. I would like to question the peculiar academic position of psychoanalysis by reflecting on its relationship to the Cartesian paradigm of subjectivity.

A new age of thinking, based on certainty, intended to overcome the dramatically experienced relativity of social and religious opinions, started with Descartes. Cartesian subjectivity was founded on the autonomous and self-evident consciousness.

Two hundred and fifty years after Descartes, under conditions of newly emerging doubts about progress in history, Freud challenged the Cartesian autonomous and self-evident subject. While accepting this interpretation, I want to try to show that, behind this rejection, Freud still thinks in a Cartesian manner. Even more, I suggest, Freud's justification of the Unconscious - and the Unconscious undermined Cartesian self-aware subject - is just an application of the Cartesian consciousness model (i. e. taking the consciousness as a starting point, as a source of criteria - see Freud: The Unconscious, 1915). I shall argue that the epistemological structure of Freud's assumption of the Unconscious repeats the Cartesian model of the foundation of consciousness.

From this provocative similarity it is possible to conclude that Freud still partly belongs to the same modern, originally Cartesian, paradigm of subjectivity. This paradigm implies the affirmation of a subjectivity principle as a constitutive principle of all human activity. It is, however, built from a mono-subject perspective which determines contents and ways of conceptualizing experience. This means that it excludes other experience - intersubjective phenomena which are the origins of subjectivity.

In this sense, even psychoanalysis - though provoking many of the propositions of academic psychology - shares with it the still prevailing conscious-centred, one-subject position.

Gordana Jovanovic
(Belgrade)

FREUD AND DESCARTES: THE CARTESIANISM OF PSYCHOANALYSIS

In the self-understanding of modernity it is still prevailing - in spite of emerging new interpretations (see S. Toulmin) - a position which in the paradigms of philosophy and science of the XVIIth century sees the final articulated break with the previous "close and completed" medieval world and a foundation of a modern age.

"The intellectual atmosphere in the latter half of the sixteenth century and the early years of the seventeenth century was a turbulent one, with radically opposed points of view widely held and persuasively argued for by their adherents. To these trends in science and religion we must add the effect of the discoveries made by travelers to the new world. The European imagination was fired with stories of new races of people with different opinions, customs, and laws." (Curley, 1978:11-12)

Descartes is one of the paradigm-makers of the XVIIth century. "Cartesius ist einer von den Menschen, die wieder mit allem von vorn angefangen haben; und mit ihm hebt die Bildung, das Denken der neueren Zeit." (Hegel, 1832-45: 120) A new age of thinking, based on certainty, intended to overcome the dramatically experienced relativity of social and religious opinions, started with Descartes.

The Cartesian revolution performed a turn away from previous scholastic *auctoritas* to a new starting point - thinking, in a form of methodically regulated capacities of the good sense. This turn was articulated through methodical rules which promoted a subjective criteria of cognition. Starting with a radical doubt -

De omnibus dubitandum est - and obeying to initially by mathematics inspired but then generalized rules Descartes reached, in his opinion, the certainty of cognition. As "all our clear and distinct ideas are true", from the certainty Descartes derived the truth and subsequently - since: *cogitans sum*, an universe of *res cogitans*, *res extensa* and *Deus*.

I agree with the interpretative thesis "that self-consciousness is the fundamental and unifying motif of Cartesian philosophy." (Caton, 1973:3) A new foundation of philosophy and knowledge was performed in a narrative genre of the first person, which Lyotard understands as a "sign of modernity" (Lyotard, 1986). Descartes started from his own *"J"* and followed a strict introspective dynamics of his own thoughts, proposing his life-history of certain and true knowledge as a new philosophy based on true principles. "The consciousness that reason is my reason is the opening shot of the Enlightenment." (Caton, 1973:32)

The Cartesian *"J"* is a self-aware agent having a complete knowledge of mental processes going on in it. "I can affirm with certainty that there is nothing in me, of which I am not in any way conscious." (Descartes, First Replies to Meditations). *In interiore homine habitat veritas.* *Cogitationes* of the *"J"* constituted an agent with the epistemological status of subject.

The Cartesian paradigm of subjectivity founded in the autonomy and self-evidence of consciousness has become a milestone of modern epoch.

The institutionalization of psychological knowledge belongs to the achievements of modern age. On the other side, a widely spread psychologization of life distinguishes a modern attitude.

The history of psychology as an institutionalized science started with the Cartesian paradigm: methodically disciplined introspection was an exclusive source of psychological knowledge. "Mit der Fassung der Seele nur als Bewußtsein hat Descartes den modernen Begriff der Psychologie geformt, womit er wider einen Wendepunkt geschaffen hat." (Hirschberger, 1976:109)

There is also another line of Descartes' teaching inherited by psychology: it concerns a mechanization claim. While Descartes reserved it for corporeal realm, many psychological schools generalized it to whole psychic life. This part of Cartesian heritage is not of direct relevance for my purpose here. I shall follow vicissitudes of the Cartesian subjectivity heritage.

One of the most threatening attacks on the assumption which identifies "mental" and "conscious" has been carried out by psychoanalysis. The most influential reception of psychoanalysis understands it as a conception provoking established common sense opinions and their scientific legitimation. They both presupposed a self-transparent agent in human activities.

> "...according to the official theory, a person has direct knowledge of the best imaginable kind of the workings of his own mind. Mental states and processes are (or are normally) conscious states and processes, and the consciousness which irradiates them can engender no illusions and leaves the door open for no doubts. A person's present thinkings, feelings and willings, his perceivings, rememberings and imaginings are intrinsically 'phosphorescent'."(Ryle, 1949:15)

Though the idea of the unconscious processes was already known in Freud's environment (Hartmann, Lipps, Nietzsche) - "a general idea of the unconscious was a part of European *Zeitgeist*

about 1880" (Shakow & Rapaport, 1964:101), psychoanalysis was a first psychology conceived of as a psychology of unconsciousness. Thus psychology, in Freud's opinion, just articulated an ontological fact of the primacy of the unconscious processes This decentration from the consciousness has meant a farewell of all the world built up on the presupposition of the self-evidence of the consciousness. This strategy has been compared with Galileo: the world is not any more as we experience it.

As Descartes carried out a turn toward the conscious thinking *"J"*, Freud carried out a decentration from this conscious *"J"* toward its unconscious predecessors and partners.

I shall argue that this interpretation covers only a first evident level of relationship between Freud and Descartes. My intention is to draw attention to the steps of theoretical introduction and justification of the concept of unconscious in order to reveal a still existing and functioning Cartesian manner in Freud's thought.

It is useful to remind that the term metapsychology appeared first time 1896. in a Freud's letter to his friend Fließ: it designates "a psychology going behind consciousness". As Paul-Laurent Assoun said: metapsychology means a "birth of an original and specific epistemic reality". (Assoun, 1976) My task here is to reconstruct conceptual steps though which the unconsciousness became an "epistemic reality".

It is remarkable that Freud placed metapsychology behind metaphysics, in the sense of translation of metaphysics into metapsychology (Freud, 1901: The Psychopathology of Everyday Life). But Freud already accepted an ontological premise that

there is an unconscious psychic reality that is possible to know in some unclear endopsychic way. Results of this process are projected as "übersinnliche Realität". The task of metapsychology is to translate this projected and objectivated reality back into its psychic origin.

It is important to stress that even at this early stage metapsychology was a kind of the postponed construction of original psychic events. In the main writing devoted to this subject Freud tried to come closer to the supposed unconscious processes. Which means has Freud used in conceptualizing the unconsciousness?

Freud was very well aware of the consequences of introducing the concept of the unconscious psychic processes in our way of thinking. Contrary to accusation of empiricism Freud demonstrated a sensitivity to constructor's contribution to a constructed cognitive product. The first chapter in the writing The Unconscious (1915) has the title: Justification for the concept of the Unconscious.

Freud gives a decisive answer: "Our assumption of the unconscious is *necessary* and *legitimate*. and we possess numerous proofs of its existence." (Freud, 1915:168). In his argumentation for the necessity of the assumption of the unconscious Freud uses conscious as a starting point:

> "...because the data of consciousness have a very large number of gaps in them...All these conscious acts remain disconnected and unintelligible if we insist upon claiming that every mental act occurs in us must also necessarily be experienced by us through consciousness...A gain in meaning is a perfectly justifiable ground for going beyond the limits of direct experience." (ibid)

The aim of our influence - Freud shared a modern interest in controlling and improving life - concerns conscious processes.

"...the assumption of there being an unconscious enables us to construct a successful procedure by which we can exert an effective influence upon the course of conscious processes, this success will have given us an incontrovertible proof of the existence of what we have assumed. This being so, we must adopt the position that to require that whatever goes on in the mind must also be known to consciousness is to make an untenable claim." (ibid.)

The criterion of legitimation of the concept of unconsciousness has been also taken from the same source: the consciousness.

"The assumption of an unconscious is, moreover, a perfectly *legitimate* one , inasmuch as in postulating it we are not departing a single step from our customary and generally accepted mode of thinking."(ibid :70).

If we summarize the argumentation steps in "justification for the concept of the Unconscious" we can say: the whole argumentation is founded in the paradigm of the consciousness which gives a starting point, criteria and norms. The Unconscious has been justified by applying consciousness model. As an "epistemic reality" the Unconscious has been constructed by means of the Conscious.

"...all the categories which we employ to describe conscious mental acts, such as ideas, purposes, resolutions and so on, can be applied to them /unconscious - G.J./...Thus we shall not hesitate to treat them as objects of psychological research, and to deal with them in the most intimate connection with conscious mental acts." (ibid:169-170)

Even in conceptualizations of instincts Freud uses a vocabulary representing conscious experiences.

"Even in the unconscious, moreover, an instinct cannot be represented otherwise than by idea. If the instinct did not attach itself to an idea or manifest itself as an affective state, we could know nothing about it." (ibid:179)

There is another function of the model of conscious: while the consciousness was questioned as an factic autonomous, self-evident agent, it was replaced to a goal of therapeutic

process. The Cartesian subject became an ideal, postponed regulatory principle.

Starting from another standpoint and with other assumption Mac Inter has come to the same conclusion concerning the conceptual relationship between consciousness and unconsciousness.

"Even when Freud does not use the word, this conception of 'ideas' dominates the development of the notion of unconscious contents. Secondly, this kind of account in which ideas are almost transformed into a kind of entity lends itself to spatial metaphors." (Mac Intyre, 1958:11)

The same interpretation we can find in Ricoeur's Freud book: the Unconscious is homogeneous to Conscious.

Trying to elaborate this interpretation, I find a possibility of transferring the Ryles concept of "para-mechanical myth" in Descartes' teaching to a new realm: we could speak of a "para-conscious myth" in Freud's conceptualizations of the unconscious. This is a point where Freud comes very close to Descartes. In spite of many non-Cartesian traits of psychoanalysis and its general anti-Cartesian attitude in deconstructing a subjectivity paradigm founded in self-evidence of consciousness, there is a surprisingly similarity in their positions concerning epistemic status and functions of consciousness: consciousness is for both the ultimate frame of reference: "being conscious ...forms the point of departure for all our investigations." (Freud, 1915:174).

Surprisingly enough this aspect of psychoanalysis has not been very much discussed. I think it is not less important than those very known ones. Besides of the historical reason, this aspect has, I suppose, a heuristic value in understanding some inconsistencies of Freud's thought. This is not a matter of his personal choice - it is rather the matter of available conceptual

and theoretical tools which necessarily "perceive" some experiences and ignore other ones with which they have not built up any conceptual links.

In spite of many breaks into "new" experiences in psychoanalysis there are also still many inherited schema which just reproduce previous choices. One of this - typical modern - choices is an autonomous and isolated consciousness as a taken-for-granted starting point. I don't neglect an anthropological as well as psychological importance of the consciousness. My intention is rather to strengthen its position by pointing out at its origin and genesis - and that not only in libidinal sources, as Freud demonstrated. This would mean to abandon the mono-subject perspective and conscious-centred mind in order to reach the process of emerging and developing structures as consciousness is.

If we call into mind an inherited intellectual environment of Freud's theorizing- and Cartesianism is a part of it - we can understand why, for example, Freud conceptualized culture using almost literally a *homunculus* model. This is just a theoretical consequence of his previous individualistic conception of individuality.

"When Freud speaks of the state of the individual 'before any culture' he shows no recognition of the extent to which man owes his humanity to the life and culture in which he develops. Apart from this he would hardly be a person and have any autonomy to exercise...To think of culture as necessarily a form of restriction on the individual, as Freud does, is to suppose that he is an individual with aims and interests of his own independently of the culture to which he belongs - whereas I have suggested that the very possibility of his having aims and interests of his own depends on his membership of a society with its peculiar institutions, forms of life and norms of evaluation"

- I would join this argumentation given by Ilham Dilman (Dilman, 1983:142-43).

I don't deny that in psychoanalysis there are some conceptualizations which are derived from broader experiences, including interaction between subjects. But this position has been not developed through whole psychoanalysis.

Freud's psychoanalysis is a hybrid discourse in many sense. Within a hermeneutic approach itself, there is an introspective and inter-subjective position.

The development of academic psychology is closely connected with the introspective position: it is either an exclusive psychological starting point or a target of radical objections against the psychology as a science. Such position determined a marginalization of the outer world, specially the social world, but it also produced a neglect of the non-conscious intrapsychic processes.

From the net of the Cartesian heritage, psychological reproductions of both parts of the Cartesian heritage (subjectivism and mechanics) and psychoanalytic hybrid conceptualizations it is possible to derive important insights into the constitution of subjectivity itself and the development of forms of its conceptualizations.

In both cases these insights call for a transition from the *auto-poiesis* of *homo clausus* to the *koinos kosmos*.

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Historic Demonstrations in Visual Perception through Motion Pictures

Hermann Kalkofen (Göttingen, FRG)

Amongst the demonstrations in psychological optics, the filming of which the Institute for Scientific Films, Göttingen, set off in the seventies, are some reported and discussed by Hermann v. HELMHOLTZ, who died 8th of September 1894. As pictures in general do, suffers the motion picture, however, from shortcomings which were recognized and described early by LEONARDO da VINCI. This contributions were refered to by perceptual psychologists not too seldom, although not too precisely. Despite the deficit stated by LEONARDO are - under definite conditons - motion pictures of actual demonstrations feasible and even superior against their originals. The following filmed demonstrations will be shown: <PORTERFIELDs Windmill>, <Reversal of Relief and Motion>, <NECKERs Cube in Front of a Mirror>, <Induction of the Brown-Sensation>. The second demonstration encourages an empiricistic, the third a nativistic explanation, whereas the fourth may be conceived as plea for an ecumenical look at visual perception.

Historische Demonstrationen zur psychologischen Optik in Filmen[1]

Hermann Kalkofen, Inst.Wiss.Film, Göttingen

1. Wer 1994 Historisches zur optischen Wahrnehmung beitragen will, wird des am 8.September vor 100 Jahren verewigten Hermann von HELMHOLTZ gedenken. Nicht nur für Richard GREGORY gilt HELMHOLTZ als "greatest figure in the experimental study of vision". Daß er sein - 1966 - "still ... most important work on the subject" (03:90) seit 1856 als "Handbuch der physiologischen Optik" erscheinen ließ, könnte ein Grund dafür gewesen sein, daß das Zentennium seines Todes im Vorprogramm dieser Tagung keine Erwähnung fand. *Nemo psychologus nisi physiologus* - geht das auch umgekehrt? HELMHOLTZ war jedenfalls insofern Psychologe, als gerade er zu jenen zählte, die "die Lehre vom Sehen mehr und mehr vom physiologischen Boden gelöst und den Armen der Psychologie entgegengeführt" haben, wie HERING an VOLKMANN beklagt (09:29). Das gilt nicht nur für die Raumwahrnehmung; HELMHOLTZ hat außerdem eine von WUNDT mit Bedacht so genannte *psychologische* Theorie des Farbenkontrasts aufgestellt.

Unter den Demonstrationen zur Psychologischen Optik, mit deren Verfilmung das Göttinger Institut für den Wissenschaftlichen Film vor einiger Zeit begonnen hat, gibt es manche - wie anders! - die HELMHOLTZ in seiner Physiologischen Optik mitgeteilt und erläutert hat. Das Unterfangen, Realdemonstrationen durch *motion pictures* zu ersetzen, begegnet bald auch Grenzen, die Bildern allgemein gesetzt sind, Grenzen, die LEONARDO da VINCI erkannt und anschaulich mitgeteilt hat. Das "Handbuch der Physiologischen Optik", in dem auf LEONARDO Bezug genommen wird, ist eine Rarität geworden, doch findet sich das sogenannte LEONARDO-Paradox glücklicherweise auch in BORINGs (1942), HELMHOLTZ gewidmetem, <Sensation and Perception in the History of Experimental Psychology>. Es ist ein eigenartiger Beweis der konstitutionellen Schwäche, mit der die ganze Bild-Gattung zu kämpfen hat. BORINGs Illustration dazu mitsamt Erläuterung, Vorstellungen aber auch zu LEONARDO und seiner Zeit, die andre Psychologen sich gemacht und verbreitet

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haben, verdienen schon eine eigene Betrachtung, bevor es mit dem eigentlichen LEONARDO-Paradox und den Verfilmungsschwierigkeiten weitergeht.

2. Wenn man ein Buch des Typs <Sensation and Perception> schreibe, läßt BORING verlauten, dann lese der Autor *original sources*, die er diskutiere, aufeinander beziehe, zitiere;

> "Thus a question arises about the original sources which he cannot see; should he cite them at second hand ...? Or should he refrain from mentioning what he has not read, since second-hand citations tend, as we all know, to perpetuate error?"(01:ix)

Antwort, die BORING sich erteilt: Man braucht nicht alles selbst zu lesen, dann nämlich nicht, wenn es nicht ganz so wichtig ist; man muß das nur zu erkennen geben:

> "Less important items I have cited at second hand, labeling the citation "[n.v.]", a symbol which means "not viewed" and hence "not verified." I have been conscientious. If the first edition is unavailable to me, I note it as "n.v." and then cite, perhaps, the second explicitly. Let the critic who thinks I should have cited nothing that has not been under my nose merely imagine that these "n.v." references are not in the book. With but a little more undocumented dogmatism, they could all have been omitted" (01:ix)

BORINGs [n.v.] sollte längst Standard sein; nicht als solche deklarierte "second-hand citations tend ...to perpetuate error". Zurück zu LEONARDO; in seiner Abhandlung der Möglichkeiten der Darstellung des tiefen Raums auf der ebenen Fläche des Bildes hat er die wirklichen und die bildlichen Tiefenhinweise beschrieben. Daran erinnert anerkennend 1962 HOCHBERG: "The depth clues were not discovered by *psychologists*, nor even studied much by them". Das hätten *Künstler* getan: "In 1585, Leonardo systematically studied them"(07:25). Wie das - hatte der Künstler da nicht schon längst - im Jahre 1519 - das Zeitliche gesegnet? An dessen Weiterleben läßt HOCHBERG indessen keinen Zweifel: "In the 1580's, Leonardo da Vinci investigated the techniques by which "pictures" could be made"(07:22). Mehr noch, HOCHBERG gibt sogar an, wie LEONARDO dabei zu Werke ging und, daß er dies in seinem Malerei-Traktat wie folgt beschrieben habe:"... place a sheet of glass firmly in front of you, and trace the outline of a tree on the glass"(07:22) und so weiter. HOCHBERG nimmt sein Zitat aus WOODWORTH/SCHLOSBERGs <Experimental Psychology>. In der mir vorliegenden TABARRINI-Ausgabe des 'Trattato della Pittura' entdecke ich keinen vergleichbaren Passus und finde nichts, was sich mit Fug und Recht als solch

ein *Leonardo Window* bezeichnen ließe. Ein Rätsel. Wer allerdings den ausgezeichneten PIRENNE dazu vermocht hat, gleichfalls ein *Leonardo Window* anzugeben, läßt sich ersehen: Ernst BRÜCKE war's, HELMHOLTZens Freund, FREUDs Lehrer, der 1877 "die sogenannte Glastafel des Leonardo da Vinci, die in Gestalt einer Glastafel vorgestellte Bildfläche"(02:3), erwähnte. Von wem nun BRÜCKE das hat, ließ sich noch nicht eruieren. M.A.HAGEN jedenfalls nennt Leon Battista ALBERTI als Urheber der auch von DÜRER benutzten Glastafel-Methode und hat dem 1. Band eines von ihr edierten Werks gewissenhaft den Untertitel <Alberti's Window. The Projective Model of Pictorial Information> gegeben. Nun hatten sich weder BRÜCKE, noch HOCHBERG, noch auch PIRENNE historiographischen Standards, wie BORING sie anlegt, förmlich verpflichtet. Doch BORING? Die Illustration des § 482 in TABARRINIs Ausgabe mag nicht von LEONARDOs eigner Hand sein. Gut möglich. BORING zieht eine andere Zeichnung heran: "The drawing was made by L.B.Alberti in the publication of Leonardo's notes in 1631"(o1:283) Ein Rätsel. Der oben erwähnte L.B.ALBERTI lebte von 1404 bis 1472. Gehört das zu den "less important items"? Ende der Abschweifung.

3. Das Paradox, das BORING meinte, besteht für LEONARDO darin, daß ein von beiden Augen *gemeinsam* gesehener, nicht zu großer *opaker* Körper den von beiden Augen *verschieden* gesehenen Hintergrund *durchscheinen* läßt, und das heißt eben: paradoxerweise transparent wird (10:162). Ohne es recht zu bemerken, ist LEONARDO lange vor WHEATSTONE, wie dieser bemerkte (01:284), der Binokularparallaxe begegnet. Die beidäugige Bildverschiedenheit, wie sie - nicht recht geglückt, doch deutsch - auch heißt, wird in der Ferne nun aber so bedeutungslos, daß das Monokular der Filmkamera sich an ein Phänomen wagen kann, als dessen Entdecker meist SINSTEDEN (1860) gilt. So stehts auch bei HELMHOLTZ. Von BORING erfahren wir aber, daß es schon 1759 von PORTERFIELD angegeben worden ist, und daß auch dieser wahrscheinlich nicht der erste war. METZGER, der keinen Namen nennt, beschreibt das Phänomen [Film 1: IWF K115 PORTERFIELDs Windmühle]:

> "Die Flügel der fernen Windmühle ... drehen sich scheinbar einmal nach hinten ... , einmal nach vorn ... Das kann garnicht anders sein, weil das Netzhautbild keine Eigenschaften besitzt, durch die vorn und hinten eindeutig festgelegt sind"(11:342).

Rauminversion macht Bewegungsumkehr, wie 1925 Franz Bruno HOFMANN bemerkt hat: Der

> "... Inversion ... nach der Tiefe ... entspricht beim Bewegungssehen die Inversion der Bewegungsrichtung[, wie sie zuerst von SINSTEDEN an der Bewegung entfernter Windmühlenflügel beobachtet wurde]" (08:579).

Die Mühlenflügel im Film und die Mühle drehen sich objektiv stets im Uhrzeigersinn.

Rauminversion läßt sich, führt HELMHOLTZ aus,

> "an einer großen Zahl perspektivischer Linienzeichnungen z.B. solchen, welche regelmäßige Körper, Kristallmodelle usw. in geometrischer Projektion (also von einem unendlich entfernten Punkte aus gesehen) darstellen, beobachten. Dieselbe Ecke oder Kante kann bald einspringend, bald ausspringend erscheinen. Oft wechselt die Vorstellung unwillkürlich. Ich finde aber, daß man sie auch immer willkürlich wechseln lassen kann, wenn man lebhaft eine andere Deutung sich vorstellt"(05:240).

HELMHOLTZ ein Voluntarist.

3. Für ihn als Empiristen interessant sind all die Fälle,

> "wo ein Schwanken zwischen zwei Deutungen möglich ist. Hier finden wir, daß wir beim ersten Anblicke in eine dieser Deutungen unwillkürlich verfallen, und zwar der Regel nach wohl in diejenige, welche die größte Anzahl ähnlicher Erinnerungsbilder zurückruft, wie bei den Reliefs von menschlichen Gesichtern, wo wir der Regel nach die der Wirklichkeit entsprechende konvexe Form zu sehen glauben"(05:245).

Gemeint ist das hohle Relief, die <Matritze>; Seiten vorher wird dazu im einzelnen ausgeführt:

> "Sieht man mit beiden Augen gleichzeitig nach der Matritze hin, so schwindet in der Regel die Täuschung; ebenso wenn man den Kopf oder die Form hin- und herbewegt. Je ruhiger Auge und Objekt sind, desto leichter tritt die Täuschung ein"(05:240).

[Film 2: IWF K 134 Bewegungsumkehr und Hohlrelief (Gesicht)]. Als Vorlage dient eine langsam rotierende Maske; gewissermaßen eine <Patritze>, die auf der Rückseite <Matritze> ist. Bei dieser Demonstration ist das grundsätzliche "Monokular-Defizit" der Filmkamera nun nicht etwa ein Manko; die sich in der Bewegungsumkehr drastisch erweisende Inversion des Hohlgesichts wird gerade infolgedessen "fast unvermeidlich", wie andrerseits "die Deutung der konvexen als eine hohle Form unmöglich"(05:241) ist.

4. Dagegen muß bezweifelt werden, daß ein Empirist an der folgenden Demonstration seine Freude gehabt sollte [IWF-Film 3: K 140 NECKERscher Würfel vor dem Spiegel]; nicht bloß weil Wolfgang METZGER sie so schön gefunden hat:

> "Das Spiegelbild erscheint unwiderstehlich mitten in dem wirklichen Würfel und dreht sich nicht, wie ein rechtes Spiegelbild das muß, entgegengesetzt, sondern gleichsinnig, so daß

alle Seiten ungefähr parallel bleiben: ein schönes räumliches Beispiel von der Wirksamkeit des Gesetzes der guten Gestalt"(11:214).

Nicht jeder Nativist wird sich Gesetzen beugen, die diesen Namen, streng genommen, nicht verdienen.

5. Noch etwas zur Farbe: Als Vertreter einer <psychologischen> Kontrasttheorie nennt wenigstens WUNDT außer HELMHOLTZ auch BRÜCKE. Letzlich auf BRÜCKE 1878 (fide 13:774) zurück geht die folgende Demonstration [Film 4 IWF K Kontrast-natur der Braunempfindung I]: Ein und dasselbe Pigmentlicht erscheint je nach der Luminanz eines *stets* als Orange gesehenen Umfelds Orange oder *Braun*. Daß dies "wider besseres Wissen" geschieht spricht für die *Empfindungsnatur* wenigstens dieses Kontrasts. Von einer HELMHOLTZischen "Urteilstäuschung" kann eigentlich nicht die Rede sein. Aber dies "taking into account", das hier m.E. sinnfällig wird, ist ja doch ein Konstrukt, das ein gelernter Gestaltpsychologe, wenn ich nicht irre, bei HELMHOLTZ gelernt hat (04). Vielleicht ein "ökumenischer" Gedanke.

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"Hormones and Homosexual: Clifford Wright and Alfred Kinsey, 1935-1941"

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On July 16, 1993, an article about a scientific study linking homosexuality with a specific site on the X-chromosome appeared on the front page of the New York Times. The "gay gene," as it was dubbed, or the "gay brain" which preceeded it by a few years, were not the first scientific hypotheses of a biological cause of homosexuality, nor, I suspect, will they be the last. Similarly, some of the scientific criticism this research has received echoes a longer debate, not specifically about genetics or neurobiology, but about the relative merits of searching for the biological bases of identify and behavior and about some of the methodological limitations of such attempts. In this paper I will examine a parallel debate from the decade before World War II, analyzing some of the assumptions common in scientific studies of homosexuality in the American context. I will describe a series of articles by Clifford Wright from the late 1930s about the relationship between hormones and homosexuality, and contrast them with Alfred Kinsey's first publication on the issue, in which he forcefully rebutted the endocrinological work.

My intention is not to show that these studies are examples of "bad science," implying that they could have or should have been done better or differently. My purpose rather is to examine the ways in which these researchers constructed legitimated knowledge and categories about certain groups of people. More specifically, I will focus on the assumptions these authors made about the cause, characteristics, and "cure" of homosexuality, and perhaps most importantly, about who counted as a legitimate subject of study.

Hormones and Homosexuality: Clifford Wright and Alfred Kinsey, 1935-1941

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On July 16, 1993, an article about a scientific study linking homosexuality with a specific site on the X-chromosome appeared on the front page of the New York Times.[1] The "gay gene," as it was dubbed, or the "gay brain" which preceeded it by a few years, were not the first scientific hypotheses of a biological cause of homosexuality, nor, I suspect, will they be the last. Similarly, some of the scientific criticism this research has received echoes a longer debate, not specifically about genetics or neurobiology, but about the relative merits of searching for the biological bases of identify and behavior and about some of the methodological limitations of such attempts. In this paper I will examine a parallel debate from the decade before World War II, analyzing some of the assumptions common in scientific studies of homosexuality in the American context. I will describe a series of articles by Clifford Wright from the late 1930s about the relationship between hormones and homosexuality, and contrast them with Alfred Kinsey's first publication on the issue, in which he forcefully rebutted the endocrinological work.

My intention is not to show that these studies are examples of "bad science," implying that they could have or should have been done better or differently. My purpose rather is to examine the ways in which these researchers constructed legitimated knowledge and categories about certain groups of people. More specifically, I will focus on the assumptions these authors made about the cause, characteristics, and "cure" of homosexuality, and perhaps most importantly, about who counted as a legitimate subject of study.

Between 1935 and 1940, Clifford A. Wright published four articles about the relationship between hormonal balance and homosexuality.[2] Wright was senior attending physician at the Endocrine Clinic of Los Angeles County General Hospital and Associate Professor of Clinical Medicine (Endocrinology) at the College of Medical Evangelists.[3] I have been able to find out very little else about the man's life, his career, or his work, other than the articles he published in which he proposed his theory of the etiology of homosexuality, claiming that "true" homosexuality was a result of a reverse in the proportion of sex hormones. He further asserted that with proper

treatment, the condition could be reversed.

Wright was a clinician, not a researcher. He made no grandiose methodological, theoretical, or epistemological claims about the production of scientific knowledge: he merely reported what he found (or perhaps what he was looking for) and what he thought it meant. His ideas about homosexuality relied on certain assumptions about the relation between biological sex, psychological gender, and the direction of sexual desire which had a long history and were common to the period. Furthermore, like most empirical studies of homosexuality before Kinsey, his populations came predominantly from men (not women)[4], the majority of whom had found their way into the criminal justice system, the medical-psychiatric system -- or both.

Compatible with the position of many of his contemporaries, Wright felt that homosexuality was a vice, not a crime, and should be treated as a social and medical problem.[5] He suggested that society re-think its attitudes towards homosexuals, especially because most of them didn't hurt anyone[6], and because at least three percent of all people were homosexual.[7] Wright's theory of the etiology and treatment of homosexuals was quite simple, and remained remarkably consistent through six years of work. According to Wright, all humans are bisexyual -- i.e., they are part male and part female.[8] The use of the term "bisexual" in this context was <u>not</u> a statement about the character of sexual behavior or the direction of sexual desire in humans. Rather, it was a reflection of contemporary theories about the biological processes of sex determination and sex differentiation in embryological development.

By the late 19th-century it was well known that the fetal tissue which would develop into either male or female sexual organs remained undifferentiated until about the seventh week of gestation. "Bisexuality," then, referred to the bi-potentiality of development -- to the common, sexually neutral (or hermaphroditic) origins of human organisms.[9] Some 19th-century theorists linked homosexuality to a disturbance in somatic development, and even Havelock Ellis, the famed English sexologist of the early 20th century, supported the possibility that homosexuality was linked to some abnormality in the process of sexual differentiation, probably having "a

fundamental source...in the stimulating and inhibiting play of the internal secretions."[10] When Ernst Laqueur announced his discovery, in 1928, that both men and women have both male and female hormones, the "bisexual" debate was recast in endocrinological terms. Now, the role of hormones in sex differentiation, in sex differences in personality, and in sexual behavior would seem to hold the key to understanding the relation between the sexes.[11]

For Wright, hormones were central to the sexual impulse, and were implicitly complementary and opposite. He believed that the fact that men have more male hormones and women have more female hormones explained heterosexual attraction:

> The sex urge, one of the strongest influences in life...undoubtedly depends largely, if not entirely, on the sex hormones and hormonal attraction. The usual sex attraction between the normal male and the normal female is undoubtedly caused by the predominance of the male hormone in the former and the female hormone in the latter. [12]

Since, according to Wright, heterosexuality was the result of a particular hormonal balance, homosexuality was a result of an abnormal ratio of sex hormones. In homosexuals, therefore, the endocrinological proportion would have been reversed -- a male homosexual, for example, would have had more female hormones than male hormones. It was not necessary for the absolute amount of androgens to be less than normal, just for the proportion or balance to be reversed.[13] Furthermore, because of his inverted hormonal balance, the homosexual's direction of sexual desire was as "natural" as that of any heterosexual:

> [T]he sex attraction of the true congenital homosexual is based on an endocrine imbalance and is as logical in regard to his hormone balance or physiology as is the normal sex attraction.[14]

The "naturalness" of the condition, however, was not viewed as reason to ignore it: Wright posited treatment for homosexuals with a combination of a gonadotropic factor (a substance that stimulates whatever sex glands are present), usually in conjunction with a male or female hormone corresponding to the anatomical sex of the patient.[15] The idea behind Wright's treatment was to stimulate the production of testosterone in the body of a male homosexual, thus bringing the body's hormonal balance back into line.[16]

Now we come to the question of how exactly Wright defined homosexuality,

which, unfortunately, he didn't do. He did, however, make two important, related, and common assumptions: first, he distinguished "true" (or "congenital") homosexuality from the "acquired" or "latent" type. These distinctions originated in late 19th-century medical literature, and were discussed (and dismissed) by both Freud and Ellis in the first decades of the 20th century.[17] In brief, the "true," "innate," or "congenital" homosexual had always and only been attracted to members of the same sex, and the "acquired" or "latent" type develped or exhibited his or her sexual tastes later in life.

Wright's second important premise was the centrality of feminine qualities and characteristics to the homosexual status of his male subjects.[18] Emphasizing the feminine nature of male homosexuals was certainly nothing new, nor was it uncommon in 1930s medical circles which evoked and re-inscribed 19th-century ideas about sexual "inversion": the homosexual's sex (or gender) was thought to be inverted or reversed -- gically, psychologically, or behaviorally.[19] A binary vision of sex, gender, and sexuality underlay the medical, psychological, and psychoanalytical literature throughout the period -- a vision in which two individuals (one male and one female) were supposed to couple in heterosexual union. The scientific definition of the homosexual rested largely on the premise of a cross-sex (i.e., "feminine") personality and the behavior that was thought to go along with it. A "feminine" male -- in dress, mannerisms, or position in the sex act (i.e., "passive") -- was, in effect, the primary criterion for a diagnosis of "true" homosexuality. Wright's ideas about the character of homosexuality were not novel, but his ideas about its etiology were. He provided a new twist on an old theme -- effeminate male homosexuals in this picture became <u>hormonal</u> inverts.[20]

Wright emphasized the feminine traits and mannerisms of his subjects, especially those who liked to dress in women's clothes.[21] This, <u>if</u> accompanied by the identifying hormonal imbalance, would be enough for Wright to diagnose a case as a "true" homosexual, <u>whether or not the subject had ever engaged in sexual activity with a member of the same sex</u>.[22] He increasingly came to define "true" homosexuality by the hormonal fingerprint itself[23], and

by 1940, he was actively claiming that hormonal assays should be used to diagnose "true" homosexuality as distinct from the "latent" type. In some cases, he went so far as to argue that an individual's guilt or innocence, if criminally accused of homosexual acts, could and should be determined by hormonal levels.[24]

Finally, I would like to point out a conceptual problem with Wright's work which is common to many studies of homosexuals in this period. Wright stressed the "wifely" role that a number of his subjects played in their relationships, dressing in women's clothes and "keeping house" for another man.[25] But what about the partners of these effeminate men? How could their hormonal balance possibly account for the direction of their desire? The only possible answer, one which Wright used implicitly, but never explicitly explored, is that the "active", "masculine," "husbands" of our effeminate cross-dressing wives were not "true" homosexuals, but rather belonged to the "latent" type. Wright's hormone reversal could only provide a plausible explanation for the "passive" invert, not for his "active" partner.[26] This is an important point, and one that leads to a central question: who counts when you're counting homosexuals? For Wright, only "inverted" men, who had (or might) play the "passive" or "feminine" role in the sexual act with another man, and who exhibited the signifying hormone levels, counted as homosexual.[27] These questions are central, I think, to any study of human behavior, whether historical (as in the case of Clifford Wright's work) or contemporary (as in the case of the "gay gene"): What sample was used, according to what criteria, and how were the criteria justified? It was precisely these questions that Alfred Kinsey addressed in his landmark 1941 article, "Criteria for a hormonal explanation of the homosexual."[28]

Kinsey's article was a scathing critique of Wright's, challenging his assumptions and his methodology. Seven years before the appearance of Sexual behavior in the human male[29], Kinsey redefined sexuality solely on the basis of behavior and posited a continuum of such behavior from exclusively heterosexual to exclusively homosexual with most people falling somewhere in-between. Kinsey effectively and intentionally blurred the traditional boundaries between the categories of sexual practice.

For Kinsey, it was especially important to analyze the methods and assumptions in Wright's work because his results

> are in line with wide-spread and long-standing popular opinion that homosexual behavior depends on some inherent abnormality which, since the time of the discovery of the sex hormones, is often supposed to be glandular in origin. This assumption is offered as specific explanation in some of the psychology and sociology texts, and some of the psychiatrists are treating homosexuality as an organic disease which is to be cured...by endocrinologic adjustments. Since [Wright's] ... studies are ... the first to test the validity of the current opinion, they are likely to attract some attention; and it is, therefore, desirable to examine the bases of the conclusions.[30]

Kinsey's critique of Wright's methods was quite succinct: Wright's sample sizes were too small; there were more significant variations between the hormone levels of individuals <u>within</u> the same group than there were between the averages of the two distinct groups; and the variations within the samples of a single individual often exceeded the average difference between the homosexuals and the heterosexuals.[31] Finally, the methods for collecting data were not standardized between, or even within, sample populations.[32] In conclusion, according to Kinsey, "little significance can be attached to the results."[33]

So much for Wright's methods. But Kinsey did not stop there, and it was his next move that demonstrates his particular brilliance as a rhetorician. After questioning the validity of Wright's findings on purely scientific and methodological grounds, he then attempted to make it impossible for anyone to pursue such a study successfully by undermining the category of "homosexual" as an identity. In other words, Kinsey denied the use of the word as a noun and restricted its use to that of an adjective: Behavior could be homosexual, but people couldn't.[34]

Kinsey argued that all previous studies of homosexuality contrasted homosexuals as fundamentally different from "normals" (i.e., heterosexuals). This, for Kinsey, was a false assumption:

> More basic than any error brought out in the analysis of [Wright's] data is the assumption that homosexuality and heterosexuality are two mutually exclusive phenomena emanating from fundamentally and, at least in some cases, inherently different types of individuals.[35]

Basically, Kinsey argued that there was no adequate basis for distinguishing homosexuals from "normals." He further found no discrete distinctions between

the so-called "true" (or "passive") homosexual and the so-called "acquired" (or "active") type.[36] According to Kinsey's preliminary study, at least one-third and up to one-half of all American men had had at least one orgasmic homosexual contact in the course of their lifetime.[37] By this standard, it would be very hard indeed to identify exactly what constituted a "homosexual," and how he would differ from a heterosexual.

Kinsey's point was masterfully argued. First, he precisely defined homosexual experience as same-sex contact resulting in orgasm. This did not include homoerotic attraction, desire, or even sexual contact not culminating in ejaculation. Despite this narrow and conservative definition, however, Kinsey's preliminary studies still came up with a 35.5% figure of homosexual experience in American men. Second, he wrote about homosexuality as a behavior, not a personality or an identity. It was a behavior that was just as "normal" as anything else; it was not a crime to be punished, a vice to be prevented, or a disease to be cured. It was <u>not</u>, for Kinsey, a transgressive act. Homosexual contact was simply a normal variation of human behavior.[38] Third, Kinsey's quantitative representation of the relative percentage or proportion of heterosexual to homosexual experiences illustrated a continuum of behavior in which it was impossible to define what a homosexual was and how he would have differed from a heterosexual, except in degree.[39] In the end, Kinsey dismissed Wright by arguing that the homosexual could not really be distinguished from any one else, and therefore no study could prove the biological basis of an identity that, for Kinsey, did not exist. While Kinsey's work was not universally accepted, it was certainly influential and largely set the terms of the debate for several decades to come.

In the 1990s, however, we have come full circle. Many (though by no means all) advocates of homosexual rights look to biologic or genetic explanations in hopes of underscoring the legitimate extension of the language of civil rights to the gay community. And many of the scientists pursuing this line of inquiry are gay themselves. Twenty years ago, such biological explanations of homosexuality would have been anathema for those opposing right-wing conservatism which is often laced with eugenic and medical undertones.

What I hope to have suggested in this paper is not that sexual behavior and identify are or are not biologically determined, but rather that there is a long history of the debate. In some circles, and in some periods of time, nature or nurture might hold sway. What is important, I think, is that the fundamental assumptions made in such studies of human behavior are often based on social considerations as much as or more than on scientific ones. The 19th-century saw the invention of homosexuality as an identity, and a concomitant ideological shift from homosexual behavior as a moral failing, to a constitutional anomoly, to a crime, to an early 20th-century conception of socially unacceptable behavior as a disease.[40] In Wright's work, in the mid-1930s, a biological explanation was in certain ways "progressive" and part of the medicalization of so-called "deviant" behavior. In other words, attempting to "cure" homosexuals was seen by many as preferable to, and more humane than, incarcerating them. Kinsey attempted to render the question moot. For him, all sexual behavior, including homosexual contact, was within the range of the "normal." He attempted to undermine the category of "homosexual" as a distinct type of identity and thereby remove the justification for differential treatment, however "progressive." While I do not mean to ignore the assumptions that Kinsey himself made in the course of his research, nor to suggest that his views were more correct or less "biased" than those of his contemporaries, a critique of Kinsey would require more time than is allotted here today. Again, however, I want to point out Kinsey's work changed the nature of the debate in this country, and is still cited as authoritative in many arenas of academic and popular discourse. Now, in the age of AIDS, the political stakes regarding homosexuality have been raised once again, and we are seeing, once again, a re-alignment of the epistemological and the political. Last summer's discussion about the "gay gene" and the recent research on the "gay brain" (which, by the way, has been critiqued in a forthcoming article by Michael Gorman of the Berkeley Psychology Department) echo similar and related debates from sixty years ago and beyond. The association of social scientific research with cultural concerns and assumptions surrounding sexuality has, obviously, a long history, and continues to be an intellectual issue especially relevant to the history of the human sciences.

ENDNOTES

1. Angier, Natalie. "Report suggests homosexuality is linked to genes." New York Times (July 16, 1993), A1:2, A12:2. See also, Hamer, D.H., et al., "A linkage between DNA markers on the X chromosome and male sexual orientation." Science, 261 (July 16, 1993), 321-327.

2. Wright, Clifford A. "Endocrine aspects of homosexuality. A preliminary report." Medical Record (New York), 142 (November 1935), 407-410. Read before the Neuropsychiatric Section of the 64th annual meeting of the California Medical Association, May 13-16, 1935; Wright, Clifford A. "Further studies of endocrine aspects of homosexuality." Medical Record, NY, 147 (May 1938), 449-452; Wright, Clifford A. "The sex offender's endocrines." Medical Record (NY), 149 (June 1939), 399-402. Based on: "The sex criminal and his endocrines" presented on invitation at the American Psychiatric Association, June 6 1938; Glass, S.J., H.J. Deuel, and C.A. Wright. "Sex hormone studies in male homosexuality." Endocrinology, 26:4 (April 1940), 590-594.

3. Directory of Medical Specialties: 1939. New York: Columbia University Press, 1940. p. 102. While it is difficult to judge Wright's significance in the field, his status was high enough to have the assistance of the Los Angeles Police Chief in procuring selected samples, he gave invited speeches at important conferences, and his work was torn to bits by Alfred Kinsey in his first publication on homosexuality.

4. The most significant exception to this rule is the massive, two-volume work sponsored by the Committee for the Study of Sex Variants in New York City. Volume 2 was about lesbians. The work was published the same year as Kinsey's first article on male homosexuality, but a good 12 years before Kinsey's report on women. See Henry, George W. Sex variants. A study of homosexual patterns. New York: Hoeber, 1941. See also, Kinsey, Alfred C. [et al.] Sexual behavior in the human female. Philadelphia: Saunders, 1953.

5. Wright (1935), p. 407; Wright (1939), p. 400. The position that homosexuality is both a medical and social problem is an interesting one. It can be read as a territorial strategy, promoting one type of expertise over another (in this case the medical/social over the legal). However, the position becomes, in the end, paradoxical: the medical or social treatment of the problem is most likely to be based on conflicting ideas of the origin of the problem -- thus the nature/nurture dichotomy. It is the definition of homosexuality as a problem (whether criminal, social, or medical) that leads to the debate about causation. Why would it be important whether a behavior is environmentally or biologically determined if there were no desire to "fix," alter, or eliminate it?

6. In his later work, Wright's classificatory ideas were complicated by criminal categories and he began to distinguish the white, middle-class, law-abiding (and "congenital") homosexual from the criminal (poor, uneducated, and usually immigrant) type. The "good" kind rarely committed crimes (other than adult, consensual, homosexual acts) unless their condition was complicated by the usual culprits -- alcoholism, psychosis or feeblemindedness. The criminal type came in two varieties: the "obtrusive" homosexuals who made advances to other men (and got caught); and those who "contribute[d] to the delinquency of minor boys." Wright (1939) p. 402.

For other classificatory schemes based on economic or educational class, as well as sexual practices, see for example: Henry, George W. and Alfred A. Gross. "Social factors in the case histories of one hundred underprivileged homosexuals." Mental hygiene, 22 (October 1938), 591-611; Henry, George W., and Alfred A. Gross. "The homosexual delinquent." Mental hygiene, 25 (July 1941), 420-442; Henry, George W. Sex variants. A study of homosexual patterns. New York: Hoeber, 1941.

One of Kinsey's most significant, and controversial, conclusions was the differential sexual behavior of what he called "social level." See Kinsey, A., et al. Sexual behavior in the human male. Philadelphia: W.B. Saunders, 1948, chapter 10. Economic and educational class are central factors in early

20th-century American sexological studies -- factors that have been largely ignored by historians of sex research.

7. Wright (1938), p. 452; Wright (1939), 402. Wright did not say where he got this estimate of frequency, but it concurred with a number of other studies in the same period. Lewis Terman, for example, estimated about 4% of the male population to be homosexual. Terman, Lewis M., and Catherine Cox Miles. Sex and personality. Studies in masculinity and femininity. New York: McGraw-Hill, 1936. p. 467.

8. Wright (1935), p. 407; Wright (1938), p. 449; Wright (1939), p. 399; Glass, et al. (1940), p. 490.

9. Wright (1938), p. 452. See also, for example: Miles, C.C. "Sex in social psychology." In C. Murchison, ed. A handbook of social psychology. Worchester: Clark Univ. Press, 1936, especially pp. 688-694; Scheinfeld, Amram. Women and men. New York: Harcourt, Brace and Company, 1943[1944]; Laqueur, Thomas. Making sex. Body and gender from the Greeks to Freud. Cambridge: Harvard University Press, 1990. pp. 169-171; Moscucci, Ornella. The science of woman. Gynaecology and gender in England, 1800-1929. Cambridge: Cambridge University Press, 1990. pp. 17-18.

10. Ellis, Havelock. Studies in the psychology of sex. Volume II. Sexual inversion. [Third Edition, revised and enlarged]. Philadelpia: Davis Company, 1928. pp. 310-311, 316. See also Freud, Sigmund. Three essays on the theory of sexuality. NY: Basic Books, 1962. pp. 7-10.

11. Laqueur, Ernst and S.E. de Jongh. "A female (sexual) hormone." Journal of the American Medical Association, 91:16 (Oct 1928), 1169-1172. As the specific chemical substances were isolated and synthesized, many speculated as to the physiological origins of, for example, female hormones in the male body. See for example: Herb, Ferdinand. "Specificity of the activities of hormones, vitamins and complements." Medical Record (NY), 129 (1929), 250-254; Allen, E., ed. Sex and internal secretions. A survey of recent research. Baltimore: Williams and Wilkins Co., 1932. (2nd ed., 1939). For secondary discussions see: Medvei, Victor Cornelius. A history of endocrinology. Lancaster, 1982; Oudshoorn, Nelly. "Endocrinologists and the conceptualization of sex, 1920-1940." Journal of the history of biology, 23 (1990), 163-186; Oudshoorn, Nelly. "On the making of sex hormones. Research materials and the production of knowledge." Social studies of science, 20 (1990), 5-33; Rechter, Julia. "Definitions of sex. Biological research on sex hormones in the 1920s." Unpublished manuscript, 1990. For a psychoanalytic critique of the idea of physiological bisexuality, see Rado, Sandor. "A critical examination of the concept of bisexuality." Psychosomatic medicine, 11:4 (October 1940), 459-467.

12. Wright (1935), p. 408. See also Wright (1939), p. 399.

13. Wright (1935), p. 407; Wright (1938), p. 449; Wright (1939), p. 399; Glass, et al. (1940), p. 593.

14. Wright (1938), p. 449. In the next article, Wright took this position even further: "From records now available it appears that true homosexuality is congenital in most cases and probably originates in fetal life at the time of sex determination and sex differentiation, and is markedly influenced by the endocrine glands, and, as would be expected, most of these individuals regard homosexual interest as the natural one." Wright (1939), p. 400.

15. Wright (1935), 409; Wright (1938), 452; Wright (1939), 402. By 1938, Wright was referring specifically to testosterone and estrin. He used commercial preparations in his work, and acknowledged the companies that supplied him with the substances. See for example, Wright (1935), 409. For more on the role of pharmaceutical companies in the development of sex hormones, see Oudshoorn, Nelly. "On the making of sex hormones. Research materials and the production of knowledge." Social studies of science, 20

(1990), 5-33.

16. Often Wright did no follow-up on his subjects to see if there were changes in their hormone levels as well as in their behavior and attitudes. "Improvement" usually meant an abatement of anxiety about sexual matters, an attraction to the opposite sex, and a more "normal" hormonal assay. See, for example, Cases 2, 8, and 9 in Wright (1935), p. 408-409, and Cases 10 and 17 in Wright (1938), p. 451.

17. See Freud, op. cit., pp. 2-7, and Ellis, op. cit., pp. 75-89. Freud discussed three types of homosexuals: the "absolute" (corresponding with the "true") type, the "amphigenic," who would today qualify as a "bisexual" (or in Freud's terms, a "psychosexual hermaphrodite"), and the "contingent," for whom external circumstances constrain heterosexual contact and allow the possibility of pleasure in and consummation of homosexual acts. In some circumstances, the "contingent" can become psychologically fixed and this would define "acquired" homosexuality. (Freud, pp. 2-3).

Ellis preferred the tri-partite division of homosexual, bisexual (in its modern usage), and heterosexual, while stipulating that it was "a useful superficial division...[not] a scientific classification." (Ellis, p. 88). He also preferred some sort of congenital predisposition for inversion, which would manifest earlier or later in life, if at all. (Ellis, 308-11, 322-24).

18. Unfortunately, Wright never defined what he meant by these terms, nor where he got them. See, for example, Wright (1935), p. 407; Wright (1938), p. 399-400.

19. See Minton, Henry L. "Femininity in men and masculinity in women: American psychiatry and psychology portray homosexuality in the 1930s." Journal of homosexuality, 13 (1986), 1-21. See also: Ellis, Havelock. Studies in the psychology of sex. vol. II. Sexual inversion. Third edition. Philadelphia: Davis, 1928; Terman, Lewis M., and Catherine Cox Miles. Sex and personality. Studies in masculinity and femininity. New York and London: McGraw-Hill Book Company, Inc., 1936, chapters 11, 12, 13. Henry, George. Sex variants. A study of homosexual patterns. 2 vols. New York: Paul B. Hoeber, Inc., 1941.

20. While positing a hormonal explanation for the etiology of homosexuality, he implicitly assumed a hormonal explanation for sex differences in personality, behavior, dress, mannerisms, and direction of desire. Furthermore, Wright by 1938 was associating homosexuality with other, physical, blurring of sexual boundaries: "That homosexuality bears a close relationship to other sex irregularities -- hermaphrodites, pseudo-hermaphrodites and intersexuals -- seems quite probable; and while homosexuals fail to show the marked body changes seen in hermaphroditism and virilism, many of them do exhibit rather definite feminine characteristics. Homosexuality seems to be an irregularity of function rather than morphological make up." Wright (1938), p. 452.

21. For a recent and provocative analysis of cross-dressing, see Garber, Marjorie. Vested interests. Cross-dressing and cultural anxiety. New York: Routledge, 1992.

22. Case #2 from his 1935 article in an excellent example. Here was a 24-year old male with "no actual abnormal or normal sex experiences, but [who] had in several instances almost uncontrollable desire toward his own sex. He...frequently took girls' parts in school plays and always enjoyed dressing up in girls' clothes." After being treated with a gonadotropic substance combined with a male hormone (antuitrin S and eschatin) the subject showed "clinically marked improvement" and reported that he "felt no fear of any consequences of [male] associations." Despite the absence of any overt homosexual experiences, this subject was conceptually treated identically to several other males who claimed to have been homosexual all their lives. Wright (1935), p. 408.

23. In 1938, he divided his subjects into three classes: "positive cases where the history, examination and behavior are diagnostic of congenital homosexuality [and who showed the tell-tale hormonal reversal], ... negative cases where the data were not suggestive of true homosexuality, but for some reason the examination has been made with this condition in mind, such as court cases [and who did not show a reversal in the proportion of sex hormones], ... [and] examinations for other conditions such as gynecomastia [enlarged breasts in men], where hormone assays significant of homosexuality have been found [and who were, therefore, diagnosed as "true" or "congenital" homosexuals]. Wright 1938, p. 449. Wright's clinical profession is evident when he does not provide "treatment" or follow-up hormone assays for those subjects who cannot afford them. Wright 1938, 450.

24. See Wright, (1939), p. 400, and Glass, et al., (1940), p. 593.

25. See, for example, Cases 1, 5, and 6 in Wright (1935), p. 408-409.

26. When Wright was writing, there was no scientific consensus of constituted the "normal" hormone levels for heterosexual males or females, and for most of Wright's early work, he used no control groups to compare to his homosexual subjects. This is one of the points on which Kinsey would base his critique of Wright's work.

27. More than 20 years earlier Freud had expressed "suspicion" that the distinction "between 'innate' and 'acquired' [was] not an exclusive on or that it [did] not cover all the issues involved in inversion." Freud, p. 6.

28. Kinsey, A.C. "Criteria for a hormonal explanation of the homosexual." Journal of clinical endocrinology and metabolism, 1 (May 1941), 424-428.

29. Kinsey (1948). The female report appeared in seven years later: Kinsey, A., et. al. Sexual behavior in the human female. Philadelphia: W.B. Saunders, 1953.

30. Kinsey (1941), p. 424.

31. Kinsey (1941), p. 424.

32. Kinsey (1941), p. 424.

33. Kinsey (1941), p. 425.

34. Kinsey's later work is not so consistent on this point. See "Homosexual outlet" in Kinsey (1948).

35. Kinsey (1941), p. 425.

36. Kinsey (1941), p. 427. Interestingly enough, this supported Freud's comments on the subject. See note 27.

37. The conclusions are drawn from a sample of 1,600 male case histories, about half of whom were of college age. Kinsey (1941), p. 425.

38. On Kinsey's ideas about variation and taxonomy in biological populations, see: Kinsey (1948), pp. 16-21. See also: Gould, Stephen J. The flamingo's smile. Reflections in natural history. New York: Norton, 1985, chapter 10; Robinson, Paul. The modernization of sex. New York: Harper and Row, 1976, pp.51-54.

39. Kinsey (1941), p. 425. This work pre-dates Kinsey's (in)famous six-point scale of homosexuality. See Kinsey (1948), pp. 636-659.

In the 1941 article, there were cases of a predominantly heterosexual male who had more homosexual contacts than a man who had exclusively homosexual relations; and vice-versa. Case 8306, for example, was 22 years

old, classified as 19% homosexual, with a reported 10,000 heterosexual contacts, and 2,400 homosexual contacts. [Case 8306 was a busy man!] Case 4047 a 24-year old man, was 100% homosexual and had had only 1,600 homosexual contacts. Case 2777, a 29-year old man, was 78% homosexual and had more heterosexual contacts (4,200) than all but one other man (homo- or hetero-sexual) in the series. Note that percentages are of homosexuality, not of heterosexuality. Kinsey (1941), p. 427. This is in keeping with Kinsey's propensity to assume "that everyone has engaged in every type of activity. Consequently we always begin by asking when they first engaged in such activity." Kinsey (1948), p. 53.

40. See, for example, Foucault, Michel. The history of sexuality. Volume 1. An introduction. New York: Pantheon, 1978.

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Meaning - Dialogue - Society. The Comparison between G.H. Mead's, L.S. Vygotsky's and M.M.Bakhtin's Concepts of Dialogue and Meaning.

"Dialogue" is currently a rather fashionable term and it appears in very different and sometimes mutually exclusive contexts. In order to elaborate this notion I would like to examine three concepts of dialogic character of language in which, as many claim, contemporary theories of language have been rooted. I think of G.H. Mead's social pragmatism, L.S. Vygotsky's idea of language aquisition and M.M. Bakhtin's conception of interpretation as reconstruction of the dialogue inhered in the work of art. They have in common not only the period of their origins (twenties and early thirties) but also the same perspective on languauge phenonomena in which a meaning of the word is viewed as the effect of the social interactions. G.H. Mead had no idea of the work of the Russian semioticians and they did not know his work either. Bakhtin and Vygotsky had very good personal connections but after the period of close collaboration in early twenties their ideas went in the different directions.

G.H.Mead gives the following definition of meaning:" A gesture by one organism, the resultant of the social act in which the gesture is an early phase, and the response of another organism to the gesture, are relata in a triple or threefold relationship of gesture to first organism, of gesture to second organism, and of gesture to subsequent phases of the given social act; and this threefold relationship constitutes the matrix within which meaning arises or which develops into the field of meaning".[1] In this way meaning is constituted by gesture and

[1]G.H.Mead, Mind,Self and Society, Chicago 1959, p.76.

a subsequent social action. Each of these elements represents a different level of the process which constitutes meaning, and therefore I would like to consider them separately.

According to Mead, "gesture" is a notion that enables a transition from action to mind and therefore makes it possible to answer the challenge made by behaviorism. Gesture is action which is rooted in the biological endowment. However, it is due to gesture that social communication is achieved, which is a prerequisite of all social processes. In their simplest forms, gestures are biologically determined actions through which one organism influences another one's behavior. Mead's favorite examples are dogs barking at each other and fencers or boxers during a fight. All three examples are instances of communication; conversation through gestures. Movements of one organism--the barking of a dog or a fencer's attack--evoke a complementary behavior by another organism: the second dog responds by barking, and a partner assumes an appropriate attitude.

According to Mead there are types of social behavior of instinctual character, for instance, sexual behavior, some aspects of parental behavior, and differentiated actions in insects' societies. The difference between these types of behavior and communicative actions, even the simplest ones of gestures, consists in the fact that in the former the other individual is treated merely as part of the environment, whereas communication aims at a change of another individual's behavior, which carries him out of his environment. On a specifically human level communication is carried out by gestures which are meaningful, that is, through significant symbols. Gestures acquire meanings when an individual who performs them reacts in the same way as an individual to whom they are addressed. This enables both co-ordination of actions between individuals and the control of one's behavior.[2]

For Vygotsky meaning is a basic unit of verbal thinking. Meanings of words combine thinking and speech, generalization and communication. A word comprises an element of thinking because it refers to entire set of objects and not to an individual thing. Speech is a means of communication; that is its original function.[3] According to Vygotsky this function cannot be separated from generalization. Expressing one's mental states always presupposes relating them to certain general categories. Meaning includes both communicative and

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[2]ibidem, p.69.

[3]L.S.Vygotsky, Thought and Language, Cambridge Mass. 1965, p.120.

cognitive functions of speech. The first can be fulfilled only because meaning is generalization. It creates a framework in which process of communication can arise and evolve. From the same perspective Vygotsky examines the development of speech in a child, stressing the social character that it has from the beginning. Further development causes a differentiation of various functions of social speech, and as a consequence there occurs a division into egocentric and communicative speech. The social character of egocentric speech consists in a transference of forms of social interactions into mental functions. Egocentric speech is external as regards its physiological nature, but internal due to its mental functions. Vygotsky assumes that speech development occurs according to the following scheme: social speech-egocentric speech-internal speech. Examinations of egocentric speech make it possible to investigate a process of transformation of the social and external into the individual and internal. The analysis of egocentric speech led Vygotsky to a description of internal speech. Internal speech is a totally different structure, incomparable with external speech, and this structural difference involves a considerable functional difference.[4]

The transition of the external into the internal is a process of creating new mental qualities and not mere internalization. Such a treatment of internalization was described by Vygotsky's disciple Galperin as the transference onto the plane of consciousness is the transference of mind-formation and not a simple production of meaning.[5] According to Galperin such an attitude differentiates the concept of internalization elaborated by Vygotsky from the French sociologists' concept of internalization and Piaget's psychological theory. The sociological concept treated the internalization as incorporation of elements of social consciousness into an individual consciousness. Piaget regards internalization as an evolution of logical operation from the overt actions to the inner capacities.

M.M. Bakhtin develops the idea of the dialogue character of language in the narrative works of literature. However, he extends the dialogue dissemination to the single word: " What we are calling a hybrid construction is an utterance that belongs, by its grammatical (syntactic) and compositional markers, to a single speaker, but that actually contains mixed within it two utterances, two speech manners, two styles, two 'languages', two semantic and

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[4]ibidem, pp. 19-20.
[5]P.J.Galperin. Toward a Theory of Internalization, voprosy Psichologii 6/1966, p.28.

axiological belifs systems. ... It frequently happens that even one and same word will belong simultaneously to two languages, two beliefs systems that intersect in a hybrid construction - and, consequently, the word has two contradictory meanings, two accents."[6] Meaning of a word is always referred to an utterance and it, in turn, to its social context. Therefore, any language expression contains at least some elements of dialogical construction. Each utterance is only a link in an endless continuity of language communication. But this communication depends strongly on the social and cultural circumstances, it is only a moment in the process of group formation. So, Bakhtin raises a metodological question of relationships between verbal and non-verbal activity as labour, rituals or ceremonies. Language is being formed in the concrete language communication not an abstract system of language forms or an individual mind of speaker. This concept of language implies the specific concept of the self conceived as constituted in the relationships with the other. Referring to Kant, Bakhtin accepts his dialectic between mind and world but rejectes the idea of "transcendental synthesis". Instead Bakhtin "posits that the architectonic activity of tha mind partake in a dialogue that is influenced by *hic et nunc* characteristics of the most immediate reality."[7] Dialogue betwee the Self and the Other becomes the frame for the construction of human individuality. From Bakhtin's point of view the pronun "I" is merely an empty term as the Self needs the Other for one's own completion.

Given the short presentations of Vygotsky's, Mead's and Bakhtin's ideas, I would like to proceed to the comparison between them. They agree at least to the following assumptions: First, language is a kind of activity which can be conceived only in its social and cultural settings. Second, the meaning of the word emerges in human actions and interactions. Therefore, meaning is not limited to ideas in human mind or to merely intelectual activity. Three, mind as well as human self appears as an implication of social relationships in which an individual is involved.

Despite these similarities they differ in many respect as to detailed issues and even as to same basic assumptions. The definite comparison between their theories is difficult because of

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[6]M. Bakhtin, The Dialogic Imagination: Four essays by M.M. bakhtin, Ed. M. Holquist, trans. C. Emerson and M. Holquist. Austin: University of Texas Press 1981, p. 304 ff.

[7]S. Daelemans and T. Maranhão, Psychoanalitic Dialogue and the Dialogical Principle, [in:] The Interpreation of Dialogue, edited by Tullio Maranhão, The University of Chicago Press 1990, p. 225.

their incomplete character. As is well known L.S. Vygotsky died in the critical point of his scientific development, G.H. Mead rewrited his manuscripts several times and M.M. Bakhtin changed at least three times his views on crucial issues. However, I think that the main line of division is the concept of human freedom. Vygotsky wants to show that human action can be conceived in entirely rational terms. For this reason he referrs to Spinoza's philosophy where the unity of the symptoms of mental life results from his assumptions of the structure of reality. Similary, for Vygotsky, rational, hierarchic and conceptual nature of the world legitimizes the assumed model of mind. G.H. Mead put emphasises on the independence of human action by introducing the concept of "I" as the source of freedom in personality. The dialectics between "I" and "me" marks the field where an individual can transcends social conditions of action. An action has a creative character in the sense that it is not an acticity directed to achieved pre-established purposes but it is a chain of adjustments and readjustments to changing circumstances in the environment. M.M. Bakhtin's point of departure in the first stage of his development is the absolute freedom of individuals which entails his or her absolute responsibility. Later on, he shares with Vygotsky the idea of the role of signs in the mind formation but contrary to Vygotsky he does not accept the absolute rationality of the world. The notion of carnival central for Bakhtin's later works reveals the possibility of dissolving "I-experience" in "we-experience". An individual becomes merely a part of the biologically oriented, unconscious crowd.

STRUCTURING SOCIAL RELATIONSHIPS THROUGH DISCOURSE AND ARCHITECTURE: THE CASE OF A COLONIAL ASYLUM

ABSTRACT

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In this paper we examine aspects of the architecture of an asylum built at the end of the nineteenth century in the Cape Colony of South Africa, in conjunction with documents such as patient records and annual reports, to trace how relevant social categories were re-created and reinforced. It is argued that the categories of insanity, race and gender were significant considerations in the design of a building aiming at the `moral management' of inmates, and that these considerations emerge from the documentary records as well. These categories were integrated into the wider ideological picture of the late nineteenth century via the interlocking discourses of order versus disorder, cleanliness versus dirtiness, civilised versus uncivilised, a menial labouring class and a non-labouring class, and self-control versus lack of self-control.

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"Toon mij uw huis, en ik zal zeggen wie u bent"

("Show me your house and I will tell you who you are")

Old Dutch proverb

In recent times the history of institutions has taken a central position in the history of the human and social sciences. It is now widely recognised that modern institutions such as schools, mental hospitals, factories, and prisons, played a crucial role in the way in which the subjects of the social and human sciences emerged. In this paper we examine the architecture of an asylum built at the end of the nineteenth century in South Africa, to examine how a number of familiar themes played themselves out in a colonial context.

Apart from the original architectural drawings, the primary data sources for the present study have been patient case records, annual reports of asylum superintendents, and annual reports of the Inspector of Asylums. Our point of departure has been that one can look at both documents and buildings as texts to be interpreted, one in terms of the other.

The theoretical assumption guiding the study was that buildings structure space so as to control social relationships. In other words, was the architecture able to affect social relations and practices, and if so, how? As Donnelly (1983, p. 55) put it for asylum architecture: "'Moral management' required a distinctive architectural plan which ... materially supported and enabled the new social relations..." In a more general sense, these are questions about interrelationships between changes in material culture and changes in social relations.

Valkenburg Asylum, established in a suburb of Cape Town, in 1891, provides an excellent backdrop for an examination of these issues. Firstly, it was the first mental asylum in South Africa carefully designed to emulate the most modern and successful of British asylums. It was designed by the Scottish architect Sydney Mitchell, who was the consulting architect to the Board of Lunacy of Scotland, and who designed the asylums of Gartloch and Govan in Glasgow. Secondly, it was designed specifically as a `Europeans-only' asylum, created to cater for a `better class' of white patient, and `hopeful cases' for whom there was some possibility of cure. The buildings erected between 1894 and 1899 were intended for these patients; in 1916 the old Uitvlugt plague camp, bordering on the asylum, but separated from it by a river, was opened to accommodate the `black insane'. Finally, Valkenburg Asylum also reflected nineteenth-century concerns with patient classification beyond those of race and class; in particular with gender and type/degree of disturbance.

The main buildings on the `white side' were arranged on what was a dispersed pavilion plan, with the separate blocks joined by covered walkways. This plan had become current in Britain from the early 1880s on (but see Donnelly, 1983, p. 57). These buildings were symmetrically arranged about the administration block which acted as the focal point of the design. The main block, behind the administration block, itself contains a number of diverse elements all separately expressed and roofed. Thus the front elevation of the building was preserved by pushing all those elements concerned with the services to the rear. Radford (1979) saw this pattern in domestic houses as well, where a fine facade was accompanied by a service `tail', containing kitchen and dining room, for example.

The asylum was situated on the outskirts of the city, at a distance from built-up areas. This was a major reason for the choice of the site at Valkenburg: to place the new establishment within a moderate distance of town; three miles was considered ideal (Radford, 1979). Despite considerable public alarm at housing lunatics so close to residential areas, it was thought that accessibility was an important enough consideration to override these concerns: patients could receive regular visits from friends and family, and outings could be arranged more easily for convalescent patients. This positioning of the mad at the periphery of the city limits provides eloquent testimony to their social position.

The site was also ideal in that it was on the large grounds of an old farm. At a very practical level it made the asylum self-sufficient in aspects of its food supply. Furthermore, healthy labour formed an integral part of the moral management of patients. Dodds, the first superintendent of the asylum (and another Scot), constantly stressed the need to provide humane care for lunatics, in a treatment regime which included congenial work and recreation in pleasant, restful surroundings. Thus farming activities and work performed in asylum workshops were helpful to patients' recovery, together with rest and wholesome diet. Clearly this kind of `occupational therapy' can only flourish when sufficient space and suitable land are available.

If the therapeutic value of labour is examined in terms of race, however, the subversion of moral treatment in colonial contexts becomes clear quite quickly. It was difficult to make `Europeans' engage in work at all, especially not work which they regarded as beneath their status. `Natives' were induced to maintain essential asylum services with their labour, which can hardly be regarded as a `remedial occupation'. The admission criteria for blacks reinforce this pattern: numerous case records suggest that black patients were only admitted to Valkenburg if they were quiet, relatively self-contained - and therefore likely to work well. By regarding blacks primarily as an unpaid menial labour force, attention was drawn away from their insanity, and need for care. Thus the black inmates of Uitvlugt were like servants, and in this disguise, their occasional contact with European patients reproduced the pattern of contact between black and white in society at large.

In his 1893 annual report Dodds stated that he in fact regarded racial classification as of equal importance to mental classification, the separation of patients according to the degree to which they were quiet or noisy, violent or tractable. In April 1907, the medical superintendents of four asylums reaffirmed the principle of segregation by race, noting that even `the better class of Coloured Patients' would cause European patients `great dissatisfaction', an indication of the extent to which class and race became mixed in the British colonies. However, the patient classification apparent in the establishment of `black' and `white' asylums encompassed much more than race. Fundamental to scientific methodology, at least since the seventeenth century and the Enlightenment (see Lovejoy, 1960), classification was the cornerstone of the treatment of lunacy in the nineteenth century. Appropriate classification of patients into groups according to social class and gender, as well as race, was thought to `remove causes of irritation' which exacerbated the insanity. Superintendent Dodds subscribed to this, and wanted wards at Valkenburg to house males and females separately, as well as separating different diagnostic categories. Lack of accommodation to house the insane however allowed only for the consistent separation of men and women - within an institution segregated by race in the first place.

Classification, categorisation, separation - all intended to systematize observation, and to bring order to social relations, the sort of order thought to inhere in nature. This is reflected in the very buildings themselves. This manifested in numerous ways:

- The asylum was set on the margins of normal social activity, physically separating its inmates from society via clearly defined boundaries. It placed them in a circumscribed location, establishing control over access to the institution. Black and white were separated via a river (ironically named the Black River), and male and female into different wards. The outer boundaries could be crossed in one place only, namely the front entrance of the administration block. This reinforces the fact that these boundaries had to be respected, and that crossing them was an act which carried social weight.

- The formal approach to the asylum was towards the administration block, providing the best elevation of the complex. It was symmetrical in design, and screened the visitor from

the service elements behind it. Thus the visitor first and foremost saw an orderly facade to the institution, with the `messy' and more informal aspects hidden from first view. Behind this central building, other buildings also were symmetrically arranged. Symmetry is the concrete manifestation of abstract concepts such as a regard for order and the ability to exercise control (Brink, 1992).

- Desire for `cleanliness of habit' which emerges so clearly from case records and annual reports reinforces this line of argumentation. Cleanliness in fact can be regarded as an embodiment of order, and black as well as white patients had their progress monitored in terms of the extent to which they `clean in habit'. Official Visitors to the asylum recorded approbation in terms of wards being `in beautiful order', implying both cleanliness and tidiness. In order to preserve both order and cleanliness, patients `of insanely dirty habits' were confined to single rooms, and were thus banished from sight. The situation with black patients was more complex. Black people were generally regarded as `dirty by instinct, habits and necessity'. Therefore, `clean' simultaneously meant `civilised' in the colonial anxiety over racial hierarchy, miscegenation, and degeneration. The need for single rooms for black women patients was noted to `exceed the number of single rooms available'. The concern with cleanliness and racial hierarchy (and type of work performed) went beyond the patients, and extended to the nursing staff as well. In 1911 a Member of Parliament asked the Minister of the Interior about white nurses who `were compelled to perform degradingly menial offices towards native men patients, which could well be performed by male native attendants'.

- However, the concern with symmetry and order did not extend to the buildings used to house black patients. The carefully-designed, interlinked pattern of the `white' side is in striking contrast to the haphazard and separate `black side' blocks. Moreover, the `black side' was administered from the `white side' administration block: for black patients there was no holding centre, but control from afar, replicating the social distance characterizing relationships between black and white in colonial society.

In conclusion, the primary data sources utilised in this study, allow us to identify, in a preliminary way, various interlocking discourses concerning the insane in a colonial setting at the turn of the century. These are of order versus disorder, clean versus dirty, civilised versus uncivilised, menial labour versus `occupational therapy', being capable of self-control versus lacking self-control. These addressed, (re)constructed, and structured social relationships between men and women, black and white, and the sane and the insane. Sometimes implicitly, sometimes explicitly, these relationships were interpreted in terms of a hierarchy, in which those higher up in the hierarchy were extended greater worth than those lower down.

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SHIFTING SCIENTIFIC RESEARCH PRACTICES AND CAREER STRATEGIES OF THREE COHORTS OF SOCIAL PSYCHOLOGISTS RECEIVING PHDS FROM THE UNIVERSITY OF MICHIGAN BETWEEN 1948 and 1973.

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ABSTRACT

This study follows up on previously reported data on a first cohort of social psychology PhDs trained (1949-53) at the University of Michigan in the joint Social Psychology programme or in the Psychology Department; we add data for two more cohorts (trained 1959-63 and 1969-73). Of the 258 PhDs studied, 54 (20.1%) were women. Several global indices of scientific production were collected for each student from their PhD was awarded until 1993; a sample of their published articles has also been analyzed in a series of on-going and inter-related studies. We had first noted an apparent overall decline in mainstream journal publication rates in successive cohorts, as well as an apparent drop in funding support level, seemingly to the disadvantage of women social psychologists. In this study, however, when each of the 54 women was paired with a fellow male graduate student trained approximately the same year and by the same mentor, gender differences largely disappeared across cohorts, while some differences remained between students trained in the Social Psychology programme and those from the Psychology Department. Although all students had come from the same general learning environment and shared a relatively small group of mentors, over time, different research practices and strategies evolved separately for each cohort.

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THREE COHORTS OF SOCIAL PSYCHOLOGISTS

In looking at the history of certain disciplinary research practices (Danziger, 1990), the social negotiation concerning certain traditions can be teased out through the analysis of a variety of social interactions among scientists (Lubek, 1994 a). This paper focues particularly upon the mentoring relationship between a PhD candidate, as apprentice, and her/his thesis supervisor. To what degree can the contemporary research practices of a scientist be traced back related to the original training received in graduate school, decades earlier? And are any linkages altered when a sub-discipline such as social psychology transits a multi-faceted crisis involving the questioning of its theories, meta-theories, epistemology, methodologies, and ethics (Lubek, 1994b)?

Since 1992, a series of studies by the authors has attempted to look at the evolution of post-World War II American social psychology by specifically focusing on the more than 400 social psychologists mentored between 1949 and 1973 at the University of Michigan -- by far the largest training centre for the sub-discipline. During that period, 226 social psychologists received their PhDs from the "Social Psychology" programme, jointly run by the sociology and psychology departments, whose guiding spirits were Ted Newcomb and Dan Katz; a similar number of students took their doctoral degrees in social psychology from the "Psychology Department" itself (which continued to award such degrees after 1973, when the joint programme ended). Students of social psychology, who

were accepted into either the Social Psychology programme or the Psychology Department, generally had similar training, followed much the same courses, and often shared the same doctoral mentors and thesis committee members. In fact, more than half the students in our study were supervised by a small group of 6 mentors (Katz, Newcomb, Cartwright, French, Lippitt and Zander) and 8 of their own former students.

The First Cohort of Michigan PhDs (1949-1953).

Last year, Bauer, Thoms and Lubek (1993 a, b) and Thoms, Bauer & Lubek (1993) reported data on the first 5-year cohort of students who graduated with PhDs during 1949-1953. We identified some differences in long-term research strategies and scientific output of the students, over the course of their careers spanning a further three to four decades. Some of these career strategies and research practices seemed more related to the choice of mentor while others seemed linked to the student's choice of affiliation with either the Social Psychology programme or the Psychology Department. For this first cohort, the most consistent finding was that a small group of students mentored by Leon Festinger, whose penchant was for laboratory experimentation, stood out on almost all measures of career visibility and mobility (e.g., publications in social psychology journals; Social Science Citation Index counts; number of career moves; success in winning research grants, especially from the military, etc.).[2] Cohort 1 Social

[2] Festinger's students were the most mobile. On the average, they held 4.0 jobs after graduation, while other Social Psychology students held 2.6, and Psychology Department PhDs had only 1.8. Festinger left Michigan in 1951 and did not mentor students beyond Cohort 1, where he trained 6 PhDs.

THREE COHORTS OF SOCIAL PSYCHOLOGISTS

Psychology programme graduates did relatively better than their Psychology Department colleagues in finding funding for their empirical studies during the 1950s,1960s and 1970s.[3]

In choosing the laboratory setting for their empirical research articles, Festinger's students almost exclusively preferred this experimental strategy, as seen in over 85% of a sample of their articles published between 1948 and 1979. By contrast, their Social Psychology and Psychology Department counterparts who had received the same graduate programme, used this setting in only about two-thirds of their work published during the 1950s. While Social Psychology students continued at this same level during the next two decades, Psychology Department students increased their use of the laboratory in each successive decade until they "caught up with" the Festinger students in the 1970s-- they were jumping increasingly on the laboratory experimental bandwagon, at the very height of the crisis when those same laboratory methods were under critical fire (Jackson, 1988)![4]

Cohorts Three (1959-63) and Five (1969-1973) and the Question of Gender.

We have since extended our study to the "middle" and "end" cohorts of the Social Psychology programme, and of their

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[3] Many retired during the early 1980s.

[4] Social Psychology students were more infrequent users of mixed-variable designs involving at least one manipulated variable; by the 1970s, amidst the sub-disciplinary crisis, the Psychology Department students had almost caught up to the Festinger students, who led the literature in such "manipulation" research strategy. And Social Psychology students never shied away from the use of correlations in their studies, to the same extent to which all their other confreres did after graduation.

contemporaneous fellow students in the Psychology Department[5]. While Cohort 1 contained 43 men and just 6 women, Cohort 3 had 76 men and 18 women, and Cohort 5 had 95 men and 30 women. Overall, the 54 women comprised 20.1% of the PhDs. An initial examination of the research production of the 26 women from the Social Psychology programme, when compared to their 117 male counterparts, led us to think that on many of the measures of scientific visibility, these women had fallen behind. [6]

However, it was felt that a fairer test of the impact of mentoring would be to match each of the 26 Social Psychology and 28 Psychology Department women PhDs with a male who had obtained his degree at about tne same time with the same mentor (or from a similar mentor grouping-- cf. Thoms, Bauer & Lubek, 1993).

Career Funding Strategies of "Matched Samples" of Men and Women Social Psychologists in Three Cohorts

In this study, we must confine ourselves to a discussion

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[5] We have studied this programme, in all, for 15 ofits 25 years of existence. rsThe three cohorts examined include about 60% of all Michigan PhDs. Eventually, we hope to complete data collection for Cohorts 2 (1954-1958) and 4 (1964-1968). We thank Dr. Dan Katz and Ms. Betty Danielson for their help in assembling the list of all social psychology graduate students. Our sample contains several anomalies: In Cohort 1, 4 of the students continued their research at Michigan but actually received PhDs from MIT, where the Lewinian Research Centre for Group Dynamics had been located before moving to Michigan in 1948. In Cohort 3 and Cohort 5, 9 and 4 students respectively had mentors from the Sociology Department. At least 7 of the 54 women, according to information supplied by Dr. Joseph Veroff, Graduate Chair of the Psychology Department at Michigan, and by the Alumni Records Office, used names other than those appearing on their PhDs at some point following graduation (usually indicating a married name). Additional data tracking these names, as well, will be sought in future studies. Although the Social Psychology programme was ended in 1973, several students' degrees were awarded in 1974, and we include them in Cohort 5.

[6] For example, preliminary calculations showed Cohort 1 women published about 25% as much as did the entire Cohort 1 while Cohort 3 women published at only 39% the average rate of their cohort. Most strikingly,the women of Cohort 5 produced only about 10% the rate of the whole cohort.

of the funding of the research of the 54 women and their male PhD counterparts.[7] For all 108 social psychologists, we consulted Cumulative Psychological Abstracts for the lists of articles published after the dissertation and up to the end of 1993. We selected for duplication and examination all "empirical" articles (Thoms, Bauer & Lubek, 1993) available to us[8]. Not everyone in our sample played the "publish or perish" academic game. Thus 66.1% of the 56 Psychology Department members in our sample yielded no published articles (17 women and 20 men), and 50% of the 52 Social Psychology programme PhDs (14 women and 12 men) were equally "invisible" to us. Concerning later publishing invisibility, the choice of doctoral programme may have played a more salient role than gender. When we examine the "empirically publishing and available" men and women in our matched sample, the men did somewhat better, overall, with 3.6 publications compared to 2.7 for the actively publishing women; men also seemed to have published more articles in "non-available" journals, perhaps as part of a research and/or career strategy. Over time, there was some change in the publishing patterns among succeeding

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[7] Of the 54 pairs matched, 30 had identical mentors,and 24 were from same "mentor group" (e.g., Katz and Enter and their students; Cartwright, Zander, Lippitt and French and their students). 28 pairs graduated the same year, 17 pairs graduated one year apart, while 5 pairs had 2 years' difference and 4 pairs 3 years' difference in the awarding of their PhDs.

[8] Because each selected article was read and evaluated concerning a number of methodological and research design choices, only those reporting "data" were gathered for this study. While most mainstream journal articles were readily available for our sampling, some articles were in more "esoteric" publications. For the Psychology Department sample, for example, we were able to include 80.3% of published women's studies, but only 54% of the men's studies.

cohorts, and the gender gap disappeared with Cohort 5.[9]

When we specifically examined each empirical article for the major source of funding, we found little change in funding practices from Cohort 1 to Cohort 5, with 69.4%, 64% and 64.4% of the examined articles acknowledging grants. In Cohort 1, women's articles received 43% funding compared to 76% of the men's; Cohort 3 saw a dramatic "role reversal" with 73% of women's articles funded and only 57% of men's. By Cohort 5, funding practices had stabilized with 64% of men's and 65.7% of women's articles in our matched sample funded. Overall, women and men both had about 65% of their articles funded[10]. Social Psychology programme graduates had about 61.2% of their articles funded compared to 71.7% for their colleagues in the Psychology Department. Again, using this "matched sample" for "equally-mentored" students, gender seems to play less a role in career funding practices than does the initial choice of which doctoral programme to enter at Michigan.

Some Tentative Conclusions

There do seem to be some differences in the later professional activities of PhD students coming out of the two Michigan programs for social psychologists in three different cohorts(/decades), although many of them shared the same mentors. When all graduates are compared, one small group of students from the very first cohort, trained by Festinger,

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[9] The 4.5 mean publications per person of Cohort 1 was soon diminished to 2.9 for both Cohorts 3 and 5; the Cohort 5 women published with identical frequency to their male counterparts, compared with the earlier 5.8 publications for Cohort 1 men against 2.3 for their women counterparts.

[10] Women received 76% of their grants from either government or military funding; men received 70% from these sources.

stands out on a number of indices. They also made a number of methodological choices that were later questioned during the sub-discipline's crisis in the 1970s. Psychology Department graduates of the first cohort seemed to be increasingly emulating this group by the 1970s on a number of indices of research practice. As well, it appears as if the publication and funding riches enjoyed by the first cohort were diminished for succeeding cohorts. Over time, the two programmes training the students were fine-tuned (cf. Cannell & Kahn, 1984) and changes occurred during those three decades in psychology, in society, and in the universities. There were a number of differences between students willing to enter an interdisciplinary social psychology programme and those who sought a social psychology degree from a traditional Psychology Department. But perhaps the most interesting point for follow-up concerns the evolving position of women in social psychology. Global indicators of the women's research strategies and career choices (as a 20% minority in the overall sample) indicate that the men had stronger career visibility indices. However, when each woman was matched with a similarly mentored male PhD, the women were seen to make gains with each successive cohort. For those women trained during the last period (1969-1973), there was an apparent approaching of equity on some indices concerning research funding.

Is such "progress" for women an artifact of the "matching" technique? Were those instructors willing to mentor women somehow different in the research strategies they passed

on to both their women and men students, than other mentors at Michigan ? It is at this juncture where the limitations of such quantified historiography become apparent, and we shall need to look further into interview and qualitative data for a finer understanding of the mentoring dynamics. Perhaps next year we'll be able to unravel more of the mysteries of the impact of graduate training on long-term professional behaviour.

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Per Magnus JOHANSSON

Sigmund Freud wrote his *On the History of the Psychoanalytical Movement* in 1914. Nine years later, in 1923, he felt it necessary to append the following footnote: "At the present time the Scandinavian countries are still the least receptive." These words call our attention to the fact that Ola Andersson pursued his work in a country where psychoanalysis ran across uncommon difficulties in achieving a foothold. We might add that the history of these difficulties has remained essentially untold.

Pioneers

Poul Bjerre (1876-1965) was the first Swede to become interested in psycho-analysis. His father was a merchant who had emigrated from Denmark in 1874 to settle down in Gothenburg. Bjerre took his medical training in Stockholm and developed an early interest in hypnosis.

In 1910 Bjerre journeyed to Vienna in order to meet Freud. The following year he spoke before the Swedish Medical Society concerning Freud's ideas. The lecture both offered a positive view of psychoanalysis and refuted objections to the theory. Nevertheless, Bjerre was subsequently to harbour the same objections himself. Normally, lectures held before the Swedish Medical Society were published in the magazine *Hygiea*. Bjerre's lecture was turned down on the pretext that it was too long.

Bjerre published his articles in several languages, translating and providing commentaries on the works of Freud and his followers. *The Psychopathology of Everyday Life*, translated in 1924, was Freud's first work to appear in Swedish. In the same year Bjerre published a book in Swedish entitled *Psychoanalysis. Its Rise, Transformations and Application*. The book included contributions by Bjerre, Freud, Carl Gustav Jung, Oskar Pfister, Johann Künkel, Alfred Adler, James Putnam, and Alphonse Meader. Bjerre concluded the book with an article entitled *The Path to Freud and Beyond*, in which he aimed to demonstrate the limitations of Freud's teachings.

In introducing psychoanalysis, Bjerre made no distinction between its founder and his disciples or between disciples who broke with its founder and disciples who remained within the founder's theoretical frame of reference. They were simply all thrown together in one volume.

Bjerre found no need to contact any other psychoanalyst in order to achieve greater self-understanding or for that matter a greater understanding of the subject itself, and he consequently did not enter into training analysis. Thus, he perceived Freud to be a psychotherapist on the same level as himself.

Bjerre's lifetime project was to be the development of psychosynthesis, a blending of religion and natural science. He had developed his own independent attitude to Freud even before either reading or meeting him.

Bjerre never had either followers or children of his own. By the time Sweden's handful of psychoanalysts organised themselves for the first time in the early 1930's, Bjerre was in the process of founding his own institution.

The first generation of psychoanalysts organises itself

The Finnish-Swedish Psychoanalytical Association was founded in 1934. Its first President was Alfhild Tamm (1876-1959), Sweden's first female psychiatrist. She had been a member of Vienna's Psychoanalytical Association since 1926. In a manner typical of the Swedish pioneers of that time, she submitted to short periods of analysis abroad with Paul Federn, Helene Deutsch and August Aichhorn. Her contributions to the field of training analysis were limited, however, since for a long period she was the only one who practised in this area, relegating her theoretical interests to a position of secondary importance.

At Freud's request, Ludwig Jekels had come to Scandinavia in the mid-30's to train the Swedish and Finnish pioneers. In a official report Jekels described the difficulties associated with working in Stockholm, particularly the implications of the fact that a single person at the same time was responsible for educational psychoanalysis, conducting seminars and supervising psychoanalysts. He also maintained that the majority of those who wished to be trained in psychoanalysis already maintained some kind of practice and therefore lacked genuine motivation for learning something new.

After a little more than three years, Jekels abandoned his efforts and decided to return to Vienna, and a short while later he settled down in the U. S. He had been responsible for the Finnish-Swedish educational centre. But the centre was dissolved in 1937 due to a lack of suitable candidates. The first attempt to transplant psychoanalysis to Sweden had gone down a blind alley. In the twentieth issue of the *International Journal of Psychoanalysis* (1939), Ernest Jones reported that Sweden had experienced a wave of resistance, and that geographical obstacles prevented the few Swedish members from meeting each other. The resistance among physicians to psychoanalysis, which had first bared its fangs in connection with the attempt to publish Bjerre's lecture in 1911, was equally strong during the course of this initial attempt to transplant psychoanalysis to Sweden. In 1934, the same year that The Finnish-Swedish Psychoanalytical Association

was founded, a book was published by Bror Gadelius, who was an internationally renowned scholar and often referred to as Sweden's first modern professor of psychiatry. Gadelius advanced the idea that the psychoanalytic theory of the Oedipus Complex and the significance of childhood memories could to some extent be applied to the inhabitants of southern Europe and to Jews, but only in exceptional cases could be used to explain the emotional life of Scandinavians. In the same year an influential professor of philosophy, the Finland Swede Edvard Westermarck, published a book in which he aimed to demonstrate that the sociological and social anthropological assumptions behind Freud's theory of the Oedipus Complex were fallacious.

The Swedish Psychoanalytical Association languished during this period. At the same time new psychoanalysts began to emigrate to Sweden as a result of the German occupation of several countries crucial to the psychoanalytic movement. The most significant of these émigrés was the Dutchman René de Monchy.

Monchy had been in analysis with Ruth Mack Brunswick and during the 1930's received his training in Viennese psychoanalytical circles. During the same period he became personally acquainted with Freud and turned to him for professional advice. Monchy had been a member of the Dutch Psychoanalytical Association since 1922 and was its President for several years (1933-38). Monchy was married to a Swedish Jewess who was among the founders of the Finnish-Swedish Psychoanalytical Association. His relationship with the grand old lady of Swedish psychoanalysis, Alfhild Tamm, was characterised by mutual respect. He spoke Swedish, was well-established in the international psychoanalytical movement and was to play a decisive role in the Swedish Psychoanalytical Association. He was also to become Ola Andersson's analyst.

Family Background

Ola Andersson was born in 1919 in Northern Sweden, where his family owned and farmed land. The family was to live in several rural areas before settling down in Stockholm. The primary reason for their moving to the capital was that Ola's father Carl Andersson (1884-1946) was appointed to a high position with the Office of the Governor of Stockholm. Due to resistance to his progressive ideas, he was forced to leave his position, settled down in southern Sweden and began to work as a school inspector. Ola's mother Jenny Andersson (1889-1945) remained outside the work force. The couple had three children. Ola was the middle child with an older brother (1917-1973) and a younger sister born in 1922 and still living. Ola Andersson mar-

ried in 1953 and his wife was also to basically remain outside the work force. They had two sons.

Andersson studied to be a teacher and received his Master's degree in Swedish and History at the University of Lund in 1943. He was to work as a teacher in one form or another for the remainder of his life.

In the late 50's, at which time the University of Uppsala hosted such scholars as Michel Foucault and Gunnar Brandell, Andersson began work on his doctoral dissertation at its Department of Pedagogy. The Department Chairman was a controversial man named Wilhelm Sjöstrand (1909-1989), who was atypical for the academic world of his day. His interest ranged over an unusually broad spectrum for a Swedish pedagogue of the 50's and included history and clinical psychology. He also had a reputation for taking care of outsiders in the academic world. During this period psychology at Swedish universities was heavily influenced by the American worship of science. Textual and historical scholarship was pushed into the background. Not even Freud was fully accepted as a possible object of study. In the late 50's the prominent Uppsala University professor Konrad Marc-Wogau began to give lectures on the limitations of psychoanalytic theory from the viewpoint of analytic philosophy. Andersson was rowing upstream in relation to his time. Sjöstrand took Andersson in and came to play a central role in his life.

The Post-War Years and Andersson's Road to psychoanalysis

Andersson is reported to have already been interested in psychoanalysis at the age of 20. In 1948, barely 30 years old, he contacted Gunnar Nycander, one of the pioneers of the Swedish Psychoanalytical Association. Nycander referred him to Monchy, who had recently moved to Sweden.

There is much evidence to suggest that the Swedish Psychoanalytical Association's educational program attained a rigor that put it on an international level as a result of Monchy's work. He was strongly motivated to share his analytical experience and knowledge with the analysands and candidates he met in Stockholm. His theoretical interests were closely bound up with his work as a clinician. Andersson underwent his first analysis with Monchy, a treatment that was to last nearly five years, initially seven days a week, including Sunday mornings. This first analysis was terminated when Monchy decided to return to his own country. Andersson chose to continue analysis with one of Monchy's analysands, the Jewish Hungarian psychologist Lajos Székely, who was fifteen years Andersson's senior.

What did Andersson's analytical experience mean to him and what value did he ascribe to the psychoanalytical clinic? We have a few clues. Andersson considered his first analysis to be a valuable experience. For that very reason, Monchy's departure seems to have been a painful ordeal for him. Andersson wrote that he felt abandoned and that emotion was the starting point for his second analysis. That he understood Monchy's explanation for his return to Holland did not make his experience any less painful. From a financial standpoint it would appear that both his own analysis and his later involvement in psychoanalytical scholarship were purchased at a high price. Although Andersson accepted this, his relative poverty no doubt left its mark. He asserted that, in order to support his first analysis, he had sold all his family posessions and used up everything he could manage to borrow. He described his psychoanalytical work as a calling, "something one can devote oneself to without any danger of attaining either riches or success according to the standards of society." At certain times, when doubts about analysis arose, he considered the possibility of going back to his profession as a Swedish and history teacher. But, at the same time he felt that understanding the role of the unconscious in human existence was a tremendous undertaking, "something that is really worth devoting one's life to."

One gets the feeling, nevertheless, that his relationship with Székely, his second analyst, was not without its complications. Andersson entered into psychoanalysis with Székely after having felt abandoned, and furthermore with an analyst who, as a Jewish immigrant, had obtained a position at the University that was far below his level of competence. Andersson embarked on a scholarly project that was supported by a prominent professor, Wilhelm Sjöstrand. His dissertation in 1962 won him the position of Assistant Professor. It is important to recall in this connection that the Swedish psychoanalysts who had their base in the academic world were few in number. For example, in 1967 Andersson was the only member of the Association who had reached the level of assistant professor.

It would therefore seem reasonable to argue that Andersson's own analytical experience left him with ambivalent feelings, and that these were to determine his future direction.

Andersson and Education

It appears from documents in the archives of the Swedish Psychoanalytical Association that it was Monchy who approved Andersson's use of his personal analysis as the basis for a didactic analysis.

In the wake of Monchy's departure from Sweden, Andersson continued his psychoanalytic training and he was a member of the Association, during a period when it experienced a number of conflicts and was threatened with dissolution. During his entire professional life, Andersson demonstrated an aversion to conflict. In close connection with his psychoanalytic training and as a result of the uneasiness within the Association, he was to devote himself to two closely-related activities: historical scholarship at the University and translations of various of Freud's writings.

Andersson and Translators of Freud into Swedish

When one attempts to reconstruct the fate of psychoanalysis in Sweden and the contribution of Ola Andersson, it is essential to understand the history of Swedish translations of Freud's writings. Andersson devoted a good deal of his life, particularly toward the end, to translating Freud. When he was unexpectedly informed of his fatal illness, he was forced to break off a translation career that was proceeding at full steam.

As we noted earlier, there had been difficulties associated with the first Swedish translations of Freud's works. The way in which Bjerre had published Freud was for understandable reasons deeply unsatisfactory for Freud personally. Subsequent developments were not without their complications either. When the Swedish psychologist John Landquist, was instructed by the influential Bonniers Publishing House to pick out a book of Freud for translating, he and Freud began to discuss the matter. Landquist chose an abridged version of *The Interpretation of Dreams*, whereas Freud would have preferred him to translate *Introductory Lectures on Psycho-Analysis* (1915-17).

After the 1939 translation of *Moses and Monotheism*, publication of Freud's writings in Sweden stopped. Some of the difficulties, in terms of resistance, that psycho-analysis had run up against in the Sweden of the 40's were manifested also in the way in which translations of Freud were published. Between 1939 and 1955, for a period of 16 years, no book by Freud was translated. On the other hand, Carl Gustav Jung, and particularly Erich Fromm and Karen Horney, were translated. Swedish society was beginning to be influenced by American culture and that showed up in the translations of Neo-Freudians and works that directly rejected Freud's contribution. In the early 50's Andrew Salter's book *The Case Against Psycho-Analysis* was also translated. A wave of animosity toward Freud rose among a number of prominent doctors during the early 50's, a trend evidenced by an extended debate in the culture section of one of Sweden's leading newspapers.

After the long hiatus, *The Psychopathology of Every Day Life* was re-issued in 1957 under the editorship of Andersson and his colleague Göran Schedin. The following year the two men jointly edited a new translation of *The Interpretation of Dreams*.

In the late 70's, long after the completion of his dissertation, Andersson began a translation of several of Freud's texts, including *Beyond the Pleasure Principle, Group Psychology and the Analysis of the Ego, The Ego and the Id, Inhibitions, Symptoms and Anxiety*, which was published in 1986. Three years later he published a translation of other texts of Freud; *An Autobiographical Study, On the History of the Psychoanalytical Movement*.

Andersson frequently rowed upstream in his choice of projects. He accomplished a major pioneering feat as a translator; he translated Freud faithfully, meticulously and at the expense of great time and effort. In actuality, he produced only two important works of his own, his dissertation and an article in which he related the true circumstances surrounding Freud's patient Emmy von N.

Andersson's Doctoral Dissertation

In December 1962 Andersson published his doctoral dissertation, written in English, which he called *Studies in the Prehistory of Psycho-Analysis: The etiology of psychoneuroses and some related themes in Sigmund Freud's scientific writings and letters 1886-1896*. Chronologically the dissertation ranges from the time when, after having been in Paris and meeting with Charcot, Freud first became interested in the factors leading to psychopathological conditions, to the point when he began employing the concept of psycho-analysis.

In his foreword Andersson wrote that his analysis did not take into consideration biographical or psychological information about Freud. Andersson pointed out that this concentration on the sources is an attempt to avoid interpreting them in terms of the later development of psycho-analysis. The dissertation was, in one sense, a response to Ernest Jones' 3-volume biography of Freud. It was written in terms of the history of science, assuming an internalist perspective and treating the development of psychoanalytic theory as essentially a continuum. He demonstrated that Freud abandoned his early neurological interests after meeting Charcot, became interested in neurasthenia and hysteria, and thus devoted greater attention to psychopathology and therapy. He pointed out, in addition, that Freud, after having taken Charcot's side, had, as a result of his therapeutic experiences, begun to approach Bernheim and employ hypnosis. Freud's first independent study of hysteria, conducted in 1888 dealt with hysterical paralysis. It became evident to him that the anatomico-

physiological explanation advanced by Charcot was inadequate. Andersson insisted that during the years 1890 and 1891 no new information came to light that could assist in the elucidation of Freud's etiological thinking.

The dissertation also set out the way in which Freud's joint work with Breuer from 1892 to 1893 led him to move away from Charcot. Parallel to this development, Freud gradually ceased treating patients by means of hypnosis. Breuer and Freud expressed the view that traumatic hysteria was the prototype of other forms of hysteria.

During the course of 1894, Freud moved away on some decisive points from the approach that he and Breuer had worked out. Under the influence of Herbart's psychology and of Krafft-Ebing, Freud painted a new picture of the emergence of psychoneuroses. For the first time, he emphasised the significance of sexual experiences in relation to obsessional neuroses and phobias. This was also the first occasion on which he employed the concepts of conflict and libido. Freud postulated that a moral conflict arises in connection with the emergence of sexual impulses. In early 1895 Freud developed the concepts of defence and repression. The process of repression became increasingly central and the defence process secondary. The repressed idea was connected to memories of sexual experiences occurring previous to adolescence. The repression of these experiences sowed the seeds of psychoneuroses. It also represented the starting-point for psychoanalytic treatment.

Andersson demonstrated that Freud, in his attempts to explain his clinical observations, developed ideas that were highly reminiscent of Herbart's "Vorstellungsmechanik", i.e. a dynamic interaction of ideas. Freud never explicitly discussed Herbart's influence in the same manner as he had in the case of Charcot. Before Andersson such scholars as Karpinska (1914), Dorer (1932), and Jones (1953) had pointed out the similarities between Herbart's psychology and psycho-analysis. But Andersson was the first person to show that Herbartianism was the dominant psychology in Freud's academic world during the formative years of his scientific development. Andersson also discussed the manner in which Freud, through Lindner's textbook on psychology and philosophy, also came to be influenced by Herbart's psychology. This was pointed out for the first time by Jones (1953). Andersson explored more deeply Lindner's use of the concept of the empirical and historical ego on the one hand and the pure ego on the other. The empirical and historical ego was a designation for the unity of the entire life history of an individual. He defined pure ego as the point of intersection of the various ideas held by a person. Andersson pointed out that at this time Freud used the

term "ego" synonymously with "the person", "the individual" or the "consciousness of an individual". This vague usage, according to Andersson, refers to approximately the same aspect of a person as does the "empirical and historical ego". Andersson pointed out that Freud conceived of the ego as a kind of ideal node at which "Vorstellungsmassen", or idea clusters, met. This approach did not greatly differ from that of the followers of Herbart, who spoke of the "Schauplatz" or dynamic interplay of ideas. Based upon an analogy with Herbartian psychology, the defence activity was conceived of as a repression by consciousness of "weak" ideas by "stronger" ones. At the same time, Andersson was careful to note the differences between Freud's approach and Herbart's. Herbart's conception of strong and weak ideas referred primarily to their interaction according to the thought model of "Vorstellung als Krafte". According to Andersson, Freud determined the strength or weakness of an idea according to its "Affektbetrag" or "Erregungssumme" and these physiological analogies were alien to Herbartianism. These analogies, however, were in keeping with the physiological speculations of J. H. Jacksson and other representatives of the evolutionist tradition.

The dissertation was a meticulously written causal study of the development of Freud's ideas. It represented the first attempt to discover the roots of Freud's early theory from the standpoint of the history of science. Ellenberger judged Andersson's dissertation to be both an objective study based on source materials and an inquiry into the development of Freud's concepts. His dissertation was rich in detail and offered a good overall perspective. He related Freud's shift from a physiological to a psychological emphasis with great competence and skilful use of detail.

Andersson was also able to discover the identity of Freud's famous patient Frau Emmy von N. as well as coming up with new biographical information on her. The case had been published in 1895 in *Studies of Hysteria* The Sigmund Freud Archives charged Andersson with finding out who the person was behind the pseudonym Frau Emmy von N. As early as 1961 he saw a number of indications that he would be able to succeed. His investigation resulted in an article that constituted the basis of a lecture he was to give at the International Psychoanalytical Conference in 1965 in Amsterdam. The lecture was first published in 1979 in the *Scandinavian Psychoanalytical Review*. In his research, he referred to material such as biographies of people who had been involved in some way with Emmy von N. He also interviewed her children and relatives, as well as referring to non-official and private documents.

Andersson elected not to publish this important article until 14 years after the Amsterdam lecture. The results of his research were known only to a few scholars at the time. A few of them, however, availed themselves of Andersson's work and made Frau Emmy von N's actual existence known to the general public. One of these scholars was Ellenberger, who in 1977, i. e. two years before Andersson published the lecture, went public with a longer article.

Andersson's two written works are acknowledged internationally as important reference sources on the historical roots of psychoanalysis. Such scholars as Didier Anzieu, Henri F. Ellenberger, Peter Gay, Nathan G. Hale Jr., Jean Laplanche, Kenneth Levin, Jacques Nassif, J. B. Pontalis, Elisabeth Roudinesco and Maria Torok have called attention to his work.

Andersson and His Institutional Base

Though Andersson was trained by the Swedish Psychoanalytical Association, he devoted but a portion of his professional energy to the work of the Association. He never assumed a significant role in the activities of the Association and sought to be neither a training analyst nor a seminar leader. Despite the existence of other alternatives, he chose to be an associate member instead of a regular member until the late 60's, which would seem to imply that he consciously avoided a position of responsibility within the Association. One document in the archives indicates that he did not feel comfortable at psychoanalytical congresses. However, he was never openly critical of the Association and considered himself to be a part of it.

Andersson submitted his dissertation at a time when the Association was focused on consolidation, with the result that its main activity was the training of psychoanalysts who could work in a clinical setting.

He maintained a virtually life-long connection with the University, and lectured there during various periods of his live , but held no position related to the training of scholars.

There is a great deal to indicate that his main interest was not in the area of clinical work, and his own practice was limited. Nor was he ever employed as a clinical psychologist or psychoanalyst in any national or local government agency. He took another path instead.

Andersson found a haven in the border-zone between philosophical, psychological and religious questions. Stora Sköndal is an institution directed by the State Church of Sweden. In one or another capacity, he was to work there as a teacher from 1947 to 1980. Initially he was Swedish instructor and later the senior master

in psychology. He lived on the outskirts of Stora Sköndal, planned to move there after retiring, and is buried in that area.

Parallel to his work at Stora Sköndal, Andersson worked at another psychotherapeutic institution with a long spiritual tradition, the Saint Lucas Foundation, where he served as supervisor and teacher. For more than 20 years, he was one of the institutions two principal supervisors.

As far as we can tell he had no active religious life in an institutional sense, and he did not present himself as a believer on either a private or professional level, but at many points in his career he had contact with the faithful and was highly regarded by them.

Andersson failed to obtain an institutional position in Sweden where his scholarly contributions and interests could be appreciated. Apparently there was something he was dissatisfied with but never talked about. In a letter to Ellenberger he related that he wanted to try to find an academic position outside of Sweden now that his children were about to leave home. He wondered if there might not be some university in the U. S. that could utilise his expertise and permit him to pursue his research.

Something that recurs in conversations with people who knew Andersson was his close identification with the world of books and ideas. Even as a young child he would show up early for dental appointments in order to take advantage of the opportunity to read *National Geographic* undisturbed. When the library at Stora Sköndal was to be catalogued for the first time in the late 40's, Andersson took on the project. He frequently referred to the Royal Library in Stockholm as the vestibule to paradise.

I have not yet succeeded in identifying one single Swedish scholar who alludes to Andersson's work in his own research, despite the fact that his dissertation was a major pioneering contribution. None of Andersson's analysands have ever made his analytical work known outside the inner circle. In a recently published article relating the history of Swedish psycho-analysis and its current status, Andersson is not named at all. Nor is he noted in the forthcoming authoritative edition of the Swedish National Encyclopaedia. His sons no longer go by their family name and at the memorial grove where his ashes are buried there is no plaque bearing his name. Nevertheless, for those who study the development of psycho-analysis from an historical point of view, Andersson clearly comes into the picture. For those who worked closely with him, he is remembered as a psychoanalyst who shared his expertise with integrity and without pretension.

Per Magnus Johansson

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ABSTRACT

ALFRED BINET: FOUNDATION OF EXPLORATION AND ASSEMENT OF INTELLIGENCE

The group of genial alumni of Théodule RIBOT (1839-1916), in relation to French Psychology, means the stratification and organization in different areas of research and application. The main ones are:

1) Psychopathology, special product of L'Ecole de la Salpêtrière, encouraged by Pierre JANET (1859-1947), in this period.
2) Psychophysiology, with Henri PIERON (1881-1964) and the Laboratory of Psychology at University of the Sorbonne (Laboratoire EPHE) as headquarters.
3) Research and applications of psychological knowledge concerning the childhood, in scholar context, special fruit of Alfred BINET (1857-1912).

BINET played a dominant role in the development of experimental psychology and made fundamental contributions to the adequate methodology for the measurement of intelligence.

Impressed by the attempt of the English psychologist Francis GALTON to record individual differences by means of standardized tests, BINET created the adequate methodology concerning that task: BINET's test was designed so that children of approximately "average ability" could solve adequate "levels" of problems at their own "age level", and most of the tasks at "lower age levels". Thus, a "mental age" was then assigned, and finally the construct of "quotient of intelligence", and the possibility of "counseling" using this data is adjusted ("L'échelle métrique de l'intelligence", 1905). These instruments have been changing but the matter of applied psychology concerning intelligence evaluation are predifined in the work of A. BINET.

We present some graphics used by A. BINET, under the influence of F. GALTON, and the first Spanish translation of his tests, made by José GERMAIN (1944).

We conclude reaffirming the high levels of his research, as the recent AMERICAN PSYCHOLOGICAL ASSOCIATION annual convention (1993) has shown, and this CHEIRON CONFERENCE (september 1994), including BINET among its topics aspects. Finally we dissemble some aspects concerning his work, as M.V. del BARRIO (1993)

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ALFRED BINET: FOUNDATION OF EXPLORATION AND ASSEMENT OF INTELLIGENCE.

The journey of A. BINET. through the academic world. made him an original character. After his studies of Medicine and Law. and not attracted by any of these two branches of knowledge. he took notice of the nascent psychological science during the years 1879-80. Throught readings his the texts of the French and British associationism. he focused his interest towards the theme of "conscience". During this time. he met RIBOT and. fruit of this contact. was the publication in the "Revue Philosophique" of an article on "La vie psychique des microorganismes" (1889) devoted to analyzing the perceptive capacities of protozoans. topic which today. could seem totally absurd but that at the time. knew some treatises (HAECKEL. the work of FECHNER "The soul of the plants". or F. DARWIN. son of Charles. who attributed perceptive capacities and consciounsness to those. Perhaps the establishment by Jacques LOEB of the concept of "tropisme". even in 1889. helped to dilucidate this scientific debate).

RIBOT would advise BINET to return to scientific works sidelined after concluding his career of Medicine. and one of the friends he counted on this context. Joseph BABINSKY. introduced him to the group of CHARCOT at the Salpêtrière (MORA. 1979). Fruit of this time would be his work "Contribution a l'étude du système nerveux sous intestinal des insectes".

A casual encounter with Henri BEAUNIS. one of the physiologists of the school of Nancy. served him to know the existence of the Laboratory of Psychology of the Sorbonne (laboratoire EPHE). Subsequently. in 1892. BINET would be appointed attached director of this laboratory and. when BEAUNIS retired in 1894. Alfred BINET would find himself like spick-and-span as director of this institution.

A greatly very important page in BINET's scientific life was constituted by his participation in the foundation of the Magazine "L' Année Psychologique". together with Victor HENRI. who contrary to most French psychologists of this historical period. had been formed with WUNDT at Leipzig. Following the foundation of this Magazine (1895). French Psychology would have an organ of

scientific expression of foremost magnitude, and the evolution of this Magazine may be used as reference for the development of this discipline in France.

The original contribution of Alfred BINET during his years in the Laboratory could be considered his conception of intelligence, opposite to the ideas of the atomism of this time, such as, H. TAINE, so admired by RIBOT. For TAINE intelligence was a collection of independent images, ideas which BINET could not hold. He would accept, like many GERMAN psychologists of this period, the existence of "thought without images" and was more interested in explaning intelligence as a whole which appears, under the form of tendencies and general schemes, which show the diverse successful tasks that human subjects solve.

As methodology for his experimental work, Alfred BINET used initialy to solicit from the subject the description of the mental states that the question provoked (the resource of "experimental introspection", already present in WUNDT and the first GERMAN psychologists), where it was sharply revealed that thought is much more than the mere succession of images or their combination. As it is well-known, in this historic period, the same method was utilized by the School of Würtzbourg, fundamentally by O. KULPE (MORA, 1987)

After a time centered on theoretical questions, Alfred BINET would devote and demonstrate an interest for "practical or applied" questions, being perhaps the infantile context that officially attracted him.

Is extremely complex to attribute to one cause this change of orientation. What is sure is that the birth and posterior evolution of his two daughters made him take infinity of notes on the most interesting aspects reoarding them as DARWIN and PREYER had already done before him, and many subsequently psychologists like PIAGET and LEWIN would to mention just a few. Another reason is going to be, undoubtedly, the social demands surge in France during this historical period, by the affluence of masses of children to different school institutions, as it happened in other european countries, fruit not only of the increase in birth rate but also of the gradual implantation of obligatory scholarization in several countries of the continent.

The fact is that Alfred BINET abandoned his theoretical studies on intelligence, most of them carried out with Victor HENRI, and would converge to the Laboratory just on Thursdays, practically abandoning the basic research that he had been carrying out with regard to intelligence.

In 1899 takes place his encounter with Theodore SIMON, a doctor at the Hospice of Perray-Vaucluse for retarded boys, which would marks the definitive road in his contributions to the history of psychology. From here on, BINET is going to enter in to a society peopled by teachers and administrator of public schools, Free Society for the Psychological Study of the Boy, all moved mainly by desire to solve the problems come on French schools by the massive affluence of students in this periode.

The intellectual novelty of Alfred BINET consists in opening the recent psychological knowledge to the pedagogical world, focusing on the practical resolution of the real problems which teachers tripped upon at the time.

The Ministry of Public Instruction created a commission in charge of making an instrument that would be allow the differentiation of the boys that could learn normally from those that couldn't. In order to solve this practical problem common at the beginnings of all schools years in all latitudes of the earth, in the past as well as in present time, they tried to find physical indexes of intellectual differences: cefalometric studies, graphology, antropometric index all type of corporal signs that could be used as propouns of the invisible intellectual capacities. Some of these indexes would not prove useful in order to effect the students' adequate classification.

BINET attempted then new roads that could answer the problem faced: He moved under the principle of "individuals are that which they do". Fruit of these proceeding would be the publication of "L'échelle métrique de l'intelligence" (1903), together with SIMON. After the success of this scale, BINET would found a new laboratory, whih an specially pedagogical orientation, in a primary school of Paris, and would rapidly publish "Les idées modernes sur les enfants", work destined to parents and teachers, as they are responsible of the correct application of the heap of knowledge that was accumulating from the new psychological science.

BINET would introduce a list of factors, some already tried by the previous French psychology (ESQUIROL, 1838, specially based on verbal capacity; SEGUIN, 1866, inserting cards on boards of similar shape), widening it fundamentally to school tasks, such as attention, memory and diverse facets of intelligence (logical, verbal, numeric, inventive, etc.). The system of the subject's answer

was, in the majority of the subtests, verbal.

These instruments, whose measurements would any contemporary psychologist question, have the great merit of having been the first used and have provoked, starting from their establishment, respect and practical efficacy to nascent psychological science. They are in great part responsible for the immediate peak of psicometrics and the psychologists' initium to elaborate "profiles" and to "guide" in schools and professional institutions, starting from the data gotten with these types of instruments.

The statistical engine of BINET was initially simple enough, similar to the professor of any course that prepares an exam and demands, in order to pass that, the student elaborates correctly 50% of the questions. The novelty is to refer these results to their chronological age, so we have already established the comparison between "mental age" and "cronological age".

Subsequently, in 1916, Lewis TERMAN (1877-1956) at the University of Stanford would produce the first, and completely accepted by the psychological North American community, version of the tests of BINET in USA. TERMAN would adopt the concept of "quotient of intelligence" (mental age /cronological age x taken from Wilhelm STERN, one of the most well-known German psychologists in this historic period. Similarly L. TERMAN would increase from 54 to 90 the number of elements each one of the subtests that contained. The whole and heterogeneous results of materials replete of school contamination was called "general intelligence", conceived as he adition of the capacities of thought implicated in mental adaptation.

The earlier work would be revised, enlarged and sophisticated by L. TERMAN with the colaboration of Maud A. MERRILL ("The measurement of intelligence", 1937) also incorporating the experience accumulated in the use of the test of BINET for two decades, by psychologists, not only American and French, but also English and from other latitudes. In this revision, they would incorporate tasks that included analogies, oppositions, understanding, similarities and differences, besides the classical of vocabulary, already present in the nominative tasks by BINET. Similarly they would incorporate phrases and incomplete drawings and diverse types of memory, especially numeric. After all, the tests of BINET were fundamentally verbal and needed to complete them with other types of tasks.

The original tests of BINET would still know a third revision on the other side of the Atlantic, the STANFORD-BINET Form L-M (1960), completed in this case with manual labours (cards, board holed for join figures, blocks, bills, small toys, scissors, glass, eating utensils, etc.) tasks already very diverse

from the initially elect by BINET in order to explore the construct "intelligence". Any psychologist familiar in managing these instruments could affirm that the apparition of the first edition of WECHSLER's tests (Intelligence Scale for Children. 1949) could be taken as the responsible influence of the necessity of these adaptations.

The last version of the scale of STANFORD-BINET came out to public light in 1973. but from the beginning of the work of WECHSLER they went incrementing the critics to the same. being especially virulent in the 70th (BUROS. 1972).

However. as recuperation of the historical memory. in the annual convention of the AMERICAN PSYCHOLOGICAL ASSOCIATION. August of 1.993. was introduced a facsimile edition of the works of this great pioneer. Alfred BINET. a sign that. beyond the criticism that could legitimately be made. is a point of reference and posterior generations should feel indebted.

What remains of the work of this great pioneer?. The influence of the work of BINET could consider important in Psychology of the Intelligence while the boost of the factorial tradition remained. When that model entered in crisis. it is normal feels of effect the appreciation toward the author that began that type of studies diminished.

On the other hand. through the years the conception of intelligence changed and naturally connected to it same. the manner of it's evaluation.

Concerning the second of the questions. the gradual appearance of the works of WECHSLER displaced in popularity the old tests of BINET. David WECHSLER considered that the scales of STANFORD-BINET were too guided toward the boys. even after adding element more and more dificult directed to the adults. To this respect he wrote. "to request from a former sergeant to construct a phrase with the words "boy". "river" and "ball" is not specially adequate for awakening either his interest nor his respect" (1939. pag 17).

Initially WECHSLER developed his test for adults "Wechsler Adult Inteligence Scale" (WAIS) which has known diverse revisions (1939, 1955, 1981). Besides the verbal subescales (information, understanding, arithmetic, likeness, digits and vocabulary), similar some of them to those already employed by BINET, it incorporated five manual manipulative scales (key of numbers, incomplete figures, blocks, short stories and puzzles). Subsequently he developed his tests for boys in school age, "Wechsler Intelligence Scale for Children-Revised" (WISC-R), constructed in 1958 and revised in 1974. The novelty, as it happened before, is the distinction between verbal and manual scales. The verbal scale is so similar to STANFORD-BINET, and its output as for the assignment of CI so similar, that the correlation between the punctuations that subjects get with the two scales is very high, as was pointed out by WECSLER himself in his publications. The manual scales is the great novelty over the tasks advanced by BINET, so that the revision of their scales, made by TERMAN and MERRILL, consisted in incorporating this type of tasks. Finally WECHSLER constructed his "Wechsler Preescolar Scales of Intelligence" (WPPSI), carried out in 1967, when already all the factorial building and the tests of CI in general, were going to begin to be critiziced by some of the new psychological currents. As both schools moved within classical punctuations of CI, without their manufacturers managing to expound it, they are going to meet combined attacks, although some put forward their scales as an overcoming of the anterior.

Concerning the global framework of the theories of intelligence, we have already had occasion of showing that the most important of the theoretical modifications has been the incorporation of the works on intelligence within the wide frame of "cognition", taking a more opening margin of question in relation to the same (MORA, 1990) The emergence of the paradigm of information processing, have given to the psychology of the intelligence one frame of much more rich context that the mere scale of "factors" or "aptitudes" to evaluate.

What evaluation could be made of the work of Alfred BINET from this final of the XXth century? If we deal with locating what contributions continue being valid in contemporary psychology, we would highlight at least the following ones:

First: His decision that physiology cannot be taken as the only intelligence indicator. This is a line of research that decade after decade appears as a try at solving the problem of intelligence, but up to the present its results are quite dissapointing (YELA, 1991). At the same crossroads was Alfred BINET and he preferred to opt, by being more congruente with the fact, for designing "tasks" that should be solved by the subjects

Second: To Take as measurement of intelligence that which "we do", measured externally, constructing the first scales dedicated to its study. Undoubtledly today we don't do it with the same tasks but the mental disposition to proceed in the study of intelligence is still perfectly valid.

Neither the list of tasks, nor the intellectual underpinnings that allow their construction outlook are going to be the same ones. It certainly is the "functional" that we give to the nominative tasks, as indicator of an "internal" capacity that we measure "externally".

Third: Statistical techniques have evolved, but with them we do similar tasks (pick-up data concerning a survey, apply them for a better knowledge of a population, employ them in the manner of a "ready reckoner" in order to locate the different subjects, to elaborate a profile of the analyzed subject, etc.)

Fourth: The data obtained with these procedures, through their statistical fiability, are converted in legitimate instruments for counseling, advising, classifying, etc. the different subjects that are subjugated to them. And, however blamble could any of these systems of classification be bigger criticism deserves, undoubted, the sheer subjetivism exercised by those who exercises control, in order to take decisions.

These four elements were already present in the brillant work of Alfred BINET, for which he had the honor of opening one of the roads we understand at the present time as the scientific and social paper of the psychologist. For all this we consider that it is legitimate to define him as the "institucionaltizer" of the studies on intelligence, as he let the way mark, although with their diferencial tints, that has continued research and posterior exploration of this parcel of the psychology, in its theoretical as well as in its applied aspects.

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Combat scientifique-combat politique : les sociologues durkheimiens dans l'Affaire Dreyfus

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L'Affaire Dreyfus est depuis longtemps considérée comme un événement capital dans l'histoire des idées politiques françaises. C'est un moment décisif dans l'évolution du socialisme qui était traditionnellement antisémite et anti-bourgeois mais qui va pourtant (sous l'influence notable de Jean Jaurès) rallier le camp des dreyfusards; un moment également décisif d'enracinement du nationalisme qui devient le symbole des valeurs traditionnels. du patriotisme. Bref : c'est un moment de réorganisation générale des idées politiques. Et lorsqu'on parle d'idées politiques, il faut voir la lourdeur des enjeux de l'époque et comprendre l'intensité psychique d'un événement qui a passionné les esprits au point de provoquer la naissance de journaux et la formation de ligues, de susciter des manifestations dont certaines faillirent dégénérer en coup d'Etat, d'exacerber des haines et des rancunes qui trouveront encore des échos trente ans plus tard. Sur fond d'angoisse décadentiste et de crise économique grave, la société française a donc été particulièrement agitée pendant ces années de fin de siècle.

Face à cet enjeu, la majorité des jeunes intellectuels se sont engagés dans le dreyfusisme, certains ont été très loin dans le militantisme. Parmi ces intellectuels, ceux qui gravitaient autour de l'Ecole Normale Supérieure ont été particulièrement dreyfusards (on connait le rôle de Lucien Herr, le bibliothécaire). Et au sein de ce milieu normalien, l'équipe de jeunes agrégés récemment constituée autour d'Emile Durkheim pour donner à la sociologie une organisation intellectuelle nouvelle s'avère avoir été l'un des pôles les plus nettement impliqués.

Qu'est-ce qui détermina une telle intensité dans l'engagement dreyfusard et quelles en furent les conséquences dans le champ des sciences humaines ? Ce sont les questions auxquelles nous tenterons de répondre en montrant que le rejet fondamental de la réduction biologique des comportements humains était une donnée de base du durkheimisme et qu'il pourra donc donner un sens à la fois politique et scientifique à l'engagement de ces sociologues (I); en rappelant ensuite comment les tensions morales et économiques de la fin du siècle ont provoqué l'exacerbation du racisme et de l'antisémitisme au moment même où toute l'anthropologie et une bonne part de la psychologie expliquaient l'intégralité de l'homme et de son histoire par les lois de la physiologie et théorisaient notamment l'inégalité des races (III); en racontant ensuite les combats dreyfusards des durkheimiens et en tentant d'évaluer leur impact (III).

I. La cohérence d'un projet intellectuel et d'un engagement politique

Le fait est là, nous l'avons longuement argumenté ailleurs (Mucchielli 1994a et b, 1995a et b) : toute la sociologie durkheimienne tend à rejeter dans son principe l'explication biologique des comportements humains. Mais ce rejet (comme toute innovation) n'est pas un commencement absolu, il a été préparé par toute une réflexion philosophique et morale liée notamment à la réception de Spencer et au grand débat sur la morale évolutionniste qui a lieu en France dans les années 1875-1885 (Conry 1974, Clark 1984). Face au fameux "Struggle for life" de Spencer, les savants français ont généralement promu une éthique de la solidarité :

- les biologistes, préférant souvent Lamarck à Darwin, se sont attachés à montrer ce que l'évolution devait non pas à la sélection mais au contraire à l'association, à la coopération, à l'organisation collective de l'existence (Roger 1979, La Vergata 1992).

- les philosophes (Fouillée, Renouvier, Marion, Guyau, Janet, Caro, etc) ont répondu d'une part sur un plan intellectuel en défendant sinon le "règne humain" des spiritualistes, du moins le propre de l'homme, sa capacité à agir sur sa propre évolution; d'autre part sur un plan politique par une réflexion sur l'idéal républicain et sur l'idéal socialiste, sur la cohésion sociale et la solidarité, sur la possibilité de concilier l'individualisme et la régulation étatique des conflits sociaux (Hayward 1959 a et b, Cochard 1992, Dubois 1985).

C'est dans ce contexte intellectuel qu'il faut comprendre les fondements de la sociologie durkheimienne qui s'expriment ici dans deux réfutations et une proposition : 1/ la sociologie ne peut exister que si la biologie n'explique pas tout l'homme; 2/ la race et l'hérédité ne déterminent plus chez l'homme que des aptitudes assez vagues; 3/ l'évolution se caractérise par l'importance croissante du facteur psychique et, chez les humains, ce sont les croyances collectives qui fondent le lien social.

II. La politisation de l'idée de race dans l'angoisse décadentiste de la fin du siècle

La fin du XIX° siècle a hérité dans les manuels d'histoire de l'étrange appellation de "Belle époque", symbolisée par Paris "ville lumière", ville de la grande exposition universelle de 1889 où on inaugure la Tour Eiffel. Cette image construite a posteriori fausse complètement la réalité de cette fin de siècle où les innovations techniques décisives s'accompagne en fait d'une profonde angoisse devant la perte des repères traditionnels et la crise économique à partir de 1880. Il en résulte une terrible montée de l'antisémitisme qui était toutefois une composante profonde de l'imaginaire politique français mais se trouve exacerbé. Après 1885, les choses s'accélèrent, surtout à partir de ce coup de tonnerre qu'est la parution des deux gros volumes de *La France juive* d'Edouart Drumont. A travers le mythe juif, il explique à la fois la dégénérescence de la race, la corruption du régime, la crise économique, l'exploitation du peuple par le capitalisme, le déclin des folklores et des traditions (Angenot 1989, Winock 1990). Enfin Drumont aura toujours soin, et cela nous intéresse particulièrement ici, de donner à son antisémitisme une allure scientifique.

En 1886, se réclamant de Renan, il systématise le couple Aryen/Sémite pour en faire la clef d'une explication méta-historique. Ce sont bien deux races dont les caractères sont constants depuis la nuit des temps et que tout oppose, tant le caractère que la physionomie et l'anatomie cérébrale : "le juif n'a pas le cerveau fait comme nous". Fort du succès de ses livres et du journal La libre parole qu'il lance en 1892, Drumont est donc le principal prophète de l'antisémitisme français. Un seul homme peut, dans ses années 1890-1900, rivaliser avec lui sur le terrain de l'antisémitisme littéraire et politique : Maurice Barrés que l'influence décisive de Jules Soury (professeur de psychologie physiologique à l'Ecole des Hautes Etudes de la Sorbonne) amène vers un antisémitisme physiologique et racial (Sternhell 1972). Pour eux, la race juive formait bien, selon la formule de Renan, "une combinaison inférieure de l'espèce humaine" dont Soury dressait le portrait physiologique en terme de dégénérescence et d'atavisme. Barrès retiendra ces leçons et leur donnera par son éloquence personnelle une portée terrible. En effet, de cette croyance en la toute-puissance des races, il fera découler ces deux conséquences : 1/ la culpabilité de Dreyfus n'a pas besoin d'être prouvée car, au fond, "elle se déduit de sa race"; 2/ l'universalisme de la Déclaration des Droits de l'Homme est une illusion ("il n'y a de justice qu'à l'intérieur d'une même espèce") car les valeurs morales sont propres à chaque race; en l'espèce le cas de Dreyfus ressort moins de la justice française que d'une "chaire d'ethnologie comparée" (cité par Sternhell 1977).

Les cautions scientifiques du racisme et de l'antisémitisme

En réalité, toute l'anthropologie depuis le début du XIXème est une longue apologie de la notion de race, de la causalité directe du physique au moral, du déterminisme physiologique des comportements humains les plus complexes (Blanckaert 1985, 1989 a et b. Mucchielli 1994c). De même la nouvelle psychologie incarnée par Taine, Ribot et leur modèle anglais Spencer explique elle aussi la totalité des manifestations psychiques par un déterminisme physiologique plus ou moins simple et croit elle aussi à l'hérédité des dispositions normales ou morbides. Mais avec des hommes comme Jules Soury, Gustave Le Bon et Georges Vacher de Lapouge, la systématisation et la vulgarisation des thèmes racialistes et héréditaristes vont atteindre des sommets étonnants. Les tirages d'imprimerie du docteur Gustave Le Bon rivaliseront presque avec ceux de Drumont et en 1894 il livre la synthèse de ses théories héréditaristes et raciales sur l'histoire et la psychologie des peuples (Le Bon 1894). Cependant, les durkheimiens n'ont guère polémiqué avec un penseur solitaire fréquentant essentiellement les milieux médicaux, ne revendiquant pas le titre de sociologue et qui saura éviter le discrédit politique que ses idées auraient pu lui valoir. Tel ne fut pas le cas de Vacher de Lapouge et sa nouvelle "anthropo-sociologie" sur laquelle nous devons concentrer davantage notre attention.

Georges Vacher de Lapouge (1854-1936) conçoit au début des années 1880 le projet d'une nouvelle science sociale entièrement fondée sur les lois de l'hérédité. Dès ses premières publications, Vacher de Lapouge pose le principe de l'explication biologique intégrale des faits humains et la nécessité d'en tirer les conséquences politiques (Taguieff 1989-90 et 1991). En effet Darwin, prolongé par Galton, a introduit une révolution scientifique grâce aux connaissances des

lois de l'hérédité. Ces lois qui prouvent l'inégalité des hommes permettent d'élaborer une politique enfin scientifique, débarrassée des spéculations des philosophes. Et cette politique c'est celle de la sélection des êtres supérieurs - les "eugéniques" - dont le calcul de l'indice céphalique révèle l'identité. Cette sélection est urgente car la démocratie endigue les effets de la sélection naturelle en améliorant le sort des êtres inférieurs qui, par conséquent se reproduisent davantage tandis que les meilleurs font de moins en moins d'enfants. Enfin, au sommet de la pyramide des races humaines trône l'Aryen, "blond aux yeux bleus, grand et puissant, le teint clair et fleuri, le visage et le nez étroits, le corps et les membres longs (...). Il est turbulent, égalitaire, entreprenant et ses facultés vont jusqu'au talent lorsqu'il est intelligent" (Vacher de Lapouge 1893). Certes "dans chaque race il y a des individus qui s'écartent plus ou moins du type à quelques points de vue, mais les caractères typiques de la race prise en masse donnent une moyenne invariable. Avec une impérieuse nécessité, les lois de l'hérédité condamnent chaque homme à être ce que veut sa naissance". En effet, "l'étendue de l'hérédité est aussi universelle et sa force aussi irrésistible que celle de la pesanteur". On sent bien ici, comme chez Le Bon, la conception quasi-religieuse - en tous cas substantialiste - de l'hérédité (Béjin 1982).

III. Le combat anti-raciste des durkheimiens au moment de l'Affaire Dreyfus

Les derniers mois de l'année 1897 et les premiers de 1898 marquent véritablement le début de l'Affaire Dreyfus. Fin 1897 ont lieu les conversions décisives dans le champ intellectuel : celle de Zola qui signe ses premiers articles soutenant les révisionnistes, celle de Lucien Herr qui commence à convertir et enrôler les jeunes normaliens, celle de Jaurès qui assure l'engagement du socialisme des intellectuels (Blum 1983, Lindenberg et Meyer 1977). Début 1898 les choses s'accélèrent. Le 13 janvier Zola lance son fameux "J'accuse" et dès le lendemain L'Aurore commence la publication des listes des noms qui protestent avec Zola. Et puis c'est au cours du premier procès Zola, en février, qu'est fondée la Ligue des Droits de l'Homme et du Citoyen.

On le sait bien aujourd'hui, le dreyfusisme a été l'expérience politique déterminante de toute une nouvelle élite intellectuelle formé dans les années 1890-1900 autour de l'Ecole Normale Supérieure, bénéficiant des réformes universitaires de la fin du siècle (Charle 1990). Or les sociologues durkheimiens font totalement partie de ce mouvement. De tous les groupes scientifiques de l'époque, c'est l'un des rares dont on peut dire qu'il fut tout entier résolument dreyfusard. Dès la première liste de protestation, puis à la Ligue des Droits de l'Homme, enfin dans les réseaux socialistes qu'organise Lucien Herr à Paris, autour de l'ENS, on les retrouve tous (Simiand, Fauconnet, Halbwachs, Mauss, etc). De même, à la Revue de métaphysique et de morale , le trio Bouglé-Lapie-Parodi se démène. Bouglé entame une série de conférences. A Bordeaux enfin, sa correspondance le révèle, Durkheim sera dès le départ le principal organisateur de la section de la Ligue des Droits de l'Homme et du Citoyen.

C'est dans ce contexte politique qu'il faut situer le rejet de la notion de race et notamment la polémique avec Lapouge.

Durkheim et Bouglé contre le racisme

Dans *Le suicide*, en 1897, Durkheim consacre un chapitre entier à la réfutation du poids de la race dans l'explication du suicide. Il note que les peuples s'étant tellement mélangés depuis la nuit des temps, on voit mal ce qui pourrait encore les singulariser et encore moins induire quoi que ce soit sur leurs comportements sociaux : "un type humain que l'on constitue uniquement à l'aide de quelques renseignements, souvent indécis, sur la grandeur de la taille et la forme du crâne, n'a pas assez de consistance ni de détermination pour qu'on puisse lui attribuer une grande influence sur la marche des phénomènes sociaux" et, en fin de compte, "chacun en parle un peu à sa manière sans grande rigueur scientifique". Dès lors Durkheim prévient ses troupes : "ces observations préliminaires nous avertissent que le sociologue ne saurait être trop circonspect quand il entreprend de chercher l'influence des races sur un phénomène social quel qu'il soit. Car, pour pouvoir résoudre de tels problèmes, encore faudrait-il savoir quelles sont les différentes races et comment elles se reconnaissent les unes des autres. Cette réserve est d'autant plus nécessaire que cette incertitude de l'anthropologie pourrait bien être due à ce fait que le mot de race ne correspond plus actuellement à rien de défini". A ces arguments de principe, Durkheim ajoute une vérification statistique : il ne peut pas être constitutif de la race allemande, par exemple, de se suicider davantage puisque les émigrés allemands en Autriche se comportent différemment. C'est donc bien le milieu qui détermine le comportement.

A partir de 1897, Célestin Bouglé va se poser en adversaire constant de l'anthropologie racialiste et de ses théories inégalitaires. Pour lui la nouvelle anthroposociologie, comme l'ancienne phrénologie de Gall, postule sans aucun fondement un lien de causalité direct entre une donnée physiologique (en l'occurrence la conformation des crânes) et une donnée psycho-sociologique. Bouglé ne conteste nullement que les individus aient à la naissance, du fait de l'hérédité, des facultés inégales : c'est une évidence qu'il n'était point besoin de montrer avec tant d'acharnement. Mais le rôle de la nature s'arrête là. Au-delà c'est dans la société que s'élaborent les principes qui organisent l'action de ces individus. Le Droit relève donc de l'idéal : "S'il est vrai que, en déclarant les hommes égaux, nous portons un jugement non sur la façon dont les a faits la nature, mais sur la façon dont la société doit les traiter, les crâniométries les plus précises ne sauraient nous donner ni tort ni raison. En croyant qu'il appartient à des observations scientifiques de juger, en dernier ressort, de la valeur de cette idée pratique, l'anthropologie oublie que les questions sociales ne sont pas seulement 'questions de faits' mais encore et surtout 'questions de principes'" (Bouglé 1897).

A ces deux objections méthodologiques, Bouglé ajoute des critiques qui ressortent de l'examen attentif des faits biologiques. Il y consacre un livre entier très documenté où il entre vraiment dans les discussions et montre les contradictions de la soi-disante "loi de brachycéphalisation croissante". Après Espinas, Perrier et Durkheim, il conteste que la concurrence soit le seul moteur de l'évolution. Pour autant il ne cherche pas à tout prix à démontrer le contraire, il constate

simplement que la solution n'est pas univoque et que cette équivocité-même fonde le rôle de l'éthique : "ce qui se dégage de plus net de nos recherches sur les leçons de la biologie, c'est l'extrême difficulté où est l'homme de "laisser parler la nature" pour enregistrer son conseil : la conseillère parle plusieurs langages et varie les réponses suivant les idées préconçues des enquêteurs. Il reste qu'en attirant l'attention sur la multiplicité des sens ou des modes de l'évolution organique et en limitant la vérité du darwinisme, nous avons libéré notre idéal des prophéties fatalistes dont on le poursuivait" (Bouglé 1904).

L'Année sociologique et l'anthroposociologie

En ce qui concerne l'*Année sociologique*, les faits sont simples : les trois premiers volumes contiennent une rubrique anthro-sociologie dont la rédaction est confiée à Henri Muffang (1864-?), disciple de Lapouge, qui y présentait les travaux français et étrangers se rattachant à cette doctrine. Toutefois, dès le premier volume, la rubrique s'ouvre sur un avertissement de Durkheim aux lecteurs : "Il a pu sembler parfois que l'anthroposociologie tendait à rendre inutile la sociologie. En essayant d'expliquer les phénomènes historiques par la seule vertu des races, elle paraissait traiter les faits sociaux comme des épiphénomènes sans vie propre et sans action spécifique. De telles tendances étaient bien faites pour éveiller la défiance des sociologues. Mais l'Année sociologique a, avant tout, pour devoir de présenter aux lecteurs un tableau complet de tous les courants qui se font jour dans tous les domaines de la sociologie" (Durkheim 1898).

Trois ans plus tard, la rubrique disparait mais non sans frais pour les anthropologues qui reçurent une volée de bois vert dans la première partie de l'Année sociologique (la "Sociologie générale"), où Hubert et Mauss interviennent exceptionnellement dans une très critique rubrique "Anthropologie et sociologie". Jean Deniker (1900) y est d'abord loué par Mauss pour avoir établi que "les idées de races, d'espèces, de variétés, ne comportent pas dans l'étude du genre homo la même acception que dans le reste des études zoologiques. (...) si ces caractères (morphologiques) permettent de classer les races et de faire peut-être quelques hypothèses sur leurs généalogies, ils ne peuvent en aucune façon servir à constituer une science complète de l'humanité. Même pour constituer une théorie des races humaines, les caractères somatiques sont encore insuffisants. Chaque groupe a plusieurs caractères divers, et tout ce que l'on peut saisir, ce sont des rapports, des ensembles de caractères (...). Il n'existe pas, en effet, de groupe géographiquement isolé, ni de race pure de tout croisement; sur les confins il y a toujours des types de transition" (Mauss 1899-1900a). La suite des critiques fait intervenir un partenaire inespéré pour les durkheimiens.

Avant et après Manouvrier

Léonce Manouvrier (1850-1927) est alors professeur d'anthropologie physiologique à l'Ecole d'anthropologie de Paris. Dernier élève de Broca, il est l'un des espoirs de sa génération. Mais il est surtout le premier, sinon à avoir compris, du moins à avoir reconnu par principe et dès le début que l'anthropologie ne pouvait plus prétendre rendre intégralement compte de l'Homme par l'étude de sa constitution biologique. C'est pourquoi il fut d'abord un adversaire résolu de

Lombroso (Robert, Lascoumes et Kaluszunski 1986, Blanckaert 1994, Renneville 1994). Son opposition aux théories de Vacher de Lapouge se situe donc dans la continuité de sa pensée. En 1899, Manouvrier intervient violemment pour établir "la nullité scientifique" de ces thèses "où s'étale un simplisme effrayant" (Manouvrier 1899). Contre cela, mais aussi, quelque part, contre le paradigme qui fonda toute l'anthropologie depuis le début du siècle, Manouvrier se fait le défenseur d'une sociologie en charge de rendre compte d'un niveau de complexité des phénomènes humains qui échappe à l'anthropologie : "il faut se garder de croire que la biologie soit capable de fournir l'explication immédiate des phénomènes sociaux". Quant à ces notions de race et d'hérédité, il explique que "les aptitudes transmissibles héréditairement sont des aptitudes purement physiologiques et élémentaires (...) ne permettant en aucune façon de prévoir les actes qui seront accomplis". Il faut désacraliser ces notions auxquelles le sens commun donne une valeur explicative que la science doit rejeter : "en réalité l'explication par la voie du sang n'a pas eu besoin de devenir elle-même une voix de sang pour être facilement acceptée. Elle a tout simplement l'avantage d'être simple et de satisfaire l'esprit sans effort". Enfin Manouvrier en vient à contester l'usage de l'indice céphalique dans la nouvelle théorie anthropo-sociologique des races (qu'il voue au même discrédit que la phrénologie de Gall et l'angle facial de Camper). En effet, un usage dévoyé des mots "brachycéphalie" et "dolichocéphalie" se répand dangereusement dans le public qui finit par croire "qu'il existerait une race supérieure entres toutes et qu'elle serait dolichocéphale et blonde" . Or rien n'est plus invérifié, rappelle Manouvrier en dénonçant "les élucubrations d'une phrénologie nouvelle", d'une mauvaise "aryanologie" et de sa "sinistre prédiction" . Il conclut sans détour : "c'est de la pseudo-science".

Ce long texte de Manouvrier ne passa pas inaperçu à l'époque et les durkheimiens en firent immédiatement un très bon usage. Par la suite, leur critique s'exerça encore dans les années 1901-1905 mais à partir de 1907, les choses étaient calmées. Lapouge semblaient à présent totalement isolé. Et si la notion de race et de déterminisme héréditaire recule un peu de l'horizon intellectuel général, nos sociologues y sont pour quelque chose. C'est là une belle victoire dans une bataille qui était engagée depuis longtemps mais qui n'aurait certainement pas été gagné aussi définitivement sans le concours des événements historiques. En l'occurence les durkheimiens se battaient contre le réductionnisme biologique, l'inégalitarisme et le racisme qui en découlaient. L'Affaire Dreyfus leur fit gagner une bataille jusqu'ici perdue devant la force des préjugés évolutionnistes occidentaux les mieux ancrés. C'est un peu comme si, sur ce problème, l'histoire leur avait donné raison.

FULL PAPER

L'ÉTUDE DES MÉMOIRES EXTRAORDINAIRES

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Running head: Memory and Alfred Binet.

INTRODUCTION

Si Alfred Binet (pour une biographie: Wolf, 1973; Varon, 1935) passe en général pour l'homme des tests d'intelligence, l'oeuvre de ce psychologue est remarquable sur bon nombre de sujets (cf., Siegler, 1992), l'étude de la mémoire chez les experts en calcul mental et au jeu d'échecs en est un exemple frappant (1).

Pendant plusieurs années, Binet poursuivit des recherches sur les diverses formes de mémoire, avec l'idée directrice que ces recherches pourraient être de quelque utilité pour la pédagogie. Pour Binet, il n'existe que des mémoires partielles, spéciales, locales, dont chacune a son domaine propre, et qui possèdent une indépendance telle, que l'une de ces mémoires peut s'affaiblir, disparaître, ou au contraire se développer à l'excès, sans que les autres présentent nécessairement une modification correspondante (2). Si Binet n'a pas utilisé les données de la pathologie pour démontrer l'existence de ces mémoires partielles, influencé par les écrits du philosophe Hippolyte Taine (1828-1893), il choisit l'étude de sujets présentant un développement hypertrofié de l'une de ces mémoires. Il a réuni en 1894 le résultat de ses travaux (Binet et Henneguy, 1894) dans un ouvrage intitulé "Psychologie des grands calculateurs et joueurs d'échecs". Cet ouvrage traite en fait spécifiquement de l'importance de la mémoire dans le calcul et aux échecs.

1 - LES GRANDS CALCULATEURS

Binet a fait pendant près de deux ans l'étude de deux calculateurs mentaux: Jacques Inaudi et Périclès Diamandi. L'analyse approfondie de la mémoire de ces deux personnages constitue la première étude psychologique des calculateurs mentaux (Brown et Deffenbacher, 1975). Quelles sont les circonstances qui ont conduit Binet à étudier ces deux calculateurs de profession ?

Le 8 février 1892, le mathématicien Gaston Darboux (1842-1917) présenta à une séance de l'Académie des Sciences un jeune homme de 24 ans, petit et robuste, appelé Jacques Inaudi. Ce personnage était connu à l'époque comme un grand calculateur mental de profession. Né en 1867 à Onorato dans le Piémont (Italie) d'une famille pauvre, Jacques Inaudi commença à calculer vers l'âge de 6 ans, en gardant des moutons. Il ne comptait ni sur ses doigts ni avec des cailloux, mais opérait de tête sur les nombres dont son frère lui avait appris les noms (ce n'est qu'à l'âge de 20 ans qu'il sut lire et écrire). Après

une vie mouvementée dans le Sud-Est de la France, il fut découvert par un impresario, M. Thorcey, qui lui fit donner des représentations dans certaines grandes villes dont Paris où il vint s'instaler en 1880 (3). Jacques Inaudi était connu pour exécuter mentalement, avec une rapidité surprenante, des opérations arithmétiques portant sur un grand nombre de chiffres. L'Académie, après avoir assisté à quelques-uns des exercices habituels de Jacques Inaudi, nomma une commission afin d'étudier cet étonnant personnage. Dans cette commission siégaient plusieurs mathématiciens (Darboux, Poincaré, Tisserand) et le neuro-psychiatre Jean-Martin Charcot à qui fut tout spécialement confié l'étude psychologique de ce calculateur prodige. Dès la première heure, Binet fut convié par Charcot à étudier avec lui un sujet si intéressant. Binet (Binet et Henneguy, 1894, p. 24-25) nous écrit qu'il a vu trois fois le jeune calculateur à la Salpètrière pendant que Charcot l'étudiait (4) puis l'a revu ensuite une quinzaine de fois sur une période de deux années (1892-1893) au laboratoire de psychologie physiologique de la Sorbonne où il a bien voulu se rendre pour se soumettre à diverses expériences de mesure.

Dans les premiers mois de l'année 1893, Binet eu aussi l'oportunité d'étudier un autre calculateur répondant au nom de Périclès Diamandi. Diamandi est né à Pylaros (îles Ioniennes) en 1868 et appartient à une famille de commerçants de grains. Contrairement à Inaudi il fit des études jusqu'à l'âge de 16 ans avant de s'engager dans la vie active. C'est à ce moment seulement qu'il s'aperçut qu'il avait de bonnes dispositions pour le calcul mental. Ayant lu un jour, par hasard, dans un journal, le compte-rendu d'une séance donnée par Inaudi, il voulut lui aussi devenir calculateur professionnel. Ainsi, en 1891 il abandonna le commerce pour donner des représentations dans plusieurs pays européens. Arrivé en France, Périclès Diamandi exprima le désir d'être présenté à l'Académie des Sciences (5) dans le but de montrer ses aptitudes pour le calcul mental afin qu'on puisse comparer ses performances avec celles d'Inaudi. C'est dans ce contexte que Gaston Darboux encouragea Diamandi à aller voir Binet au laboratoire de psychologie-physiologique de la Sorbonne. Après une conversation qu'eut Binet avec Charcot à ce sujet, ce dernier proposa une collaboration dans le but d'étudier la mémoire de Diamandi. Ce dernier se rendit au laboratoire pendant une quinzaine de séances, de 3 à 5 heures chacune, étalées sur plusieurs mois. Il ne rencontra jamais Inaudi malgré la demande sans cesse renouvelée qu'il formulait dans le but de se mesurer avec son rival (6).

Les résultats des premières recherches sur Inaudi furent d'abord publiées le 15 mai 1892 dans la Revue des Deux Mondes (Binet, 1892a) puis

dans les Bulletins du laboratoire de la Sorbonne (Binet, 1893a, Binet et Henneguy, 1893). Une première étude sur Diamandi fut publiée en Juin 1893 dans la Revue Philosophique (Charcot et Binet, 1893) sous la forme d'une courte note. Les résultats obtenus après l'étude approfondie de ces deux calculateurs prodiges furent rassemblés, complétés et publiés dans l'ouvrage de Binet et Henneguy (1894). Quelles sont en résumé les principales conclusions auxquelles a abouti Binet ?

Premièrement, Inaudi et Diamandi sont capables d'effectuer des opérations arithmétiques compliquées et calculent très vite. L'étude sur Inaudi a montré cependant qu'il n'est pas beaucoup plus rapide que des calculateurs de profession (caissiers de supermarché), à qui l'on permettrait de faire les opérations sur le papier. Le trait essentiel du calculateur prodige est de faire des opérations de mémoire. Les résultats véritablement extraordinaires auxquels arrivent ces personnages reposent en effet avant tout sur une mémoire prodigieuse. Deuxièmement, ils présentent un développement remarquable de la mémoire des chiffres. Ainsi, Inaudi a par exemple l'habitude de répéter des séries de 24 chiffres dans l'ordre (l'empan normal est de 6 ou 7 chiffres) ou à rebours, mais son empan est beaucoup plus important puisqu'il peut arriver à des séries entre 40 et 50 chiffres dans l'ordre. Si l'apprentissage d'une série de 25 chiffres ne prend que 25 secondes pour Inaudi ce temps s'élève à 3mn30s pour Diamandi. L'étude d'Arnould (Binet et Henri, 1893), un mnémotechnicien de profession, a montré que s'il met 3 minutes pour ce même apprentissage il lui faut beaucoup de temps pour réciter les chiffres (31s contre 7 secondes à Inaudi et 9 secondes à Diamandi), cette lenteur est la conséquence directe de la stratégie mnémotechnique utilisée à savoir un recodage chiffre-lettres (7). Troisièmement, ces deux calculateurs sont des exemples remarquables de mémoire partielle. Chez Inaudi, par exemple, les autres mémoires, même celle des lettres, sont très peu développées. Son empan pour les lettres est normal puisqu'il est incapable d'en répéter plus de 5 ou 6. D'autres expériences faîtes sur ce calculateur avec des matériaux non chiffrés montrent des capacités normales. Quatrièmement, l'étude de ces deux calculateurs atteste l'existence de deux types de mémoire, l'une visuelle et l'autre auditive. Diamandi se représente les chiffres visuellement mais pas sous la forme d'une image photographique. Il effectue toujours un recodage qui lui permet de représenter visuellement le matériel sous la forme de sa propre écriture. Inaudi, quant à lui, ne se sert pas dans ses opérations d'images visuelles, mais d'images auditives (il entend les chiffres mais ne les voit pas). Le calcul mental met en oeuvre ses organes phonatoires et Binet a montré que la

suppression articulatoire (subvocalisation) des chiffres (chanter une voyelle) a pour conséquence de diminuer ses performances lors du calcul mental. De même, lorsqu'on lui présente visuellement 5 nombres de 5 chiffres disposés en échiquier il met beaucoup de temps à retrouver les chiffres suivant telle ou telle direction (diagonale du carré par exemple). Il est le premier exemple connu d'un grand calculateur mental qui n'est pas visuel. Cinquièmement, les tours de force des calculateurs prodiges ne peuvent pas être attribués à leur intelligence. En effet, Binet n'a pas constaté chez les calculateurs prodiges d'hérédité bien marquée pour l'aptitude au calcul mental. Etudiant la littérature à ce sujet (Scripture, 1891), il constate que la plupart d'entre eux sont nés dans des milieux pauvres, n'ont subi l'influence d'aucun maître; leur précaucité est remarquable. A mesure qu'ils grandissent, ils tendent à se répartir en deux groupes, les uns deviennent de grands mathématiciens et les autres restent simplement des calculateurs prodiges, s'absorbant dans leurs calculs et ne manifestant, en dehors de leur spécialité, qu'une intelligence moyenne voire médiocre. Sixièmement, Binet montre que seul un exercice répété contribue à produire et à maintenir la supériorité que possèdent dans leur spécialité les calculateurs prodiges. Il note par exemple qu'Inaudi perdit beaucoup de sa capacité au calcul mental après qu'il ait consacré un mois à des études dans les livres. On sait aujourd'hui, par exemple, que l'entrainement accroit dans des proportions considérables l'empan mnésique (Ericsson, Chase et Faloon, 1980).

Le résumé des principaux résultats obtenus par Binet montre combien cet auteur a été un génial précurseur dans le domaine de l'étude des calculateurs mentaux. Binet fut en effet le premier à souligner l'importance de la mémoire dans le calcul et à souligner le rôle de l'exercice et des stratégies dans ce domaine. L'analyse plus récente des procédures de calculs utilisées par le mathématicien A.C. Aitken, considéré comme le plus grand calculateur mental jamais étudié, a conduit Hunter (1962, 1977) à montrer l'influence des facteurs déjà décrits par Binet. Mais les écrits du psychologue français contiennent également d'autres informations qu'il convient de souligner. En effet, Binet a aussi décrit assez longuement comment les experts en calcul mental exploitent leur connaissances afin d'augmenter leur mémoire. Ce type de stratégie a récemment été avancé par Chase et Ericsson (1982) pour expliquer la supériorité des experts sur les novices. Selon ce principe d'encodage mnémonique, l'expert encode une nouvelle information en s'aidant des connaissances déjà acquises au cours de sa pratique. D'autre part, Binet avait aussi noté que les calculateurs utilisaient une stratégie de calcul de gauche à droite, un résultat en accord avec les

analyses plus récentes qui ont été faites sur d'autres experts en la matière (cf. Hunter, 1977; Smith, 1983). Enfin, la découverte assez surprenante, concernant la capacité de ces calculateurs à répéter en sens inverse de présentation 200 nombres présentés au cours d'une session de 2 heures, a été soulignée récemment par Staszewski (1988) avec d'autres calculateurs.

2 - LES JOUEURS D'ÉCHECS

L'intérêt de Binet pour les joueurs d'échecs qui jouent sans voir lui est venu en février 1891 (Binet, 1893b, p. 826) à la faveur d'un événement qu'il mit en relation avec les écrits d'Hippolyte Taine. Il avait appris à l'époque qu'un jeune alsacien, devenu par la suite un philologue distingué, du nom d'Alphonse Goetz (1865-1934) avait joué huit parties en simultané et en aveugle au Café de la Régence à Paris (ce personnage fut d'ailleurs pendant plusieurs années le meilleur joueur français). Ayant à l'esprit l'observation rédigée par Hippolyte Taine (1870) sur un joueur d'échec dans son célèbre ouvrage intitulé "de l'intelligence" (t.I, p.80), il faisait l'hypothèse que ce joueur avait utilisé l'imagerie visuelle (mémoire visuelle) pour accomplir un tel exploit. Cependant, en l'interrogeant personnellement, il semblait que ce tour de force n'avait pas été accompli de cette manière. N'ayant pas compris l'explication que lui donna Goetz à l'époque, il encouragea ce dernier à rédiger son auto-observation.

Binet abandonna temporairement l'étude de la mémoire des joueurs d'échecs jusqu'à ce que Alphonse Goetz se décide à publier en Août 1892 un court article sur le jeu sans voir dans "la Stratégie", un journal spécialisé dans le jeu d'échecs. Dans cet article (republié par Binet et Henneguy, 1894, pp. 340-351), Goetz soutenait l'idée que le joueur sans voir n'utilise pas sa mémoire visuelle; il ne se représente pas l'échiquier comme s'il le voyait, mais il calcule et raisonne. Il sembla difficile à Binet d'accepter une pareille hypothèse dans la mesure où plusieurs joueurs de première force consultés sur ce point ne partageaient pas l'opinion de Goetz. Cependant, l'article de Goetz prouvait que ce distingué joueur ne ne servait pas d'une mémoire visuelle concrète. La complexité de la question intrigua Binet. Pour l'éclaircir, il fit une enquête afin de recueillir les observations des plus grands joueurs d'échecs. Le questionnaire, publié d'abord en septembre 1892 dans la revue "la stratégie", fut reproduit dans la plupart des journaux échiquéens de l'étranger et traduit en plusieurs langues (russe, anglais, allemand, espagnol, etc.). Binet écrivit aussi des lettres personnelles aux maitres de l'échiquier d'alors afin de recueillir leur opinion, celle de

l'allemand Siegbert Tarrasch (1862-1934), un des plus forts joueurs au monde et l'un des meilleurs pédagogues échiquéens de tous les temps, fut si intéressante qu'elle a même été publiée par Binet (Binet, 1893c; Binet et Henneguy, 1894, pp. 351-361). L'enquête commencée en octobre 1892 a été close en Juin 1893 (Binet, 1894b). Les résultats de cette enquête et d'autres observations effectuées au laboratoire de la Sorbonne sur les joueurs d'échecs furent communiqués au journal "la Revue des Deux Mondes" au cours de l'année 1893 avant d'être présentés dans l'ouvrage "la Psychologie des Grands Calculateurs et Joueurs d'Echecs" (Binet et Henneguy, 1894).

Binet (1893b, Binet et Henneguy, 1894) admet que le jeu sans voir repose sur les connaissances (mémoire des souvenirs anciens, nous l'appelerions aujourd'hui la mémoire sémantique), l'imagination (mémoire visuelle) et sur le souvenir des positions, des raisonnements et des calculs (mémoire des idées). Binet fut un des premiers psychologues à attacher une grande importance au rôle que joue les souvenirs anciens dans l'acquisition de nouvelles connaissances et à souligner l'implication importante pour la psychologie de l'étude des experts. Si les joueurs sans voir sont doués à un haut degré de la faculté de visualiser, c'est-à-dire de se représenter les positions comme s'ils les voyaient, il y a entre eux à cet égard de grandes différences. En général, ils voient l'échiquier devant eux et n'en voient distinctement que la partie sur laquelle le combat est actuellement engagé, la vision mentale de l'échiquier se fait essentiellement par portion successives (il avait déjà constaté cela chez le calculateur Diamandi) (8). Les différences les plus importantes se rapportent à ce que Binet appelle la mémoire visuelle concrète et la mémoire visuelle abstraite. Les joueurs qui se servent de la mémoire visuelle concrète se représentent la couleur des cases, la couleur, la forme et les détails caractéristiques des pièces. Mais la majorité ne se servent que d'une mémoire visuelle abstraite (intellectuelle) comme l'ont expliqué Alphonse Goetz et Siegbert Tarrash. Le joueur conserve le sentiment de voir mentalement l'échiquier, localise toujours devant lui l'image, mais la forme des pièces, la couleur des cases, celle surtout des pièces disparaissent plus ou moins, les joueurs percoivent dans l'objet seulement les détails nécessaires à l'usage (on parlerait aujourd'hui de mémoire opérative). En revanche, le joueur se représente avec une grande netteté la position des pièces sur l'échiquier, les rapports spatiaux qu'elles affectent entre elles, et ce qu'il appelle souvent la puissance de la pièce, c'est-à-dire probablement le mouvement que la pièce peut exécuter. Cette mémoire des positions des pièces et de leurs mouvements, Binet (1893c) propose, suivant une suggestion de Jean-Martin Charcot, de l'appeler "mémoire visuelle géométrique". Binet

signale encore, chez les joueurs sans voir, le rôle, plus important peut-être qu'eux mêmes ne le croient, de la mémoire verbale et enfin celui de la mémoire de récapitulation qui fait par exemple que le joueur, incertain de la position d'une pièce, rejoue mentalement la partie entière.

Le facteur clé que Binet a identifié dans son étude était certainement la manière avec laquelle les experts de l'échiquier se représentent les situations. Si la mémoire visuelle des positions joue un rôle, certains résultats (Lories, 1984) semblent suggérer que dans une tâche où le support perceptif est absent, les bons joueurs choisissent d'utiliser un code organisationnel particulier indépendant des caractéristiques de l'image des positions. Il parait dès lors vraisemblable qu'existent en fait plusieurs codages différents développés probablement au fur et à mesure que progresse la partie. Les recherches récentes concernant la représentation de la position ont surtout été effectuées à partir du paradigme de De Groot (pour une revue: Lories, 1984). Dans les expériences originales, De Groot (1965, 1966) présentait les positions des pièces à des joueurs sur un échiquier durant environ 5 secondes. La position modèle était ensuite cachée, le sujet devant la reproduire sur un second échiquier. Il est apparu que les joueurs les plus forts au jeu d'échecs se montrent aussi supérieurs à cette tâche si, et seulement si, les positions présentées sont significatives et techniquement plausibles. Cette supériorité disparaît lorsque les positions utilisées sont des positions aléatoires des pièces sur l'échiquier, et non plus des positions correspondant à des parties réelles (cf. aussi Chase et Simon, 1973a, 1973b). Ces mêmes recherches ont aussi montré que la rétention est meilleure si la position est présentée par étapes et construite par adjonction successive des configurations significatives dont elle est constituée, plutôt que constuites par présentation successive de configurations quelconques.

CONCLUSION

Les résultats obtenus par Binet ainsi que les idées qu'il a avancé concernant la mémoire des calculateurs prodiges et des joueurs d'échecs qui jouent sans voir sont informatives et encore d'actualité un siècle après. Le psychologue suisse Jacques Larguier des Bancels (1912) a eu raison d'apprécier tout particulièrement l'apport personnel d'Alfred Binet à la psychologie scientifique de la mémoire et de considérer que c'est peut-être sur ce point qu'il a donné les aperçus les plus nouveaux. En effet, lorsqu'on considère l'oeuvre de Binet avec attention, on s'aperçoit que la mémoire y est continuellement présente. Elle fut étudiée à maintes reprises et sous des

aspects très divers. Ainsi, dès ses premières publications Binet (1883; Binet et Féré, 1886) traite de l'influence inconsciente de la mémoire dans la perception et le raisonnement. Il devient après Hermann von Helmholtz (1866), un des premiers psychologues expérimentalistes à parler de ce qu'on appelle aujourd'hui la mémoire implicite (Schacter, 1987). Il est certain que sa rencontre avec des calculateurs prodiges et les joueurs d'échecs lui a fourni le premier thème de ses recherches expérimentales lorsqu'il fut admis par Beaunis dans le laboratoire de psychologie de la Sorbonne en 1891. Conscient du fait que la mémoire est une des fonctions psychologiques qui se prête le mieux à l'expérimentation, il développa à partir de 1893 de très nombreuses idées et expériences à ce sujet (Binet, 1894a, 1897, 1899, 1900, 1903; Binet et Henri, 1894a, 1894b, 1895a, 1895b) dont on commence seulement aujourd'hui à découvrir l'importance (Cunningham, 1988; Thieman et Brewer, 1978). L'écrit de Binet et Henneguy (1894) contient d'ailleurs des réflexions intéressantes au sujet de la mémoire normale qui sont toujours d'actualité (9), nul doute que la lecture de ses autres ouvrages et articles suscitera dans l'avenir l'attention des psychologues travaillant dans le domaine de la mémoire.

NOTES

(1) Dans leur article sur les mnémotechniciens oubliés, Brown et Deffenbacher (1975) ont déclaré sur Binet :"We should remind the reader that the critical studies in order of importance are those by Müller, Susukita and Binet... Binet's study, though fairly extensive, is directed more at calculation power than memory". L'auteur de cet article ne pense pas que ces affirmations soient justifiées. Au contraire, d'une part, les écrits de Binet contiennent de très nombreuses informations et résultats expérimentaux concernant le fonctionnement normal et supra-normal de la mémoire qui auraient encore leur place dans des revues actuelles de psychologie et que, d'autre part, l'apport de Binet à la psychologie de la mémoire est très important mais a malheureusement été totalement sous-estimé (cf., Wolf, 1976; Siegler, 1992).

(2) L'idée de l'existence de diverses formes de mémoire s'est développée au cours du XIXe avec la réflexion de philosophes comme Maine de Biran et les travaux des physiologistes comme Gall qui fonda la théorie des mémoires partielles. Un siècle plus tard, il apparaît que le débat sur le problème de savoir s'il faut considérer la mémoire comme une entité unique est très vif (cf., Nicolas, 1993).

(3) Il est à noter qu'Inaudi fut étudié à l'âge de 12 ans par le célèbre neurologue Paul Broca (1880) qui écrivit même sur ce cas une petite note à caractère anthropologique et psychologique.

(4) Le rapport de Charcot parut le 7 Juin 1892 dans les Comptes Rendus Hebdomadaires des séances de l'Académie des Sciences (Charcot, 1892).

(5) C'est en date du 20 février 1893 que l'Académie confia l'examen de Diamandi à la commission qui avait été chargée de faire le rapport sur Inaudi (Bertrand, 1893). Cette commission semble avoir assisté à quelques expériences mais aucun rapport ne fut publié.

(6) Binet eut l'oportunité de rencontrer et d'étudier quelques années plus tard (Binet, 1909) la soeur de Diamandi qui possédait une mémoire des chiffres aussi étendue que celle de son frère.

(7) Binet et Henri (1896, p. 437) rapportent une anecdote sur Inaudi qui avait pour ami un mnémotechnicien très habile: "parfois, il font la gageure, se trouvant ensemble, de retenir les numéros de fiacres qui passent; tous deux y arrivent aussi vite, avec des procédés tout différents; huit jours après, Inaudi ne se rappelle plus rien, et le mnémotechnicien se souvient encore."

(8) Chase et Simon (1973a, 1973b) ont proposé de parler d'un "oeil mental" qui construit une image de la position déformée (imprécise à la périphérie).

(9) Binet (Binet et Henneguy, 1894) a souligné le premier le rôle de l'interférence rétroactive dans l'oubli en mémoire à court terme: "la disparition du souvenir de 9 chiffres est presque infaillible quand nous cherchons, après les avoir répétés, à en retenir une nouvelle série de 9. A moins d'employer quelque artifice du genre de ceux qu'enseigne la mnémotechnie, nous constatons dès que nous faisons un effort pour apprendre la seconde série, que la première s'est complètement évanouie. Inaudi n'est point sujet à ces faiblesses de mémoire" (p. 56). On trouve aussi dans cet ouvrage des remarques intéressantes sur ce que l'on appelle aujourd'hui l'oubli dirigé ou motivé: "la faculté d'expulser de sa mèmoire un fait dont la connaissance devient inutile peut être considéré comme un sérieux avantage pour l'individu qui la possède. Il me semble que l'on doit arriver, par un effort volontaire, à développer en soi cette modalité de la mémoire" (p. 57-58). De même, Binet souligne l'importance des connaissances préalables pour l'enregistrement en mémoire: "le souvenir sera plus tenace si on l'assimile

lentement, si on cherche à nouer un grand nombre de relations entre le fait nouveau et les faits anciens qui sont dans la possession de notre mémoire" (p. 58).

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Lectures de la psychanalyse dans *l'Année Psychologique* de Piéron, 1913-1945

Annick Ohayon

Pour l'historien de la psychologie, *L'Année Psychologique* est un outil incomparable. Sur le modèle des autres "Années", cette revue est conçue par Binet, dès sa fondation en 1894, non seulement comme l'organe du laboratoire de psychologie physiologique de la Sorbonne, et ainsi la plate-forme de diffusion de ses travaux au plan international, mais surtout comme le bilan, la synthèse des travaux les plus importants en psychologie réalisés chaque année dans le monde.
La première vocation de la revue est regroupée dans la partie "mémoires originaux" et la seconde dans la partie "analyses bibliographiques". Ainsi, si l'on veut savoir ce qui, pour les psychologues d'une époque donnée est central: théories, domaines d'applications, méthodes, objets, c'est cette partie, avec les choix de présentation, d'organisation, d'analyse et de critique qu'elle révèle, qui s'avère particulièrement significative.
Nous allons ici nous intéresser à l'accueil, à la lecture et à la place que les psychologues français réservent à la psychanalyse, telle qu'elle apparait dans *L'Année Psychologique* dirigée par Pièron, entre les deux guerres mondiales.

Lorsque Pièron prend la direction de la revue en même temps que du laboratoire de la Sorbonne, en 1912, à la mort de Binet, il hérite en fait d'un très lourd fardeau. On sait que Binet, qui a assuré pratiquement seul cette tâche, y a usé ses forces et sa santé. La psychologie connait alors une extension rapide, et son bilan annuel ne peut plus être le fait d'une équipe aussi restreinte. A titre d'exemple, les analyses bibliographiques qui représentaient entre quarante et cent pages sur les cinq cent ou six cent pages de la revue à l'époque de Binet vont devenir progressivement l'essentiel de son contenu, les deux tiers ou les trois quarts, c'est à dire entre quatre cent et huit cent pages pour chaque livraison. Le nombre de périodiques référés augmente dans les mêmes proportions: une vingtaine pour Binet (trois à quatre personnes assurent les analyses), quarante cinq pour le tome XX en 1914 (douze personnes font les analyses bibliographiques) et vers le milieu des années trente, plus d'une centaine de périodiques et une trentaine de collaborateurs. Du travail artisanal et quasi solitaire d'un Binet, on est passé à une organisation d'envergure, dont Piéron est le maître d'oeuvre.
La présentation de *L'Année Psychologique* de Piéron diffère un peu de celle de Binet. Elle est organisée en quatre parties: les mémoires originaux, les notes et revues ("petites études expérimentales, observations critiques, revues bibliographiques" Piéron dans l'A.P. de 1914) les analyses bibliographiques et la chronique ("who's who"

de la psychologie internationale, la chronique est toujours exclusivement rédigée par Piéron, et reflète bien ses partis-pris).

Quelle place est réservée à la psychanalyse dans l'A.P. de Piéron? C'est une place succinte, et exclusivement circonscrite aux analyses bibliographiques. En cette matière, il y a une rupture manifeste avec la ligne adoptée par Binet. En effet à une période où la diffusion des travaux de Freud est pratiquement nulle en France, deux mémoires originaux dans l'A.P. portent directement sur la psychanalyse.

Le premier en 1909, est un article de Jung: "l'analyse des rêves", le second en 1912 est de Maeder: "sur le mouvement psychanalytique". Arrêtons nous un instant sur le contexte de production et de diffusion de ces articles. C'est Binet qui commande à Jung un article de présentation de la psychanalyse. Fin 1908, celui-ci écrit à Freud (correspondance Freud/Jung Tome 1,1906-1909, p.246, Paris, Gallimard 1975): "J'ai déjà fabriqué une petite chose pour Binet, ce n'est naturellement que de l'orientation superficielle, mais écrit de telle façon qu'un français aussi puisse comprendre, pour autant qu'il le veuille. Malheureusement, seuls les psychologues auront la chose entre les mains, elle sera donc sous mauvaise garde. Les Français n'ont-ils pas déjà sauvagement ébouriffé notre Maeder? Binet tire son préjugé favorable à mon égard, jusqu'ici inébranlé, de la source Larguier des Bancels, Privat-Dozent à Lausanne et infecté via Claparède". On voit que la collaboration des psychologues français et des psychanalystes commence bien mal, et que les préventions réciproques ne sont pas négligeables. Binet en fait est plus qu'intéressé; dans le "Bilan de la psychologie en 1910", il place les recherches de Freud sur la psychanalyse parmi les quatre questions principales de la psychologie.

Le second article, celui de Maeder ("l'ébouriffé") est lui aussi une commande de Binet. Il devait paraitre en 1911, mais cela n'a pas été possible faute de place. Il sera donc présenté dans le numéro de 1912, qui se trouve être un hommage à Binet après sa mort, ce qui accentue son importance. C'est un article long, fouillé, militant. Maeder insiste sur le fait que Freud n'est plus seul mais qu'une véritable école s'est crée autour de lui , qui renouvelle la psychiatrie et toute la psychologie. Il présente la méthode et l'ensemble des publications en allemand et en français, en particulier ce qui parait dans les "Archives de Psychologie". C'est donc bien à cette période, par la Suisse, par Claparède d'une part, et par Jung et Bleuler d'autre part que l'épidémie psychanalytique commence à "infecter" la France.

Ce n'est à coup sûr pas à Piéron qu'il faut reprocher d'avoir contribué à sa propagation. De 1913 à 1945, aucun mémoire original sur la psychanalyse. Un seul article de l'A.P. de 1931, dans la partie "Notes et Revues" fait référence explicitement à la psy-

chanalyse. Il est écrit par Eugène Schreider et s'intitule:"Sur quelques phénomènes d'ambivalence et de refoulement dans la psychologie du travail". Schreider a une position tout à fait originale. Il est le seul, que ce soit dans l'A.P. ou dans la revue de la Science du Travail, et plus tard dans le Travail Humain à proposer une explication psychanalytique du rapport au travail, en en étudiant les facteurs affectifs inconscients.
A la même période, Lahy, Laugier, Piéron et Bonnardel développent une étude scientifique du travail essentiellement en termes psychophysiologiques et psychotechniques. Schreider est très conscient de la hardiesse de son propos, et croit bon de se couvrir par une citation de Piéron (Psychologie expérimentale, 1927, p.72) "Freud a poussé à l'extrême les données théoriques fort exactes établissant une métaphysique de l'inconscient". En bref, ce que dit Freud n'est pas faux, mais exagéré.

A part cette exception remarquable, nous ne trouvons trace de la psychanalyse que dans les analyses bibliographiques. Et encore, sa place y est-elle restreinte, et changeante. Elle apparait en 1907 dans la rubrique VII:"association et imagination, rêves, psychanalyse". Celle-ci comporte dix pages sur quatre cent soixante quatorze, l'essentiel des études portent sur le rêve. Ce sont Duprat et Piéron qui les rédigent.
La guerre interrompt la publication; la revue reparait en 1920. Dans la rubrique VII, la psychanalyse a disparu. Par contre à partir de 1922, on va en trouver mention dans la première rubrique:"Généralités, traités, théories et méthodes" car c'est le moment où commencent les premières traductions des livres de Freud en français.
A partir de 1923 (et ce jusqu'à 1939) elle réapparait en tant que telle dans la troisième rubrique:"psychologie comparée". Cette rubrique comporte plusieurs parties. Dans la partie cinq:"psychologie pathologique" (qui est dans toutes les livraisons de la revue, extrêmement consistante) apparait une sous-partie, bizarrement intitulée: "psychanalyse, schizophrènie, autisme, démence précoce". On ne savait pas que la psychanalyse était une maladie! Cette partition curieuse a cependant un sens: on peut y lire le souci de Piéron de ne pas faire de la psychanalyse de Freud un domaine propre de la psychopathologie, mais de la relier à l'école suisse allemande de Jung et de Beuler.
Dans cette même rubrique: "psychologie comparée", on retrouvera aussi des mentions concernant la psychanalyse dans la sous-partie: "psychologie ethnique et sociale". Il s'agira essentiellement des oeuvres de Freud concernant la psychanalyse de la vie sociale. En 1931 apparait une nouvelle rubrique (XII) "conscient et inconscient- suggestion et hypnose- etats de sommeil- interprétations psychanalytiques" qui correspond en fait à un retour à la classification de 1914, avec le rêve et l'hypnose. Cette rubri-

que est de faible importance et la place de la psychanalyse y est encore plus mince. Voici donc comment l'A.P. de Piéron présente la psychanalyse: comme une doctrine générale (rubrique I), une sous-sous section de la psychopathologie ou de la psychologie sociale (rubrique III), ou bien, quand on ne sait vraiment pas où la mettre, comme un de ces objets au statut scientifique incertain que sont l'hypnose et la métapsychie (rubrique XII).

Après avoir vu le mode de classement de la psychanalyse dans l'A.P., intéressons nous à ceux qui la lisent, la critiquent et la commentent. Une première constatation s'impose. Tout ce qui est de Freud et sur Freud, au moins jusqu'en 1930, et même après, est le domaine réservé de Piéron. C'est lui qui vient très largement en tête des commentateurs, en particulier des traductions des livres de Freud.
En ce qui concerne les livres, le deuxième auteur des critiques (dans une proportion bien moindre que Piéron) est Wallon. Pour les revues, nous trouvons l'ordre suivant: Piéron toujours, puis Wallon, Marguerite Lichtenberger, Poyer et Guillaume. A partir de 1930, jusqu'à 1937, les analyses portant sur la psychanalyse sont presque exclusivement le fait de Jean-Frois Wittman.
Jean-Frois Wittman est le premier psychanalyste qui collabore à l'A.P.. Il a fait des études de psychologie aux Etats-Unis. Analysé par Loewenstein, il devient membre adhérent de la société psychanalytique de Paris en 1930 et titulaire en 1932. Il entreprend alors des études de médecine (il a pour condisciples Marc Schlumberger et Françoise Marette) que sa mort interrompra brutalement. Peintre, il a été un membre actif du groupe surréaliste. Ses travaux portent sur l'art, l'expression des émotions, et le suicide. Avec lui nous allons voir la tonalité générale des analyses changer, devenir plus documentée, technique et positive.
Après sa mort, les contributeurs sont divers et la part faite à la psychanalyse diminue à nouveau. On trouve des critiques sous la plume de Charles Blondel, d'Annette Baron-Fessard, Jawiga Abramson, Claude Nony, Madame Piéron et en 1939 (mais pour cette seule année) de Lagache. Il faut attendre la fin de la guerre, la livraison de 1945-46, pour voir arriver parmi les collaborateurs de l'A.P. un deuxième psychanalyste, membre de la S.P.P. et médecin: Maurice Benassy.
En ce qui concerne les revues, quelles sont celles que réfèrent les psychologues français? Il est patent que Piéron et ses collaborateurs ignorent résolument les publications spécifiques de la psychanalyse, en langue allemande. Ni le Jahrbuch, ni le Zentralblatt, ni Imago ne sont cités. A partir de 1920, la seule revue de psychanalyse régulièrement compulsée est l'*International Journal of Psychanalysis.* Parmi les autres revues en langue anglaise, les plus fréquemment citées sont: *Character and Personality*

-British Journal of Psychology -American Journal of Psychology- Journal of Abnormal and Social Psychology.

Pour les revues en langue française, nous trouvons *Scientia* (revue internationale de synthèse scientifique, où paraissent les premières traductions de Freud en français), *Les Archives de Psychologie, La Revue Philosophique, Le Journal de Psychologie*, et surtout des revues médicales: *L'Encéphale, Les annales médico-psychologiques, L'Hygiene Mentale.*

Mais si l'A.P. de Piéron ignore les publications allemandes de la psychanalyse, elle ignore encore plus superbement la *Revue Française de Psychanalyse*, qui est pourtant la seule de son espèce en France à partir de 1927. Elle ignore aussi *L'évolution Psychiâtrique* au moins jusqu'en 1930, c'est à dire jusqu'à l'arrivée de Jean Frois-Wittman. La psychologie scientifique française ne veut rien savoir de la psychanalyse française. Il s'ensuit par exemple que la plupart des articles de Lafforgue et de Marie Bonaparte ne sont cités que lorsqu'ils sont rédigés en anglais! Il faut préciser que cette ignorance est réciproque, puisque *La Revue Française de Psychanalyse* ne cite jamais l'A.P. dans ses revues bibliographiques. Le seul organe où psychologues et psychanalystes se rencontrent et se parlent est le mensuel pelmaniste *La Psychologie et la Vie* (référé dans l'A.P.) dirigé par Paul Masson-Oursel, où collaborent des psychologues comme Janet et Fontègne, des philosophes, en particulier Alain et des psychanalystes tels que Lafforgue, Pichon, Allendy et Paul Schiff. Mais il s'agit d'une revue de vulgarisation, et les risques de s'y commettre au regard de ses pairs sont probablement moindres.

Méfiance réciproque, ignorance, mépris parfois, telle semble être la tonalité générale des relations entre la psychologie et la psychanalyse en France entre les deux-guerres. Voyons précisément comment ceci s'exprime dans l'A.P., en particulier sous la plume de Piéron.

Il nous semble qu'on peut repérer deux périodes. La première où le ton est essentiellement critique, qui va de 1920 jusqu'au début des années trente, la deuxième où la critique se nuance, pour faire place parfois à des appréciations très positives de 1930 à la mort de Freud, en 1939.

La première période correspond précisément à la publication des ouvrages de Freud en français. Dans l'A.P. de 1921, Piéron commente la parution de La Psychanalyse (1921, Genève, Sonor) avec une introduction de Claparède, et l'Introduction à la Psychanalyse (1921, Paris, Payot). Piéron s'attache essentiellement à la présentation que fournit Claparède du petit livre de Freud, présentation tellement "sympathique" que "cherchant à dissimuler certaines malformations de la pensée de Freud, il s'attire de la part de ce dernier des remontrances". Voilà comment Claparède s'efforce de rendre la psychanalyse présentable au public français:

- Aucune des idées principales de Freud n'est nouvelle
- La nouveauté réside dans la valeur qui leur est donnée et dans leur systématisation.
- Beaucoup de psychanalystes "hiérophantes de quelque doctrine ésotérique", font tort à la doctrine, en la transformant en crédo. Et Piéron d'ironiser: "Il faudrait donc retirer la psychoanalyse aux psychoanalystes (NDA: le vocable psychoanalyse disparait vers 1923 pour psychanalyse) ou du moins à la plupart d'entre eux, même si certains concepts- en les dégageant d'un pansexualisme auquel la guerre a donné le coup de grâce- peuvent prendre une valeur générale, la méthode, dans la mesure où elle dépasse l'analyse de Pierre Janet, dont elle est née, peut-elle être féconde dans sa subjectivité essentielle, ce dont il est permis de douter. En tout cas, pour apprécier, il faut connaître. Dès lors, on ne peut s'abstenir de lire Freud". Cette longue tirade met bien en relief toute l'ambivalence de Piéron. Il n'aime pas du tout, mais il ne peut s'empêcher de reconnaître que...

Et dans ses critiques nous retrouvons souvent le même contenu; parmi les termes qu'il emploie pour caractériser la doctrine freudienne, nous relevons: subjectivisme, dogmatisme, foi, croyance, religion (qui s'opposent à l'attitude scientifique); parmi les adjectifs: ingénieux, subtil, romanesque, amusant, distrayant, outrancier, libidineux (qu'il emploie constamment à la place de libidinal) répétitif, lassant, métaphysique, et toujours des formules telles que "il faut bien reconnaître que", "on ne peut s'empêcher de...". Un exemple parmi d'autres: dans l'A.P. de 1925 Piéron rend compte de la parution chez Payot en 1924 de Psychologie collective et analyse du moi. La thèse de Freud, dit-il, suite au "roman psychanalytique des origines de la société humaine exposé dans Totem et Tabou n'est évidemment pas dépourvue de tout intérêt...Une Eglise, une Armée, un Parti doivent à des liens libidineux leur cohérence, liens qui ne rattachent tant un individu à l'ensemble des autres, que chacun des membres du groupe à un individu privilégié à un chef" mais hélas, conclut-il "Freud se contente de rapides affirmations".

L'année 1926 est marquée par la parution chez Alcan des Sciences des rêves. Piéron loue la traduction d'Ignace Meyerson, mais aussi la sincérité de Freud, qui se dévoile dans son être intime, "sincérité qui mérite d'être admirée". On comprend d'autant mieux l'intérêt manifesté par Piéron pour la somme monumentale de Freud qu'on sait qu'il a lui-même travaillé à l'étude des rêves, y compris des siens. Cette évolution du ton de Piéron concernant l'oeuvre de Freud est encore plus sensible à partir de 1930. En 1933 il présente Essai de Psychanalyse appliquée paru chez Gallimard, dans une traduction de Marie Bonaparte. Il admire sincèrement l'analyse du Moïse de Michel-Ange, s'intéresse à l'emploi de la psychanalyse dans la pratique judiciaire, et termine ainsi: "on ne peut, à lire ces essais écrits dans la maturité, qu'admirer l'intelligence si souple et si variée dont fait preuve le vieux maître viennois". Avec l'âge, Freud se-

rait-il devenu moins dangereux? En tous cas il est évident que la psychanalyse appliquée dont il est question dans les Essais n'est pas sur le même terrain que la psychologie appliquée de Piéron.
Jusqu'à la mort de Freud, on ne trouve plus mention de ses oeuvres sous la plume de Piéron. Voici la notice nécrologique qu'il rédige pour la chronique du volume 39: "Sigmund Freud, exilé, est mort en Angleterre, dans la nuit du 23 au 24 Septembre 1939 dans sa quatre-vingt quatrième année.
On a trop écrit sur la personnalité et l'oeuvre du célèbre fondateur de la psychanalyse pour qu'il soit utile de rien ajouter à son nom, à l'occasion de sa disparition après oeuvre accomplie". Dans l'art de la litote, il est difficile de faire mieux.

Nous avons insisté longuement sur le commentaire de Piéron concernant l'oeuvre freudienne parcequ'il représente l'essentiel de la lecture de la psychanalyse opérée dans l'A.P. et qu'il incarne, sur un versant négatif non dénué d'ambivalence la ligne générale de la revue.
Nous devons mentionner aussi que Piéron accorde une place non négligeable à l'oeuvre de Jung et que ses critiques à son égard sont beaucoup plus nuancées. Parmi les auteurs les plus souvent analysés, nous trouvons, de très loin en premier: Freud, puis Jung, Mélanie Klein et Ferenczi (essentiellement par J.Frois Wittman entre 1930 et 1937) et en dernier lieu Adler.
Pour les auteurs français: Pichon, que Piéron tient en grande estime, Hesnard puis Allendy, Lafforgue et Sophie Morgenstern. Marie Bonaparte n'est citée qu'une fois, et en anglais, mais elle n'est pas médecin, et n'écrit pas dans les revues de psychiâtrie.
Si l'existence même de *La Revue Française de Psychanalyse* n'apparait pas, les congrès des psychanalystes de langue française sont toujours régulièrement annoncés et parfois commentés. L'article le plus long que nous avons recensé est de Wallon et porte sur le livre de Charles Blondel La Psychanalyse paru chez Alcan en 1924. Sur trois pages il loue la présentation faite par Blondel de la doctrine freudienne; quand à la critique de Blondel "le ton plait ou déplait, il a bel et bien un accent d'honnêteté" dit Wallon. Wallon a cru bon d'introduire son propos par cet avertissement ironique et subtil: "la résistance au freudisme, sentimentale comme souvent elle s'avère, peut, en vertu d'une de ses thèses essentielles, celle de la censure et du refoulement, être interprétée par lui comme une confirmation...Au contraire, l'opposition que lui fait Blondel s'étaie sur une connaissance exacte". Ainsi, ce n'est ni sur une oeuvre de Freud, ni sur un ouvrage au ton positif que nous avons trouvé le commentaire le plus argumenté.

Notre analyse de la place, et des lectures faites de la psychanalyse dans l'A.P. prend fin après la guerre. En effet dès sa réapparition (volume 45/46) Piéron, qui n'est plus tout jeune, n'assure plus seul la direction de la revue. Il est secondé dans sa tâche par Alfred Fessard et par Paul Fraisse. Une ère nouvelle s'ouvre alors pour la psychologie, avec la création de la licence de psychologie et la nomination de Daniel Lagache comme professeur à la Sorbonne. Tout ceci contribuera à positionner différemment les rapports des psychologues français avec la psychanalyse.

Summary

The status of psychoanalysis has been studied in bibliographic analysises of the *Année Psychologique*, the review conducted by Piéron, after Binet, from 1913 to 1945. This position was limited and has been considered with criticism until 1930.
Piéron provides directly all the comments on Freud's publications, and also what appears about Freud himself.
Mainly, french psychologists ignore the french psychoanalytic work and the german specific litterature. The principal criticism is based on the non-scientific character of psychoanalysis.

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AN ABSTRACT OF THE ARTICLE "LUCIEN LEVY- BRUHL'S THEORY OF PRIMITIVE PERSONALITY-MISUNDERSTOOD, UNDERESTIMATED AND CRITICIZED IN THE HISTORY OF PSYCHOLOGY"

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In my paper I am going to present the reception of Levy-Bruhl's theory in social sciences and its role in the history of psychology.Secondly I will try to explain why in my opinion his theory of personality might be interesting for modern psychologist.I intend to propose a different than typical way of " decoding" it.

LUCIEN LEVY-BRUHL'S THEORY OF PRIMITIVE PERSONALITY- MISUNDERSTOOD, UNDERESTIMATED AND CRITICIZED BUT ... INDISPENSABLE IN THE HISTORY OF PSYCHOLOGY

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There have been a few reasons that made me write the paper on Levy-Bruhl's theory of primitive personality:
-absence or inadequate interpretation of the theory in handbooks of ethnopsychology and theories of personality
-on the other hand, the great influence of its concepts on psychology and antropology
-imperfection of modern theories of personality and the incomplete outlook upon human individuality they propose
-conviction of complimentary character of it and its usefulness in describing complete structure of human soul
-border-line approach it presents, as combines elements of sociology, antropology and psychology.

Who was Levy-Bruhl, what can we say about life and work of such a controversial scientist?He was born on April 10, 1857 and died on March 13, 1939 so was almost contemporary to Sigmund Freud.During his long life he held an office of a head of the Sorbonne's Department of the History of Modern Philosophy, founded the Institute of Ethnology and was a guest lecturer at Harvard and many others American and European universities.His early works became written under the great influence of the father of Positivism Auguste Comte, German philosophers among them Immanuel Kant and first of all of the originator of French Sociological School Emil Durkheim.What is

interesting he also took courses in clinical psychology at the clinic of Sainte Anne.Not groundlessly he used to be called a true nobleman of intellect, famous for his exquisite courtesy and modesty.

After a time when he had been interested in studies of etnics, in first years of XX century he started to do research work on primitive thought.Two of his concepts, presented in 1910 in his book "How Natives Think" became popular:

* collective representations - intimations of reality given to the individual by virtue of his membership in a group or in other words the basic premises according to which the primitive man organizes his world, a kind of the individual's views on the nature of the universe handed down by ancestors by means of the myths and rituals

*the law of participation - the conviction that objects, beings and phenomena are connected by mystic powers hidden in the deeper level of reality, that is why primitive thinking is basically mystical (prelogical)-it is organized to coexist with the source of life mystery.

It is not probably well-known that these ideas have many counterparts in psychological theories.The term prelogical thinking was used by J.Piaget to explain child's thinking and B.Malinowski, famous Polish ethnopsychologist was particulary concerned with magical attitude towards reality coming from special, mystical mentality.W.Wundt published his "Elemente der Volkerpsychologie" in 1912, two years after famous Levy-Bruhl's book "How natives think" was edited.

However more exciting thing is that Levy-Bruhl late in life met his contemporary Sigmund Freud! His theory in many ways is parallel to Levy-Bruhl's, with emphasis on non-rational sources of behaviour, on the role of the unconscious in structuring the perception of reality (the ordinary and mystical in group life correspond to the conscious and unconscious mental process in individual psychology), on the importance of the mechanisms of projection and identification.They also similar way react against

the sterility of mechanistic thinking and its failure to explain the dynamics of human behaviour.

C.G.Jung's theory of archetypes and Levy-Bruhl's idea of collective representations also have certain features in common:

-they are the patterns of behaviour

-they are symbolic figures of the concept of reality

-they have aprioric and autonomous character

-both possess emotional component

-may cause the experience of "numinosum".

All above mentioned similarities testify to the presence of Levy-Bruhl's thought in the history of psychology.

Between 1910 and 1938, when he wrote his six volumes on primitive mentality, his concepts became famous and at the same time considerably criticized.Antropologists did not agree with the idea that primitive people have a different from ours, mystical mentality.They also accused him of using ethnographic sources uncritically, mixing facts coming from different cultures.In modern Polish handbooks of social sciences we will not find much information about his theory.In Polish handbooks of psychology I happenned to find only one excerpt,where I learnt that in Levy-Bruhl's opinion prelogical thinking is a lower kind of thinking, the way that uneuducated people usually reason(That is certainly a nonsense!In the most popular Polish handbook of the history of sociology there were only two sentences about him being in the footnotes. The author adds that there is no place to write about him!In some antropological books we will come across the statements that Levy-Bruhl was one of many followers of Durkheim' school, that he used to be famous 70 years ago and have an influence on European antropology, that now its theory is off the map and is not assented by modern scientist and so on.

The best recapitulation of the unfriendly and incorrect reception of Levy-Bruhl's ideas will be the article of Polish psychiatrist who apart from some essential mistakes wrote his surname with an extra "c" letter (Levy-Bruchl!).

It is always hard to find his ideas or even any references to them in books on the theory of personality and , as far as I

know, he is the author of the only handbook of primitive personality (in ethnopsychology). We can agree that all those books are to describe the personality of "civilized" not primitive man but let us take a look at well known theories of personality, if there is any need to complete them with elements of the concept of collective representations.

In the history of psychology we know five main and to some extent different views on human personality:
- as a set of learned responses shaped by means of external influences
- as a balance of competing forces, under the influence of the unconsciousness, motivating human actions
-as a set of specific traits deciding of the consistency in human behaviour
-as a permanent need for self-fulfillment with the importance of human's potential for growth and creativity
- as a system of mental representations of reality organizing and regulating informational metabolism between a man and environment.
If we were trying to built a modern definition of personality it might look like this one: "...an immensely complex subject shaped by past experiences, responsive to new conditions, interacting with the people and events, driven by specific conflicts and capable of creatively resolving them using cognitive powers to seek own fullfilment" That postmodernistic medley looks and sounds strange and with all its diversity does not exhaust the problem.The leitmotivs of present theories according to Lindzey and Hall are:
*descriptions of human behaviour
*coexistence of consciousness and unconsciousness
*individuality of a man and ego concept
*affiliation to a society
*ideal personality
*structure and development

Yet almost 90 years ago lived a man who composed a theory

which, if we properly decode it , may be complimentary to those mentioned above.What is more-in my opinion it foreruns contemoprary cultural approaches to human's personality.From his book "The soul of the primitive", a handbock of the primitive theory of personality, one can learn the notions the primitives possess of the life-principle.I would like to propose an experiment-to reread it as a theory of unknown, mystic part of (no matter if primitive or civilized) numan's personality.

Apart from rational thinking, to construct his view on reality and himself a man uses mystical (irrational)thinking.This process is responsible for the reception of all the forces and relationships imperceptible to the sences but meaningful for his life.It does not recognize contradictions in terms of Aristotelean logic, does not comply with the law of causality,is impervious to experience, it is iconi c and emotional.As Levy- Bruhl states "to live is to participate", therefore all principles of thinking (mystical generalization, abstraction, the law of mystical unity and causality) serve the participation in invisible, real,and deeper sense of objects and beings.

The kind of memory used in that process is called specifical memory.It is an exact memory of details, working in the affective way, concentrated on the concretes.As that mentality does not create the notions the memory is highly developed.

That kind of thinking has an influence on perception of personality.Levy-Bruhl says that the human's personality is extended by the individuals "appurtenances"-secretions, footprints, objects made or handled by himself, his property, name, work etc.It means that whatever is connected with us or comes from us is ourselves.The limits of the individual are variable and the appurtenances are integral parts of a personality and are confounded with it (they are individual's double).That is why we sometimes identify the artist with his sculpture and driver with his car and have a problem with a proper name for a new-born child-the sound of it, its meaning will have an influence on the child.

The other assumption of that theory is the idea of homogeneity in essence of all beings.There is one essence, whatever we call it-mana, life-principle, mystic power,inherent in all persons, plants, animals.Therefore human's personality is just one form of invisible idea that determines all beings, unifying them at the same moment.That causes an intimate communion between all beings and has an influence on almost organic solidarity among members of the same social group (in certain cases they may be substituted for each other).
The following problem is an age-long dilemma of the relation between soul and body.The antithesis of them does not exist in that mentality.The more something is mystical, the more it is real, so matter itself is a form of spirit.There is no use of distinction between body and soul since they participate in the same essence.One of the Chinese philosophers used to say:"That which is one is one, that which is not one is one"-in other words both material and immaterial quality are the same.
The last suggestion for our new theory of personality will be Levy-Bruhl's opinion on the human's development.He states that the blossoming out into adult life is not the terminal point of life-span.More important are abrupt transformations .One of them is "initiation"-the special moment during life-span when a man becomes somebody else, a child becomes a man, a man becomes a warrior, a warrior becomes a sorcerer etc.At these turning points of life a man learns important collective representations that let him participate in mystical forces.

What we have seen so far clearly shows that Levy-Bruhl's theory had an influence on social sciences but on the other hand was seriously criticized and in my opinion misunderstood.The critics fixed their attention on such aspects of his theory like scientific terms (e.g.prelogical, mystical),the source of the cultural fact he uses or put stress on the idea that primitive peoples have different mentality from civilized men.From the psychological point of view these objections are not so important.The aim of my paper

was to present the theory in a new perspective-as a forerunning and complementary to modern ones - theory of personality.His idea of specific memory could be used to explain the problem of extraordinary memory of autistic people.The problem of dual personality is similiar to the concept of the bi-presence of the individual during the participation in two natures (e.g.the mythical ancestor and child or man and tiger).The idea of extended limits of personality explains the relation between a man and his name and the derivative of mystical power in our times may be the need for myster or in terms of Rollo May book the cry for the myth of XX century man.There are many more applications of that theory that make it interesting for contemporary psychologist.In spite of its XIX century origin Levy-Bruhl's theory can be an inspiring proposal to create a new, hollistic theory of personality.Some attempts are being made in, what we could call, New Age (or roots) psychology.

L'enquête internationale sur les hallucinations 1888 - 1892

Déroulement et enjeux

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Les deux premiers congrès internationaux de psychologie, à Paris en 1889 et à Londres en 1892, ont gardé la trace d'une vaste étude qui semble avoir une certaine importance. L'objet de cette étude : les hallucinations. Sa méthode : l'enquête.
Il s'agit de soumettre les phénomènes dits occultes à la méthode scientifique et, dans un premier temps, de vérifier leur existence.

Les promoteurs de l'enquête sur les hallucinations, la Society for psychical research de Londres (SPR), sont soucieux de se démarquer tant des spritualistes que des phénomènes relevant de la pathologie.
L'objet de l'étude sera donc "les hallucinations télépathiques ou véridiques, coïncidant avec un évènement réel, et les hallucinations proprement dites, ou non véridiques, ne se rapportant à aucun évènement réel".
L'enquête recueillera donc les témoignages de personnes ayant eu des hallucinations visuelles, auditives, tactiles, alors qu'elles étaient éveillées et ne souffraient d'aucun trouble ou délire.

Nous proposons d'examiner la mise en oeuvre et la réalisation de cette enquête à grande échelle, qui s'est déroulée sur plus de trois ans. Nous suggérons par ailleurs qu' elle fait partie d'une stratégie de positionnement de l'Angleterre sur la scène internationale de la psychologie en cette fin du 19e siècle.

Fondée à Londres en 1882, la SPR déclare dans un manifeste: "(...) le moment est favorable pour entreprendre un essai organisé et systématique d'investigation d'un vaste groupe de phénomènes discutables désignés par les termes de mesmériques, psychiques et spiritualistes". Son premier président, Henry Sidgwick, professeur de philosophie à Cambridge, proclame dans son discours d'ouverture: "*"Nous sommes tous d'accord sur le fait que l'état de choses actuel est un scandale pour l'époque éclairée dans laquelle nous vivons, que la querelle sur la réalité de ces merveilleux phénomènes (...)fasse encore rage, que tant de témoins compétents aient dû déclarer leur croyance en eux, que tant d'autres s'intéressent profondément à ce que la question soit tranchée et pourtant que le monde instruit, en tant que corps social, doive simplement demeurer dans une attitude d'incrédulité."*
Les spiritualistes, de leur côté, voient favorablement et avec une certaine distance, la création de la SPR. Ils écrivent dans leur revue *Light* en 1883 :"*Les spritualistes ne peuvent douter (...) que (...) la SPR apportera des preuves aussi nettes et indiscutables de l'existence de la clairvoyance, de l'écriture des esprits, des apparitions d'esprits et des diverses formes de phénomènes physiques que celles qu'elle a réussi à apporter pour la transmission de pensée. (...)En tant que société, ses membres étudient simplement les os et les muscles; ils n'ont pas encore accédé au coeur et à l'âme.*
En fait les compte-rendus et le Journal de la SPR font un peu douter de cette distance, tant les témoignages apportés par les lecteurs évoquent souvent "les douces conversations avec de chers amis défunts. Mais Sidgwick tient le cap.
En décembre 1884 il invite Charles Richet, professeur de physiologie à la Faculté de Médecine de Paris, à présenter à Londres, devant la SPR, ses travaux récents sur la trans-

mission de pensée. Richet vient de publier dans la dernière livraison de la *Revue philosophique de France et de l'étranger* (décembre 1884) un article intitulé "La suggestion mentale et le calcul des probabilités". Le compte-rendu de la conférence prononcée à Londres, publié dans les *Proceedings of the SPR*, souligne que Richet fait opérer un grand nombre d'essais et y applique les lois de la probabilité. C'est probablement l'un des activateurs de l'enquête sur les hallucinations. Le paradigme est simple: si le phénomène apparaît plus souvent que ne le voudraient les lois du hasard, alors on a une preuve de l'existence du phénomène.
En juillet 1885 une société soeur de la SPR, nouvelle née, l'American Society for Psychical Research, publie à Boston un premier numéro de revue. On peut y lire qu'"une commission (de l'ASPR) a envoyé des circulaires réclamant une collaboration pour des expériences conçues selon les plans suggérés par le Profeseur Richet. Ils ont reçu des réponses concernant 5.500 essais pour deviner la couleur d'une carte à jouer alors que la personne se concentre sur la carte et 11.600 essais pour deviner un chiffre de 1 à 10.

De 1884 en juillet 1888 le travail d'enquête se met en place. Il n'est pas encore question d'une enquête à grande échelle. Il faut surtout réunir des faits, non des faits de seconde main, mais des faits directement observés. Sidgwick lance un appel en ce sens à l'assemblée qui vient de le réélire en juillet 1888.

C'est Louis Marillier qui va faire office de cheville ouvrière dans le lancement de l'enquête à une échelle internationale en utilisant le congrès de 1889. Marillier, agrégé de philosophie, collaborateur de la *Revue philosophique*, est secrétaire de la SPR pour la France. Il est aussi secrétaire du comité d'organisation du congrès international de psychologie physiologique pour lequel il prépare et signe, de même que Richet, plusieurs rapports préparatoires.

En avril 1889 le *Journal of the SPR* annonce une nouvelle étude sur les hallucinations.
En juillet 1889 la structure de l'enquête est en forme et le président Sidgwick, est responsable de l'enquête sur les hallucinations. Le questionnaire comporte deux formulaires. Sur le premier, "A", une seule question "*Cette question s'adresse à des personnes de toutes opinions; elle n'implique aucune croyance ou incroyance relativement aux fantômes, ni à la télépathie, ni à quelque explication que ce soit du phénomène étudié (...) J'espère qu'on remarquera l'impartialité de la formulation de la question.*"
"Vous est-il arrivé, alors que vous étiez complètement éveillé, d'éprouver l'impression nette de voir un être vivant ou un objet inanimé, sans que vous puissiez rapporter cette impression à aucune cause extérieure ? Vous est-il arrivé, dans les mêmes conditions, d'éprouver l'impression, nette d'être touché par un être vivant ou un objet inanimé, ou bien d'entendre une voix humaine ?" La réponse est oui ou non. L'enquêteur note alors le nom et l'adresse du répondant, son sexe, l'occupation et l'âge. Une seule ligne est prévue par sujet et chaque formulaire comporte 25 lignes que l'enquêteur doit dater, signer et renvoyer à l'organisateur. Dans un second temps un formulaire "B" est utilisé pour recueillir, auprès de ceux qui ont répondu affirmativement, les réponses à 5 questions. Sur ce formulaire figure la mention "on ne publiera ni noms ni adresses, sans une permission spéciale". 5.700 réponses ont été collectées par Gurney notamment, mais ce nombre est insuffisant. "*Si chaque membre remplissait une fiche de 25 noms et persuadait un proche d'en faire autant, nous aurions 30.000 réponses et nous ne douterions pas de la possibilité d'atteindre les 50.000 qui nous sont nécessaires.*".

A Paris en août 1889, Richet adopte une position réservée lorsqu'il présente, en qualité de secrétaire général, les travaux du Congrès: "*Pour l'étude statistique des hallucinations, on sait que nos collègues de la SPR de Londres ont entrepris depuis quatre ans une enquête sur la fréquence des hallucinations. (...). A vrai dire ce n'est là qu'un travail préliminaire. Une enquête sur les hallucinations n'a pas, en elle-même, un aussi grand intérêt que l'étude détaillée, approfondie, de quelques unes de ces hallucinations.*"

Dans les rapports sommaires distribués aux membres du Congrès Marillier présente l'enquête d'un façon étonamment brève: "*La SPR (de Londres) a ouvert une enquête sur les hallucinations, dont le but principal est de déterminer la fréquence de ce phénomène chez les individus normaux. Les hallucinations des aliénés et des hystériques, les hallucinations du désir fébrile, les hallucinations qui sont le résultat d'une intoxication, les hallucinations hypnagogiques restent en dehors du cadre de ce recherches. Les premiers résultats de cette enquête seront communiqués au Congrès.*"
Pierre Janet, ainsi qu'une partie de la commission, objecte sur la distinction entre hallucinations d'un bien portant et hallucinations d'un malade, qui, d'un point de vue théorique, n'est pas défendable. Il plaide pour une étude moins centrée sur le rapport des oui et des non et plus soucieuse d'un examen poussé des réponses positives. Janet rejette en fait l'objectif central de l'enquête Il objecte encore sur la notation des nom et adresse, la signature de la personne recueillant les renseignements devant suffire.
Mais c'est au nom d'une machine déjà lancée, et en s'appuyant sur l'autorité de William James, que la commission décide de poursuivre l'enquête sous la forme prévue en intégrant toutefois, et d'une façon toute formelle, certaines objections de Janet, Delboeuf et Ballet.
L'enquête trouve cependant des encouragements auprès du congrès dans le sens où ce genre de manifestation doit servir à rassembler les forces. Une enquête internationale est assurément un objet de prédilection, d'autant plus que d'autres projets d'enquête sont en cours. On envisage de reprendre une enquête sur l'hérédité sous l'autorité de Galton, une enquête sur l'audition colorée. Plusieurs défenseurs de cette idée s'expriment, parmi lesquels Schrenk-Notzing de Munich qui développe un long plaidoyer en faveur d'une association des sociétés de psychologie.
Mais où en est précisément l'enquête ? Marillier fait le point: en Angleterre 2.036 réponses (il ne prend, apparemment, pas en compte les 5.700 réponses de Gurney), en France 345 réponses. Il ne fournit pas de chiffres pour les Etats-Unis où l'enquête est cependant en cours. "*Qu'un questionnaire prenant pour bon celui qui circule en Angleterre et en France, soit répandu dans les autres pays, et que les résultats de cette enquête soient centralisés entre les mains d'un comité désigné par votre commission d'organisation et nommé au prochain congrès de psychologie physiologique.*" Moyennant une légère concession, accordée à Delboeuf, pour mieux connaître les antécédents psychologiques des répondants, la proposition de Marillier est votée à l'unanimité par la commission.
Le congrès décide que la prochaine manifestation prendra le titre de Congrès international de psychologie expérimentale, qu'il se déroulera en août 1892 à Londres et que la SPR fera de son mieux mais, ajoute Myers qui vient de faire cette dernière proposition, "*je demande qu'on ne se prononce pas de peur de blesser des psychologues ou des physiologistes qui n'appartiennent pas à cette société.*"
D'août 1889 à août 1892, Sidgwick fait monter la pression. Au cours de l'année 1890 il fait plusieurs rapports provisoires. 6.481 réponses pour l'Angleterre, environ 2.000 pour les Etats-Unis, 633 pour la France et la Suisse. L'enquête est commencée en Allemagne mais ni en Russie, ni en Italie. Il faut 50.000 réponses pour 1892.
Il s'appuie sur les engagements de Paris et le soutien apporté par le Congrès.
Cette pression est relayée en France par Marillier bien sûr. Il fait passer des annonces dans la *Revue philosophique*, la *Revue scientifique*, *Le Temps*. La Société de Psychologie Physiologique à Paris se prononce en faveur de la poursuite de l'enquête. Dariex, directeur des *Annales des Sciences psychiques*, diffuse par sa revue les deux formulaires du questionnaire.

En août 1892 trois rapports sont présentés. Le premier par Sidgwick: 17.000 réponses au questionnaire en langue anglaise. La plus grande partie a été collectée en Angleterre, mais plusieurs réponses venant du Brésil et de Russie ont été incluses. Le second rapport est présenté par Marillier. 3.493 réponses venues de France, de Belgique, de Suisse, quelques autres d'Italie, du Mexique, de Russie. Le troisième rapport enfin, sous la responsabilité de W. James et de R. Hodgson, fait état de 6.311 réponses. Ses auteurs signalent que l'enquête est encore en cours.

Dans la publication complète des résultats de l'enquête que publieront les *Proceedings of the SPR* en 1894, Sidgwick ne traitera que les 17.000 réponses.

Les difficultés à obtenir des réponses, le "recrutement" des enquêteurs, la crédibilité à apporter aux résultats, fournissent des indications intéressantes sur l'écart existant entre le souci de validité et les savoirs-faire liés aux procédures d'enquête.

Si Sigwick peut conclure à un résultat probant dans le sens où les hallucinations véridiques sont plus fréquentes que le hasard, il demeure partagé quant aux effets politiques de la marque de la SPR sur le congrès de Londres. Avec Myers il souligne, devant les membres de la société, que la SPR cesse d'être tabou et a acquis une respectabilité aux yeux des allemands.

Documents utilisés:

- *Proceedings of the Society for psychical Research*
- *Journal of the Society for Psychical Research*
- *Revue philosophique de la France et de l'étranger*
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- *Premier Congrès International de Psychologie Physiologique, Août 1889*, Paris, Bureau des Revues, 1890
- *International Congress of Psychology. Second session, London, 1892*, pp.ix, 186, London 1892, in 8°

ABSTRACT : Between 1888 and 1892 a large international inquiry - census of hallucinations - is decided by the Londonian Society for Psychical Research. The SPR wants a scientific basis for "veridic hallucinations". The 1st International Congress of Psychology is used as a support to make a start and as a guarantee. England will get a new position among other countries and SPR will turn it to account. Differences between preoccupations about scientific validity and practice of surveys.

GRAND GUIGNOL ET PSYCHOPATHOLOGIE

Les Détraquées d'Olaf et Palau .

Le titre de la pièce tout comme le nom de ses auteurs, sont suffisamment excitants et énigmatiques pour donner le coup d'envoi à cette intervention .

Créée en août 1922 au Théâtre des Deux Masques, rue Fontaine, à Paris, repris en 1935, dans ce même théâtre, publié en 1956 dans la revue d'André Breton, Le Surréalisme, même, ce drame signé de deux noms aux allures de canular, serait tombé dans l'oubli sans le compte-rendu d' André Breton dans Nadja .

Que raconte la pièce ? Dans une institution de jeunes filles, une enseignante, Solange, en un élan de passion sadique, étrangle une élève de onze ans, après lui avoir fait subir différents sévices avec des épingles, des pointes, des plumes . Quand A. Breton voit la pièce, c'est Blanche Derval qui interprète le rôle : "Brune, châtain, je [illegible] . Jeune . Des yeux splendides, où il y a de la langueur, du désespoir, de la finesse, de la cruauté . Mince, très sobrement vêtue, une robe de couleur foncée, des bas de soie noire . Et ce rien de 'déclassé' que nous aimons tant" .

Qui est Olaf ? Qui est Palau ? Palau -le nom n'est pas sans évoquer d'orgiaques prémisses - est Pierre Palau (1883-1966), comédien, metteur en scène, auteur dramatique . S'il n'a écrit pour le répertoire du Grand-Guignol (répertoire sur lequel nous allons revenir) qu' une adaptation, celle de L'Ombre rouge de Joseph Kessel, sous le titre L'Icône qui s'éteint, il a produit plusieurs pièces pour le Théâtre des Deux Masques, le concurrent du Grand-Guignol, installé impasse Chaptal, entre 1921 et 1935 : des comédies (Le Coup d'essai, Mon frère de lait) et des drames (Solitude, La Peur, Les Avortées) .

Olaf, c'est le pseudonyme de Joseph Babinski (1857-1932), un élève de Charcot, le découvreur du fameux réflexe qui porte son nom . Curieusement, cette découverte a lieu en 1896, l'

année de l'ouverture du Théâtre du Grand-Guignol .
L'identité du savant ne fut révélée qu'au moment de la publicatio de la pièce, soit plus de trente ans après sa création , dans une postface rédigée par P. Palau : "L'idée initiale (de la pièce) m'a été inspirée par des incidents assez équivoques qui avaient eu pour cadre une institution de jeunes filles de la banlieue parisienne . Mais étant donné le théâtre auquel je la destinais -les Deux Masques- dont le genre s'apparentait au Grand-Guignol, il me fallait en corser le côté dramatique tout en restant dans l'absolue vérité scientifique : le côté scabreux que j'avais à traiter m'y obligeait . Il s'agissait d'un cas de folie circulaire et périodique, mais pour le mener à bien j'avais besoin de lumières que je ne possédais pas . C'est alors qu'un ami, le professeur Paul Thiéry, chirurgien des hôpitaux, me mit en relation avec l'éminent Joseph Babinski, qui voulut bien éclairer ma lanterne, ce qui me permit de traiter sans erreur la partie pour ainsi dire scientifique du drame" . Précisons que cet aspect scientifique de la pièce -il s'agit de manipulation d'objets pointus- est présent dans les didascalies et que, destiné à la représentation, le texte qui nous est parvenu et qui n'est pas celui de la création, n'est guère sensible dans ce que nous lisons aujourd' hui .

A. Breton fut, lui-même un élève de Babinski au Centre neurologique de la Pitié, de janvier à septembre 1917 . Dans Le Manifeste du Surréalisme, en 1924, il écrit l'admiration qu'il lui porte : "J'ai vu à l'oeuvre l' inventeur du réflexe cutané plantaire : il manipulait sans trêve ses sujets, c'était tout autre chose qu'un examen qu'il pratiquait, il était clair qu'il ne s'en fiait à aucun plan . De-ci, de-là, il formulait une remarque, lointainement, sans pour cela poser son épingle, et tandis que son marteau courait toujours . Le traitement des malades, il en laissait à d'autres la tâche futile. Il était tout à cette fièvre sacrée".

Il convient de rappeler que le début du siècle n'établissait pas de frontières aussi étanches qu' aujourd'hui entre scientifiques et littéraires . C'est ainsi

qu'un certain Professeur Roger, médecin aliéniste, a écrit une pièce, L'Enquête, mise en scène par André Antoine au Théâtre Libre et reprise au Grand-Guignol en 1932 . Elle raconte comment dans une crise de fureur épileptique, un juge d'instruction a tué l'amant de sa femme . Ses crises sont soudaines et ne laissent rien dans la mémoire . C'est précisément ce phénomène de dédoublement qui intéresse le Grand-Guignol .

Le Grand-Guignol, qu'est-ce que c'est? C'est le théâtre d'épouvante et de rire, qui s'est constitué, de 1896 à 1962, impasse Chaptal, à Paris, dans le quartier de Pigalle . Il pratique l'alternance de comédies et de drames, à raison d'au moins quatre pièces par soir, selon la tradition du "spectacle coupé" . La référence à Guignol, la célèbre marionnette lyonnaise est celle qu'à retenue l'inventeur du genre, Oscar Méténier, un "chien de commissaire". Comme Guignol, porte-parole des canuts, O. Méténier a été victime de la censure Le milieu des "apaches" dans lequel se déroulaient ses pièces, de même que le parler argotique qu'il utilisait, empêchaient la représentation de ses pièces au Théâtre Libre . Mais O. Méténier est un Guignol qui a grandi, un Grand-Guignol : il prend la liberté d'ouvrir son propre théâtre pour faire jouer ses propres pièces . Le genre s'est élaboré au début du siècle à partir d'une pièce de Jean Berleux, Carrier, horloger-bijoutier (1901), qui proposait un pendu sur la scène . André de Lorde (1869-1942) a suivi la veine noire et sanglante avec une adaptation d'une nouvelle d'Edgar Poe, Le Système du Dr Goudron et du Professeur Plume en 1903 .Les thèmes du Grand-Guignol tournent autour du fait divers (méprises, vengeances) et de la médecine (Le Chirurgien de service de Léo Marchès, qui dénonçait la mort d'un malade par manque de soins, Paris n'ayant, toutes les nuits, qu'un seul chirurgien de service, fut à la base d'une réforme dans l'Assistance Publique), avec l'accent mis sur l' aspect psychiatrique . Pour être plus précis et plus scientifique dans son oeuvre, A. de Lorde a accepté la collaboration d' Alfred Binet qui était venu mener auprès de lui son enquête, comme il l'avait déjà fait auprès de François de Curel, de Paul Hervieu, de Victorien Sardou . Tous les deux sont fascinés

par les phénomènes de dédoublement au point de pratiquer eux-mêmes : A. de Lorde est une personnalité parisienne, beau-fils du tragédien Mounet-Sully, Conservateur de la Bibliothèque de l'Arsenal . Un journaliste en fait le portrait suivant, en s'étonnant de la dualité de son personnage : "Les rares travailleurs qui consultent des livres à la Bibliothèque de l'Arsenal, ont dû y rencontrer, les jours où son service l'y amène, un jeune attaché, gras, rose et petit . Un lorgnon chevauche son nez, cachant ainsi un peu des yeux sans cesse mobiles, et sur les lèvres flotte toujours un sourire heureux : il a la figure rasée d'un ecclésiastique qui aurait abandonné le froc pour devenir acteur . C'est l'homme le plus gai du monde, et c'est lui pourtant qui a donné au théatre contemporain les pièces les plus effrayantes" . Quant à A. Binet, il ne se présentait pas impasse Chaptal sans sa barbe postiche pour ne pas être reconnu. Si la collaboration de Babinski et de Palau fut épisodique, celle de Binet et d'A. de Lorde s'échelonna sur plusieurs années avec la production de plusieurs pièces : L'Obsession (1905), Une leçon à la Salpêtrière (1908), L'Horrible Expérience et L'Homme mystérieux (1909), Un crime dans une maison de fous, publié sous le titre Les Infernales, Les Invisibles (1912)et vraisemblablement une pièce dont A. de Lorde n'a pas souhaité la représentation, mais par contre la publication comme faisant partie de son "théâtre médical" : Les Charcuteurs.

Leur thèse par rapport à la folie : c'est un phénomène de dédoublement . Le fou, c'est l'homme mystérieux, dont on ne peut présumer des actes : "Les fous ne sont pas ce que le public s'imagine . Leur déséquilibre n'offre pas toujours des caractères précis et il est souvent malaisé de déterminer la frontière qui les sépare de l'homme normal . Parfois même, au premier abord, rien ne révèle la folie . Vous trouverez dans les asiles des pensionnaires qui raisonnent admirablement ; interrogez l'un d'eux, vous constaterez chez lui une suite parfaite dans les idées, et vous penserez, indigné : 'Cet homme n'est pas fou ! Que fait-il ici ?' Mais qu'une phrase, un mot prononcés par hasard viennent déclencher le ressort de l'idée fixe, vous serez témoin d'une transformation imprévue . Un démon terrible se dissimulait chez cet être si calme d'apparence ; soudain déchaîné, il le possède totalement

et ce n'est plus un homme paisible que vous avez devant les yeux, c'est une bête féroce". (La Galerie des monstres) .

Ce point de vue sur le fou que partagent de Lorde et Binet n'est pas seulement un ressort dramatique augmentant le mystère qui entoure le déséquilibré, c'est une position militante dont le théâtre se fait l'écho : "Ce mystère dont s'enveloppe le fou, voilà ce qui le rend dangereux, car il s'est formé chez beaucoup de gens une conception des plus inquiétantes, celle du fou-victime . Sous l' influence d'un humanitarisme un peu excessif, on est arrivé à considérer les médecins aliénistes comme des bourreaux, ou du moins comme des monomanes en proie au besoin de voir partout des déments . Cette idée singulière a fait son chemin ; le législateur, les tribunaux s'en sont peu à peu inspirés, et voici le résultat : il ne se passe point de jour que nous ne trouvions à la rubrique des faits divers le récit de quelque crime commis par un aliéné relâché après un court séjour à l' asile . Certes l'internement d'un malade doit être entouré de toutes les garanties possibles, et les fous, comme les demi-fous, ont droit à notre sollicitude . Mais cette sollicitude, peut-être conviendrait-il de l'étendre aux infortunés dont nulle tare n'entame la raison et qui sont exposés chaque jour à tomber sous les coups d'un fou en liberté" . (A. de Lorde) Cette position est grandguignolesque à souhait : suffisamment frileuse pour plaire au public petit bourgeois qui fréquentait l'impasse Chaptal .

L'enseignante des Détraquées ne présentait , elle non plus , aucun des stigmates présumés de la folie . Et pourtant ... Même si la pièce n'a jamais été représentée au Grand-Guignol, elle fait bel et bien partie du genre . D'ailleurs, le médecin s'appelle le Dr Bernier, un nom que l'on retrouve dans Les Infernales .

Pourtant jouée 278 fois, la pièce ne reçut que des critiques à sa création ; Roland Dorgelès, le premier, s'indigne : "Une ordurette en deux actes, Les Détraquées, dont je ne veux même pas nommer les auteurs : ils ont

peut-être une famille . Mes lecteurs me sauront gré certainement de ne pas leur rendre compte de cette saleté . Tout permet d'ailleurs de supposer qu'il se trouvera une clientèle -et quelle ! - pour aller voir ça" . A. Breton, par contre, s' enflamme : "Bravant mon peu de goût pour les planches, j'y suis allé jadis (au Théâtre des Deux Masques), sur la foi que la pièce qu'on y jouait ne pouvait être mauvaise, tant la critique se montrait acharnée contre elle, allant jusqu'à en réclamer l'interdiction . (...) . Je ne tarderai pas davantage à dire l'admiration sans borne que j'ai éprouvée pour Les Détraquées, qui reste et restera longtemps la seule oeuvre dramatique (j'entends : faite uniquement pour la scène) dont je veuille me souvenir ."

Une trentaine d'années après la publication du texte par A. Breton, Jacques Baratier en a fait un film (1986) . L'action se déroule, en 1920, aux Fauvettes, une pension de jeunes filles . Toutes ces demoiselles, emportées transportées par la chaleur de l'été, y découvrent, sous la haute surveillance de la directrice (Ingrid Caven) les charmes de leur corps . Il y a aussi l'abbé Pioch (Daniel Mesguich) et le docteur Aubryon (Topor), l'inquiétant inspecteur Levron (Michel Albertini) et Solange (Catherine Jourdan) .

Les Détraquées représentent un bon exemple de la collaboration entre littéraires et scientifiques . Pour la mécanique précise de ses pièces, le Grand-Guignol avait besoin du secours de "chirurgiens" . Non seulement pour le texte, mais pour la mise en scène qui exigeait le cumul de petits faits vrais pour mener plus sûrement le spectateur à l'épouvante .

Agnès Pierron .
Paris, Pâques 1994.

Probst, University Hamburg, June 15, 1994, 13th Cheiron Paris

Zur Position Wilhelm Weygandts innerhalb der internationalen Eugenikbewegung vor dem Ersten Weltkrieg.

13th Cheiron Europe Annual Conference /
13e Congres annuel de Cheiron Europe
7- 11 September 1994, Paris

Paul Probst
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SUMMARY

The Position of the German Psychiatrist and Psychologist Wilhelm Weygandt on the International Eugenic Movement Before World War I

Historically, eugenics goes back to the British anthropologist Francis Galton. Eugenics was a heterogeneous agglomeration of sciences, welded together by social movements. Eugenics aims at the improvement of mankind by controlling human reprocuction. We can distinguish between three forms: "main-line eugenics", "reform eugenics" ,and "racist eugenics".

Wilhelm Weygandt (1870- 1939), student of Wundt and Kraepelin, was psychiatrist and expert in mental hygiene. In this paper, his medical as well as psychological publications referring to eugenic topics are analyzed. Weygandt's position on the international eugenic movement will be discussed.

13TH CHEIRON EUROPE ANNUAL CONFERENCE
7 –11 SEPTEMBER 1994
PARIS

ABSTRACT:
An analysis of changing conceptual frameworks in relation to changing social and political circumstances in the ideas of Graham Wallas. Wallas was a liberal British intellectual who approached the problems of political conduct in mass society in the post Victorian period. Critically, Wallas attempted to interconnect the disciplines of social psychology and political science in response to the needs of social organisation and the modernisation of culture.

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LES REVUES DE L'INSTITUT D'ORIENTACIO PROFESSIONAL DE BARCELONA (1920-1937).

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Abstract:

Our work analyses the journals of the Institut d'Orientació Profesional of Barcelone (Professional Orientation Institute of Barcelone) a reliable exponent of the first spanish applied psychology. These journals was the divulging organ of the works of Emilio Mira and other research workers conected with the Institute and there are essential as a primary documentation to know the beginnings of this work area in Spain.

PAPER ECOLOGIC

LES REVUES DE L'INSTITUT D'ORIENTACIO PROFESSIONAL DE BARCELONA (1920-1937).

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Les revues de l'Institut d'Orientació Professional (I.O.P.) de Barcelone possèdent un grand intérêt car elles sont des fidèles représentantes de la première psychologie appliquée espagnole et qu'elles constituent une documentation essentielle pour la connaissance des débuts de ce domaine dans notre pays (Sáiz et Sáiz, 1990), mais nous ne pouvons pas parler d'elles sans faire, d'abord, un brève référence au contexte social et culturel dans lesquel elles se sont développées, et sans expliquer ce qu'a été et ce qu'a signifié l'I.O.P.

L'I.O.P., appelé plus tard Institut Psicotècnic de la Genaralitat de Catalunya (Institut Psychotechnique de la Generalitat de Catalogne), provenait de l'ancien Secretariat d'Aprenentatge (Secrétariat d'Apprentissage), né dans un milieu politique et social favorable. En Catalogne, dûe à son caractère industriel et à l'existance de la "Mancomunitat" (organe de Gouvernement autonome qui orienta de nombreuses institutions vers le renforcement de la culture catalane), la politique dirigée spécialement vers la rénovation pédagogique et l'insertion des jeunes dans la vie sociale et laborale fut favorisée. Dans cette atmosphère, le dénommé "Secretariat d'Aprenentatge" fut crée en 1914, dans le but d'orienter les jeunes à choisir leurs professions, devenant l'un des centres pionniers de l'orientation professionelle, vu qu'il n'existaient que deux prédécesseurs, celui de Parsons, en 1908, à Boston et celui de Cristhiaens, en 1912, à Bruxelles. Ce Secrétariat fut donc, comme nous venons de le dire, le prédécesseur directe de "l'Institut d'Orientation Professional" (Institut d'Orientation Professionnelle) crée en décembre de 1918, qui bénéficia depuis ses débuts de l'appui instituionnel, et qui jouit d'une organisation soigneuse et d'une bonne dotation d'appareils et matériels.

L'I.O.P., lors de son développement, a supporté trois étapes liées aux événements historiques et politiques qui ont agité l'Espagne, et qui lui ont donné ses différents noms:

1er. De 1918 à 1923 (depuis sa fondation jusqu'à l'arrivée au pouvoir du Général Primo de Rivera): Institut d'Orientació Professional.

2ème. De 1923 à 1930 (époque de la Dictature du Général Primo de Rivera): il se convertit en 1927 en la "Seccion d'Orientacion et Seleccion Profesional de la Escuela de Trabajo de la Diputación de Barcelone" (Section d'Orientation et de Sélection Professionnelle de l'Ecole du Travail de la Diputation de Barcelone).

3ème. De 1933 à 1936 (depuis de la II République jusqu'à la Guerre Civile): il devient l'Institut Psicotècnic de la Generalitat de Catalunya.

L'objectif principal de l'Institut était d'offrir au citoyen un service gratuit à partir duquel une orientation était proportionnée aux jeunes pour qu'ils puissent se prononcer sur une profession appropiée. Quand un jeune arrivait à l'Institut en quête d'information, il était reçu et on lui ouvrait une fiche dans laquelle ses caractéristiques générales étaient précisées. Ensuite, il était dirigé ver le Laboratoire Médical-Antropométrique où ses aptitudes physiques étaient définies, et, postérieurement, on l'envoyait au Laboratoire Psychométrique, où ses aptitudes mentales étaient évaluées. Le résultat des observations réalisées dans ces services s'ajoutaient dans la fiche. Avec toutes ces données, le responsable de la section d'information pouvait donner son opinion au sujet de la profession convenant le mieux au futur apprenti (Sáiz et Sáiz, 1993). L'Institut a eu, donc, une fonction essentiellement orientatrice, mais, en plus, cette tâche a été accompagnée. d'une function précise de recherche.

L'I.O.P. a joui d'une reconnaissance internationale de la part des hommes et des instituts psychotechniques de l'époque par son sérieux, sa bonne organitation et son méticuleux travail de recherche. Nous trouvons un exemple de ce fait avec Claparède, qui, à son premier contacte avec l'Institut, avait été agréablement étonné de sa qualité (Anonyme, 1920a; Borrás, 1955; Kirchner, 1975; Sáiz et al., 1994; Siguán, 1981): "*Nous qui l'accompagnions* -se référant à Claparède- *nous nous sommes rendus compte tout de suite de la surprise agréable que lui produisait cette visite, jusqu'à tel point qu'il dit: c'est l'Institution d'Orientation Professionelle la plus complète que je connaisse, sans doute la meilleure d'Europe*" (Mallart, photocopie jointe au rapport de Borrás, 1955). De plus, dans un petit article du Journal de Génève du 16 de septembre de 1920, il eut l'occasion pour commenter, de nouveau, le savoir-faire qu'avait l'I.O.P. et la Catalogne dans cette matière: "*A une époque comme la nôtre, où il s'agit avant tout de produire, de produire vite et de produire bien, personne ne saurait se désintéresser de ce mouvement. Certains Etats l'ont compris, notamment celui de Catalogne, qui accorde un crédit annuel de 40,000 fr. au bel Institut d'Orientation de Barcelone, que j'ai eu l'occasion d'admirer récemment*". (Claparède, 1920).

L'Institut fut un centre actif pour le développement de la Psychologie appliquée en Espagne (il établit des accords avec différentes institutions et réunit une grande quantité de tests et d'instruments de mesures qu'ils ont addapté à ses nécessités, et à la fois, des tests spécifiques furent crées) et il se convertit en une plate-forme pour l'établissement des relations avec des groupes de recherche en Psychologie de majeure partie de l'Europe et de l'Amérique, permettant ainsi l'ouverture des frontières de la Psychologie espagnole. Ainsi, il se créa un noyau de collaborateurs stables, l'un des canaux de formation des neuveaux professionnels, et il n'hésita pas à divulguer ses travaux, et pour cela, en plus, de la participation active à toutes sortes

de rencontres internationales, il créa une revue pour publier les travaux en cours de réalisation au sein de l'Institut, avec un clair intérêt divulgateur. Cet acharnement à ouvrir l'I.O.P. reste clairement reflété dans le premier numéro de sa revue, qui naquit en 1920 sous le nom de "Anals de l'Institut d'Orientació Professional".

"Au point de compléter cette mission initiale depuis l'intérieur, le moment est venu de regarder vers l'extérieur: ouvrir toutes grandes ses portes pour qu'il puisse, d'un côté, montrer qui il est, ce qu'il veut faire et comme il pense le faire, et de l'autre, recevoir (...) aux uns, les intéressés (...) et aux autres, les techniciens apportant leur collaboration (...) Une autre finalité (...) est de faire connaître, en forme graphique et périodique, l'Institut se proposant une tâche d'échange et de coopération" (Direction des Annales, 1920b).

Le reveu eut différentes périodes asociées au moment politique, et pour cela, elle souffrit des discontinuités et porta différents noms. Comme nous l'avons indiqué ci-dessus, elle naquit en 1920 sous le nom de "Anals de l'Institut d'Orientació Professional" éditant sept numéros entre 1920 et 1923, elle disparut quelque temps à cause de la suppression des subventions de la part des organismes catalans qui furent eliminés par la Dictature du Général Primo de Rivera, et elle réapparut de nouveau en 1928 sous le nom de "Anales de la Sección de Orientación de la Escuela de Trabajo" publiée en castillan avec seulement trois numéros. A l'instauration de la II République, en 1931, l'Institut ne dépendit plus de l'Ecole de Travail et il se convertit en l'Institut Psicotècnic de la Generalitat de Catalunya, créant une nouvelle revue -conti-nuation claire des antérieures- editée en collaboration avec le Séminaire de Pédagogie de l'Université de Barcelone, sous le nom de "Revista de Psicologia i Pedagogia" (Revue de Psychologie et Pédagogie), publiée en langue catalane, mais avec l'inclusion de quelques articles en autres langues. Bien que cette revue continue en tant que porte-parole de l'Institut, elle possède aussi un caractère clairement différent. Nous pouvons lire les desseins iniciaux à travers les paroles de Emili Mira, recueilles textuellement:

"La "Revista de Psicologia i Pedagogia" procurera recueillir dans son texte tout ce qui se passe dans notre pays en relation avec la Psychologie et l'Education. Pour cela, elle prendra contacte avec les Institutions et les personnalités nationales et étrangères qui se dévouent à sa culture. Elle tentera, de plus, d'être, chaque jour venant, une source d'information et d'orientation "(Mira, 1922, p.1).

La revue débuta en 1933, publiant quatre numéros par an, en s'éditant régulièrement jusqu'en 1937, moment où elle s'interromput, sans aucune indication explicite, au numéro 19, probablement dû aux tensions de la guerre.

Les trois périodes de la revue ont en commun la publication d'articles originels, mais, en plus, toutes possèdent un alinéa de nouvelles sur des congrès, des actes, des accords et des manifestations en relation avec la Psychologie et la Pédagogie, en plus de présenter une information bibliographique. Cependant, les revues différent dans le contenu de leurs articles, qui concorde avec l'evolution qu'a vécue le propre Institut et la propre Psychotechnique, ainsi, alors que les premiers numéros de la revue ont un caractère franchement divulgateur du type d'organisation et de fonctionnement de l'Institut, les derniers numéros présentent des articles de réflexion ou de recherche, beaucoup plus semblables aux revues contemporaines.

L'analyse des articles 153 originels publiés dans la revue (Anals = 44 articles; Anales = 16 articles et Revista de Psic. i Pedag. = 93 articles), permet de vérifier les thématiques traitées, ainsi que les auteurs les plus productifs. L'analyse des thématiques nous rend compte que la revue répond à un intérêt psychotechnique notoire, vu que la plupart de ses travaux se refèrent à la sélection et à l'orientation professionnelle (voir table 1). L'analyse des auteurs qui ont publié dans la revue démontre, que pareillement à d'autres revues de l'époque, les auteurs les plus produtifs sont ceux qui furent en relation avec la dynamique de l'I.O.P. (voir table 2)

TABLE 1. Thématiques.

THEMATIQUE	Nº ARTICLES	%
Psychotechnique, orientation et sélection professionelle et organisation du travail.	82	53,60
Pédagogie générale	42	27,45
Psychologie générale et infantile	21	13,73
Conférences	8	5,22
TOTAL	153	100 %

TABLE 2. Auteurs le plus productifs.

AUTEUR	Nº D'ARTICLES
MIRA, E.	18
TRIAS DE BES, LL.	12
SOLER DOPFF, C.	10
GRANADA, A.	9
RUIZ CASTELLA, J.	7
COSSIO, M. B.	5
ALIER, J.J.	4
AZOY, A.	4
MORAGUES, J.	4
ROURA, J.	4

L'analyse détaillée de la revue permet de vérifier, à travers la lecture de chacun de ses articles, l'état actif de la Psychologie appliquée en Espagne, au début du siècle, le fonctionnement et l'organisation de l'I.O.P., ainsi les techniques et les systèmes de mesure utilisés, d'observer en plus, du fait de la présence de quelques photographies, les installations et les appareils qui s'utilisaient, ce qui permet de constater le bon niveau atteint par l'I.O.P.. Tout cela nous conduit à affirmer que cette revue doit être considérée comme l'une des sources primaires fondamentales pour l'étude historique de cette période de la Psychologie en Espagne.

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THE ROLE OF HANS EYSENCK IN THE PROCESS OF THE INSTITUTIONALIZATION OF CLINICAL PSYCHOLOGY IN BRITAIN

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INTRODUCTION

Nowadays, the idea of a clinical psychologist without an organished post-graduate training is strange and exceptional, however it was not till the 50's when this kind of training was considered essential. Before 1950, the training of clinical psychologists in Britain, was unsatisfactory. There were devoted and brilliant pioners, but practically training was mostly on the apprenticeship system.

Hans Eysenck was a leading figure for the stablisment of this status. The impetus given by the Maudsley School in changing the role of the clinical psychologist, based largely upon the Eysenckian theory that neurotic disorders are learned and amenable to treatment with learning techniques, was a great contribution. The existence of a strong, scientifically based, and productive training department acted as a catalyst and example in the development of clinical psychologysts.

In England , pre-war developments were almost entirely in the area of educational psychology ; work with adults was hardly done by psychologits at all, no training courses for such work were available. The situation changed slowly after the Second World War and since then over a dozen heads of departments have had their postgraduate training at the Institute of Pychiatry, but some of this significance can only be seen retrospectively, by looking back over the outline of his life.

CLINICAL PSYCHOLOGIST

Eysenck was officially charged at the end of the Second World War with the creation of a profession of clinical psychology in Great Britain, and he started the first academic training course in that discipline at the Institute of psychiatry in the 1950's.

No such profession of clinical psychology existed in Britain, hence Aubrey Lewis encouraged Eysenck to visit the United States to study the practice there, and to return to found the first university based training course for clinical psychologists at the Institute of Pychiatry. The visit to the University of Pennsylvania (1949-1950), had an important effect on Eysenck's future career, and indeed on the future of clinical psychology and psychiatry in Britain. Among its major influences was that it brought Eysenck in contact with an environment in wich psychoanalysis has made an extraordinary powerful impact on psychiatry, clinical psychology and the outlook of many educated people. This set on the course of a long struggle againts psychoanalysis because it outraged his concept of psychology as a truly scientific discipline.

On his return to England, few years later in 1952, Eysenck wrote a paper on "The Effects of Psychotherapy". This paper was conceived as a presentation of the assessed effects of various psychotherapeutic treatments, and was seen as a tremendous blow against psychoanalytic therapies. A year later 'Uses and Abuses of Psychology' appeared as the first presentation of Eysenck's alternative to Psychodinamic Therapy. Eysenck was one of the first to welcome and to push forward the new approach of 'behaviour therapy'. His training and advocacy are one of the main reasons why they now form part of the weaponry of any up-to-date psychiatric service. Giving such therapy does not require the skills of the medically qualified psychiatrist. Behaviour therapy is therefore one of the fields in which clinical psychologist serve a fully responsible function, rather than providing test results for the use of the psychiatrist.

After his visit during his stay in the United States to many psychological departments, Eysenck found what he called the subservience of psychologyst to doctors. In an earlier paper he discussed the function and training of these two professions (Eysenck,1950) and forty years later he said that the clinical psychologist should be an independent professional and not in any sense subservient to the psychiatrist, or to be considered in terms of a medical auxiliary. (Eysenck, 1990, p.127)

In England, psychiatry is often referred to as "medical psychology", indicating that psychiatry (or at least a large part of it) is neither more nor less than applied

psychology. Precisely because of this close and overlapping relationship, there have been many arguments, discussions and even legal quarrels between the two disciplines and there is no agreed line of demarcation between them. (Eysenck and Rachman , 1973, p.1)

The two professions had litle knowledge of each other, and perhaps litle desire to know each other. It seemed desirable and historically justifiable to split psychiatry into two independent parts, one concerned with organic disorders and their treatment, which in turn was largely medical in nature; the other concerned with behavioural disorders and their treatment, which in turn was largely behavioural. It was argued that the former discipline should be prerogative of medically trained psychiatrists, while the latter should be the prerogative of non-medical psychologists trained in special courses for post as clinical psychologists (Eysenck, 1975)

It is recomended that the equal and independent existence of these two groups of workers (psychiatrist and clinical psychologist) should be recognized, each having independent access to patiens satisfying the requirements of their particular expertise, but that both groups should work together, in harmony contributing their special kind of knowledge for the good of the patient. The precise nature of such co-operation would naturally have to be worked out in the course of such mutual interaction, but there must not, in the nature of the case, be any automatic assumption of authority or leadership based on medical competence.

TRAINING IN CLINICAL PSYCHOLOGY.

Aubrey Lewis thought about the creation of a sub-department of psychology which would form part of the department of psychiatry with Hans Eysenck as director. His main job would be to introduce clinical psychology as a profession.

As soon as Hans Eysenck started as a director of the Institute he incorporated new plans for teaching and research under three headings, Research, Teaching and Clinical work. The teaching activities were directed towards psychiatrists, clinical psychologits in training and postgraduate students doing research for their Ph.D, or other higher degrees. The overall head of the department was Eysenck. The department was, however, split

into a Research Section and a Clinical-Teaching Section. The Research Section was primarily concerned with work relating to Eysenck's dimensional system. The Clinical-Teaching Section was primarily concerned with the training of clinical psychologists by means of the postgraduate diploma in clinical psychology . In all courses Eysenck trained psychologists tended to emphasise individual differences.

In this sense, Eysenck put forward a number of arguments against the belief that psychotherapy was part of the clinical psychologist's role. The team of psychiatrist - psychologist-social worker constituted according to his oppinion a combined attack on a problem, based on specialization of functions. In this team, the psychiatrist was responsible for carryng out therapy, the psychologist for diagnostic help and research design, and the social worker for investigation of social conditions in so far as they affect the case. Training courses should concentrate on those areas in wich the psychologist could make his most significant contribution to the psychiatric team.

What can hardly be denied is the fact that behaviour therapy serves as an excellent illustration of the point that clinical psychologits can make a genuine contribution to psychiatry by making use of the principles and laws of psychology, and that in this way they are able to add in a unique manner to psychiatric knowledge and methodology. (Eysenck, 1965, p.40,41)

The first course of training in clinical psychology was started at the institute of Psychiatry (Maudsley and Bethlem Royal Hospitals) in 1948, and since them similar courses, though on a smaller scale, have been started in Liverpool, Glasgow, and also in clinics and institutes lacking University affiliation. The transformation of clinical psychology which has taken place since 1950 is therefore a process in which Eysenck's department played a great part, but there were other trends which were recognised in that department, and which were a lesson to more traditional academic departments.

Eysenck is a widely known name, and his former students of the world. He is the author of at least seventy books, quite apart from a prodigious output of journal articles, chapters of books and other kind of documents . The inmense volume of work produced by Eysenck and his co-workers absolutely stultifies any attempt to describe or summarise it. He has been not only an enormous prolific researcher in most of the fields of psychological theory but also a controversial writer. Some of the questions dealt by Eysenck has been converted in a well known controversial questions, that have originated broad discussion and have given a great visibility to Esenck'ideas among the scientific community. The study of general social attitudes, the question on intelligence or Eysenck'ideas on psychosomatic medicine are some of the most debatable questions.

Citation frequency counting is one of the most widely acknowledged ways to assess the impact of one author upon the scientific comunity. For many years past, the name of H.J.Eysenck has far outdistanced all other British psychologist in the S.S.C.I. This index provides the backing for a claim that Eysenck is the most influential of psychologists writing in this country since world war II. It has also been shown by Ruston et al. (Rushton, 1977) that in a specific study of departments of psychology in Britain, Eysenck was the most cited psychologist, according to recent literature covered by the S.S.C.I.

The evolution of Eysenck's impact (SSCI data 1966-1988) , as measured through number of citations in S.S.C.I. shows for Dr.Eysenck's works a line rising moderatly along the years till a total of 15985 citations. (Sanchis, 1993). Eysenck's average is about seven hundred citation per year and it may be not forgotten that the average author in S.S.C.I. receives three citations per year and eminent authors are receiving in turn of one hundred references each year (Tortosa and cols.,1988).

H.J.Eysenck constructed a number of instruments to assess the personality in order to found his theories. The great visibility reached by Eysenck's contributions upon the scientific comunity seems largely due to the generalized use of some of these tools

created by this author for personality assessment tasks. The main fields in which his influence became more visible are Clinical psychology, Personality and Psychotherapy.

It is also evident in the influence he has had on the application of scientific psychological method to many areas by those who studied under him.

CONCLUSIONS

The transformation of clinical psychology which has taken place since World War II in Britain was a process mainly due to Hans Eysenck and the psychologists who worked under his supervision and now direct many psychological departments in this country. Eysenck's impact on different scientific fields show the important role of this german author upon the instutionalization of the clinical psychology .

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ABSTRACT

Hans Eysenck was a leading figure for the stablishment of the training of clinical psychologists in Britain. The impetus given by the Maudsley School based in his theories was a essential contribution for the development of clinical psychology.

Eysenck 's impact upon scientific community is a clear proof of the importance of this author.

ABSTRACT

Continuities and priorities: Alfred Binet (1857-1911) and William Stern (1871-1938)

Wilfred Schmidt, University of Alberta

In 1911 Stern described the creation of the Binet-Simon test of intelligence as the greatest advance in educational psychology. The research that led to this breakthrough was, however, part of a much wider search for identifying and understanding individual differences, to which Binet was making significant contributions since 1890, while Stern published his first programmatic statement on individual differences and "ideas towards a differential psychology" in 1900. This paper deals with some of the seminal contributions of Binet and Stern up to 1912, emphasizing continuity, commonalities and differences, and priorities. The role of cooperative research on a large scale, international and interdisciplinary in nature, on issues in differential and applied psychology, is highlighted.

Continuities and priorities: Alfred Binet (1857-1911) and William Stern (1871-1938)

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> A short time ago - October 18, 1911 - the gifted and highly esteemed creator of the method [of age gradation in intelligence testing] died. His all-too early demise, that we mourn most bitterly, compels others now to pick up the threads that he had spun (Stern, 1912, p.21, as translated by Whipple, 1914, p. 31).

This is what Stern said in the paper he read at the Fifth Congress of Experimental Psychology in Berlin in April, 1912, only six months after the death of Binet. It was in this paper, expanded into a monograph *Die psychologischen Methoden der Intelligenzprüfung*, that Stern introduced the concept of the intelligence quotient (Stern, 1912, p. 28). I have started with this quotation, because it reflects a sense of continuity and of belonging to an international community of psychologists at a time when an important breakthrough in the testing of intelligence had occurred. But, and it is important to stress this, intelligence tests were only one offshoot of the much wider field of the study of individual differences, which Binet and Stern pursued with such intensity. Two other offshoots, in those early years, were child psychology, and the study of memory and of suggestibility. Binet's study of individual differences was inextricably bound up with his work as an experimental child psychologist, and Stern saw the study of differences between children at different developmental levels (preschool, school, puberty and adolescence) as part of *differential psychology*, which was the new term that he had suggested in the subtitle of his first venture into the field of individual differences (Stern, 1900), and which he used as the title of the most *systematic* and incisive treatment of the methodological problems in this field at that time (Stern, 1911). Around the turn of the century the study of individual differences in memory and in suggestibility attained prominence in European countries (less so in

North America). Moreover, all three of these "specializations" of psychology were pursued with a keen eye for their relevance for cultural problems: problems in public education and in the administration of justice. What I propose in my paper is to trace Stern's involvement in the new field of individual differences (excluding child psychology) from 1897 to 1912, paying particular attention to continuities, priorities, and commonalities and differences between Stern and Binet.

For his Dr. habil dissertation Stern presented a series of reports on the experimental work that he had carried out on the perception of change (Stern, 1898). Part of the requirement for habilitation was a colloquium, where he had to lecture on a topic of his choice. He chose "Tasks and methods of a psychology of individual differences". Soon he thought of expanding his colloquium lecture into a brochure, "for apart from Kraepelin noone in Germany has written anything *systematic* about the topic" (letter to Jonas Cohn, July 1, 1897). Three months later he set his aims higher. He now spoke of plans for a "differential psychology", and he invited Cohn to co-author "Studies in differential psychology", adding "in Germany a great deal of energy ought to be devoted to pursuing studies in this field." The co-authorship did not materialize, but in July 1898 Stern wrote that he intended to complete a monograph before the end of the summer holidays. It took him a little longer; in the following year he published a short paper on the differential psychology of judgement (Urteilen), and on Dec. 9, 1899 he informed Cohn that his monograph on differential psychology would appear in the spring of 1900, which it did. In this book Stern surveyed the study of individual differences worldwide, and made suggestions for many research projects. One such project envisaged experiments on accuracy of recall.(Gedächtnistreue), and Stern set about immediately to carry out this research himself (letter to Jonas Cohn, dated 31st July, 1900), and he asked Jonas Cohn to help him by finding additional subjects for the research and carrying out the necessary experiments according to the procedures laid out by Stern.

Continuities Binet/Stern

Meanwhile Binet had been carrying out a long series of experiments on suggestibility, culminating in the publication of his book on that topic (Binet, 1900), i.e. in the same year as Stern was planning his research on accuracy of recall.

Like Binet, Stern always felt a need for co-researchers. The need for *organized cooperative research on a large scale* in the whole field of differential psychology was very much on Stern's mind at this time. He prepared a paper on this topic for the International Congress of psychology in Paris in 1900; though he did not attend the congress, it was included in the Proceedings edited by Janet (Stern, 1901).

Sterns paper on accuracy of recall appeared in January 1902 in a legal journal (Stern, 1902). In the foreword to this paper Stern reiterated his call for organized cooperative research, this time directing it to researchers in the field of *applied* psychology, and within that field in particular to those sharing his interest in accuracy of recall and related issues. There was an excellent response.. In the following year the first number of the *Beitrage zur Psychologie der Aussage* (Contributions to the Psychology of Testimony) appeared. It listed an impressive editorial board, which included among others Alexius Meinong (Graz in Austria) and Gerard Heymans (Groningen in the Netherlands), and contained also a pledge of cooperation from some thirty interested psychologists, psychiatrists, educators, criminologists, members of the legal profession including judges, and historians. The response ensured a truly interdisciplinary approach to the studies to be undertaken. In passing we may note that interdisciplinary approaches to the understanding of human and cultural issues were characteristic of Stern throughout his career. The first number of the *Beitrage* featured eight papers, including two by Stern himself: one on the nature of applied psychology, its possibilities and limitations, the other on the aims of research on the psychology of testimony.

The *Beitrage zur Psychologie der Aussage* were immensely effective in mobilizing research, not only in Germany, but also in Russia. In 1905 a professor of law in Kasan

reported in the *Beitrage* on Russian literature and experimental research on the psychology of testimony (Zavadskij, 1905). From that report we learn that already in 1902 a number of members of the legal profession had recognized the significance of Binet's studies of suggestibility (1900) and Stern's research on the accuracy of recall (1902). Stern's paper had been translated into Russian in the same year in which it had been published in Germany, and the two papers of Stern in the first number of the *Beitrage* were also translated speedily. What was particularly striking was that almost all the experimental research on the psychology of testimony in Russia was carried out by members of the legal profession, starting with replication studies of experiments by Binet and Stern.

It is clear that the vast program of research to which the *Beitrage* bear witness could not have got off the ground and sustained without the active involvement of prominent members of the legal profession. In this connection it is of interest to read Stern's brief review (1906) of Binet's paper (Binet, 1905), in which Binet had drawn the attention of his French readers to the psychology of testimony movement in Germany. Stern points out that Binet "*justifiably* [emphasis added] claims that he himself had many years earlier called for such a research program", and Stern adds that now Binet, "not without melancholy", states that conditions in France had made it impossible to implement it, "mainly because of the indifference of the members of the legal profession". It should be pointed out also that Stern had no illusions about Binet's priority in the study of suggestibility in children, for he referred to that work quite prominently in his paper on accuracy of recall, even including an appendix of seven pages giving details of one of Binet's experiments (Stern, 1902, p.49-56). In view of these facts the statement by Wolf in her biography of Binet that, in regard to the psychology of testimony, Stern and his co-workers had not "appropriately credited Binet's earlier contributions" (Wolf, 1973, p.327/328) seems not quite warranted.

Returning now to the quotation in the introduction to this paper, the matter of

priority with regard to intelligence tests has always been crystal clear. Stern had always agreed with Binet that in the study of "individuality" it was necessary to pay special attention to "higher mental processes". On the other hand, for many years he saw no merit in searching for a series of brief tests as indices of "intelligence". He voiced both these opinions in his review (Stern, 1900) of Stella Sharp's paper (Sharp, 1899), in which she had explored in minute detail the relationships between scores on tests covering 10 different psychological functions of the kind that Binet and Henri had proposed in their program for individual psychology (Binet & Henri, 1896). Interestingly, Binet, who reviewed the same paper by Sharp, was encouraged by it to continue with his search for a series of brief tests (see Wolf, 1973, p.152). But, of course Binet often had his own doubts and frustrations, and in his effort to understand "individuality" used other approaches as well, such as that in his paper on the dramatist Hervieu (Binet, 1904). Stern was particularly impressed by this study; he [illegible] the "combination of intuitive empathy and scientific psychological analysis . . . to produce in the end a rounded, many-facetted and true-to-life picture of the person as a whole" (Stern, 1911, p.344). It is probably no coincidence that Stern's first doctoral student made a study of E. Th. Hoffmann, the novelist of the German Romantic Period (Margis, 1911). Margis commenced the study in 1909, more or less at the same time as Dr. Otto Bobertag (his doctorate was in chemistry), working closely with Stern, had embarked on a survey of the intelligence of school children, using the Binet-Simon test (Stern, letter to Jonas Cohn, May 17, 1909). Stern's special interest was in extending the tests to enable the identification of gifted children (Stern, 1910, translation 1911). Through lectures and workshops teachers in Breslau and surrounding areas quickly became acquainted with the new methods, and at the first conference of the newly founded Bund für Schulreform one whole day was devoted to "Problems of intelligence and the school". In Stern's paper at this conference he described the principles of the Binet Simon scale of 1908 as "one of the most significant

advances in educational psychology up to now" (Stern, 1911, p.15).

Binet did not live long enough to witness the incredible impetus that his test instrument gave to the intelligence test movement in Europe and America, nor to see how tests were used and abused. Stern did. In his institute in Hamburg he and his associates fueled that movement, but at the same time Stern persistently warned against mechanical and simplistic interpretation of test scores. When he came to the U.S.A. as an emigré in 1934, he was shocked by the the test practices in the schools, and according to Gordon Allport (1968, p.324) "his chief desire was to introduce personalistic psychology into America, to counteract, as he said, the 'pernicious' influence of his earlier invention, the I.Q.".

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THE EARLY HUMANISTIC TREND IN RUSSIAN AND CENTRAL EUROPEAN PSYCHOLOGY: THE CASE OF NIKOLAI OSIPOV (1877-1934)

Irina Sirotkina*

At the beginning of the century Russian psychology and psychiatry already contained the "humanistic trend" -- the one that was shaped in accordance with the principal assumptions of the humanities, embraced humanistic values, and was regarded by its proponents as a part of the Geisteswissenschaften. This trend continued to exist after the October Revolution as well, having eventually become a part of Central European culture. Keeping aside the otherwise interesting issue of the possible maintenance of the humanistic trend in psychology in Soviet Russia, I intend to examine this trend as it manifested itself in the largest intellectual center of Russian emigration of 1920s, being developed by the group of Russian philosophers, psychologists, and psychiatrists settled in Prague after the establishment of the Soviet regime. Within this group, the figure of Nikolai Osipov -- a psychiatrist and psychologist, whose career had begun in Russia, successfully proceeded there for almost two decades before the 1917 Revolution, and ended in Prague in the early 30s -- will be of my particular interest.

As regards Russia, debating the epistemological status of psychology and psychiatry, in which the standpoints of both Naturwissenschaften and Geisteswissenschaften were presented, can be traced back to the very beginning of the 20th century. Following the establishment of the Soviet regime, however, the narrow-minded scientism began firmly to oust the humanistic orientation. The idea that "modes of thinking of the psychiatrist-clinician and the historian are the same," maintained, among others, by Osipov (N. Osipov, "Double-facedness and the unity of medicine," in "The Works of the Russian People's University in Prague," vol. 2 (1929), p. 181 [in Russian]), was completely neglected by Marxist methodologists. The official standpoint of that day was put forward, in particular, by a Soviet psychologist Leo Vygotsky, in his book entitled "The Historical Meaning of the Crisis in Psychology" (1926). This work, albeit remained unpublished until recent years, clearly emphasized (and, as it turns out, prophesied) the mainstream tendency of Soviet psychology to conceive of itself as belonging entirely to the domain of natural sciences. So far as Soviet psychology was declared to be reliant on "objective laws," whatever attempts to describe it in terms of humanistic, individualizing thinking were disregarded. Despite the fact that certain branches of the discipline (child psychology, clinical psychology, psychiatry) actually continued to practice such forms of research as case-studies, in which the "personal element" is essential, the latter were none the less claimed unimportant for the mainstream (Soviet) psychology.

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Although some methodological works which advocated the opposite point of view were still appearing in Russia in 1920s, its main adherents left the country. Thus the lack of humanistic psychology and the correspondent line of philosophical reflection in the USSR was actually compensated by Russian emigrants, with whom this trend has also become a part of Central European cultural panorama. Among those who emigrated to Czechoslovakia was Osipov -- one of the most eloquent champions of this humanistic view.

Nikolai Evgrafovich Osipov (1877-1934) began his professional career at Moscow University, under the auspices of which he founded the laboratory of psychotherapy in V. P. Serbsky's psychiatric clinic. During 1906-11 Osipov had treated as much as 549 cases of neuroses, which were systematically analyzed in his manuscript entitled "Polyclinic for neuroses. The scientific report of the laboratory of psychotherapy" (completed in 1933; unpublished). Osipov emphasized the psychological (vs. organic) etiology of all neurotic diseases, which is probably the reason why he accepted both Freud's concept of psychoneuroses and method of their treatment. However, Osipov insisted on the plurality of psychological reasons for neurotic diseases. Neither he confined his approach to the emphasis of rational psychotherapy on making the patient aware of her early experiences. In his view, the advocates of rational psychotherapy zeroed in so completely on this one tendency that they mistakenly turned it into the whole of the human condition. Long before Victor E. Frankl's "logotherapy" and the appearance of humanistic psychology (as the widely-acknowledged approach), Osipov already pointed out the importance of realizing "absolute values" for the patient's recovery, thus stressing the priority of "value-therapy" for psychoanalytic treatment.

V. Serbsky, who was S. Korsakov's successor as the head of the psychiatric clinic at Moscow University and the leader of the Moscow Psychiatric School (1880-1930), expected Osipov to occupy this position after his own retirement. However, both Serbsky and Osipov had to leave the University as a result of the infamous political conflict initiated in 1911 by Kasso, a bluntly conservative minister of education. Osipov continued his activity at the Rukavishnikov Shelter for the Delinquent Children. During this period (1911-18) his psychological interests opened to even broader horizons. Insofar as he refused to recognize the substantial boundaries between abnormal, pathological, and normal psychic processes, Osipov was prepared to move to the comprehensive studies of personality. Accordingly, his writings in this decade include "The program for the study of personality" (1913). His meditations on the epistemological status of clinic psychiatry led him to describe it, utilizing Windelband's and Rickert's framework, as a discipline of the "historical type" (see the quotation above).

In 1910 Osipov visited laboratories of Freud, Jung, Bleuler, and Dubois. It is no wonder, therefore, that he was the first to introduce and popularize psychotherapy in Russia

-- in particular, by editing the so-called "Psychotherapist Library," 11 volumes of which contained translations as well as original contributions on relevant topics by Russian authors. He co-edited the journal "Psychotherapy" (with A. Bernstein and Yu. Kannabich) supplying it with numerous reviews on the West-European authors as well as his own writings on psychotherapy of neuroses. Parallel with his work at the Rukavishnikov Shelter, Osipov actively maintained his relations with other Moscow psychiatrists, serving as a secretary of the Physicians' Society of the Psychiatric Clinic, which held its regular meetings on Fridays (and was named the "Little Fridays" Society).

After the 1917 Revolution, Osipov had left Moscow and Soviet Russia for Czechoslovakia, where the supportive governmental policy enabled Russian scholars to pursue their professional careers and even establish the new intellectual community. In 1923, the Russian People's University was founded in Prague, and Osipov began to teach there as a docent. Apart from teaching and his continuing consulting activity on the matter of neuroses, he participated in numerous meetings of Philosophical Society and in seminars on a variety of topics (e.g., on Russian grammar and stylistics, Dostoevsky studies, general problems of natural sciences, etc.). Osipov also guided the Russian Psychiatric Circle in Prague, having got several devoted students.

They used to call Prague of the 20s as "Slavic Athens." The new constellation of Russian intellectuals, formed there, included such notable figures as philosophers G. Florovsky, D. Chizhevsky, I. Lapshin, N. Lossky, V. Zen'kovsky, historians A. Kizevetter and S. Gessen, a biologist M. Novikov. Osipov seems to be engaged in active dialogue with many of them, as well as Czechoslovakian scholars (E. Babak, prof. Haskovec and others). For instance, in his writings of that period one may often find references to Lossky, whose intuitive philosophy Osipov regarded as close to his own outlook. He also meditated on the philosophy of psychoanalysis, putting forward a concept of Transedental Love: according to it, he interpreted the "phenomenon" of revolution as a regression to narcissism.

As a kind of nostalgia, Russian literature was enthusiastically discussed by emigrants at a numerous seminars, meetings and celebrations of writers' jubilees. A. Bohm's seminar on Dostoevsky, in which Osipov took an active part, was a most productive examples of such studies. The seminar adopted the method of quasi-psychoanalytic interpretation of "unimportant things," "little details" of Dostoevsky's works, which clarified their psychological sense. Among the titles of Osipov's presentations at this and other meetings were the following ones: "Words with the opposite meaning," "'The Double' of Dostoevsky," "The problem of the diagnosis of personality," "Leo Tolstoy and medicine." Conducting the psychological and psychopathological analysis of Tolstoy, Dostoevsky, Gogol, Goncharov and their literary

characters, Osipov contributed to the popular topic of *fin de siecle*, "Talent and Insanity". As one of his teachers, N.N. Bazenov, he did not think that genius' mental troubles signalized of the degeneration, but are a sign, or sequence, of a high developed psychological organization.

Cheiron, Paris 1994

Abstract

Counter-colonial discourse and the decolonization of anthropology: Listening to social scientists and writers from Oceania.

Irmingard Staeuble, Freie Universität Berlin

The paper is concerned with the contributions indigenous social scientists and writers from Oceania have made or can make to the process of decolonizing anthropology. In the first part, the dimensions of decolonizing anthropology so far discussed in the postcolonial critical debate are summarized. The second part gives an account of mainly Papua New Guinean anthropologists' writings, as analyzed by Morauta (1979) and commented upon by indigenous and Western social scientists. This evidence suggests closer analysis of the structural relationship between colonial and counter-colonial discourse, a point discussed with reference to Keesing (1994). The third part provides an entry to the Oceanic experience of colonial inferiorization and the difficulties of disentangling oneself from its webs, drawing from the recently emerging written Papua New Guinean autobiographical and novellistic literature.

Paper prepared for Cheiron, Paris 1994

Counter-colonial discourse and the decolonization of anthropology: Listening to social scientists and writers from Oceania.

Irmingard Staeuble, Freie Universität Berlin

At last years' meeting, I pleaded for listening to indigenous voices from the Third World in order to both oversome the colonial heritage of structural communicative asymmetry, and develop some sensitivity for the lasting vulnerability of former colonized peoples. Since then I have tried to get access to indigenous views from one of the richest regions in terms of classical as well as postcolonial anthropological and ethnopsychological research - from Oceania. My main concern in this paper is with the contributions indigenous social scientists and writers have made or can make to the debate on a decolonization of anthropology. The indigenous material used is largely second-hand because, so far, hardly any social science articles, novels or stories have found distribution beyond local grey journals and papers. Still, I think the use of such material is justified in an attempt at developing critical perspectives for future research at the sites of accessible material.

Dimensions of decolonizing anthropology.

Postcolonial critique of anthropology has focused on its colonial (less on neocolonial) uses, class and male bias, exoticism, the essentialization of cultures, and ethnographic authority. The questioning of hegemonic discourse in ethnography

has brought to the fore the Eurocentrism inherent in the very construction of the Other (by the use of Western notions of culture, personality, and time), the hidden authorship of the informant, and the lasting marginalization of non-Western voices by the structures of academic production. Attempts at a critical reconstruction of anthropology, from marxist, feminist or postmodernist angles, in constructionist or dialogical fashion, seem bound to face ever deeper layers of hitherto unrecognized hegemonialism and the power politics of knowledge (Nencel & Pels 1991). It may well be doubted if the task of decolonizing anthropology can ever be achieved.

The challenge by indigenous voices.

Taking account of their research conditions, indigenous anthropologists have emphasized their ongoing marginalization by the structures of academic production. Availability of research funds, library facilities, printing possibilities still privilege Western researchers, not to mention the unreversed asymmetry of large-scale Euroamerican research in far distant cultures and but a handful of Third World anthropologists researching the West (Fahim 1982). For anthropologists from the Pacific Islands, the Tonganese Hau'ofa was one of the first to describe academic working conditions in his country. In search for more Oceanic anthropologists, I came across Louise Morauta's (1979) attempt at analyzing indigenous Papua New Guinean anthropological contributions to a revision of the approaches and role models offered by foreigners. Followed by a dozen comments, half of them by indigenous Pacific and African anthropologists, this debate

might have been expected to initiate a substantial controversy - still, I could find no continuation.

Papua New Guinea gained political independence in 1975. Morauta, born and educated in England, taught at the University of Papua New Guinea since 1970 and became a citizen of PNG. Her analysis is based on the work by 12 PNG social scientists selected for distinct viewpoints out of 130 articles and papers. In terms of local academic conditions it is important to notice that the writers of these papers were geographically and institutionally dispersed, lacking any institutional focus for discussion as well as national political interest groups to which intellectuals might attach themselves (Morauta 563). Her source materials were "not readily available locally, let alone in other countries", as stated in the comment by a student collective from PNG university (567).

The 6 themes selected in Morauta's analysis concern: 1.) criticism of both foreign anthropologists' behaviour (professional exploitation, the stealing of artifacts, and failure to report back adequately), and of their work, especially with regard to foreign ends served by foreign social science, compliance with colonial governments (e.g. in legitimizing actions against cargo cults), and specific questions of concepts and content, like the notion of "primitive society" and ethnographic "biased pictures" and "half-truths". 2.) Pleas for insider research, sometimes amounting to the "doctrine of the insider" (Merton). 3.) An emphasis on cultural unity rather than diversity, which is reasoned in terms of the goal of national political unity, the need to establish a cultural identity, and the desire to promote distinctively PNG forms of development. 4.) Reinterpretations of

social change contrasted to as well as correcting foreign accounts (a phenomenon similar to developments elsewhere in the Third World). 5.) Interest in the role of tradition in contemporary society, with a specific emphasis on traditional cultural elements as manipulable factors to achieve certain political goals. 6.) A widespread demand for a close relationship between research and action, be it in the form of applied social science in public service or, more often, as consciousness raising and the designing of development projects.

The comments on Morauta's analysis differ remarkably in terms of indigenous respectively Western authors. Most indigenous authors push the lines of anticolonial discourse further, Western anthropologists cited critically in the material analyzed are up in defense of their approaches, with an undertone of denying that there is anything indigenous anthropologists could do better than foreign ones. As to the PNG commentators, a student collective from PNG university emphasized the need for expanding the term "decolonization" in order to answer questions referring to decisions on research priorities, funding agencies, purpose and benefitters of research projects, and pointed to the still open question of neocolonial anthropology. Waiko, one of the indigenous anthropologists cited by Morauta also raised the question "In what way and to what extent is the foreign intellectual milieu - methodological, theoretical, epistemological - >being decolonized<?" (572), criticizing Morauta's exclusive concern with the intellectual elite and asserting, instead, the importance of dialogue between national intellectuals and the masses. A sociologist from Nairobi emphasized the structural marginalization

of indigenous anthropology, referring to Galtung's notion of "academic imperialism".

The evidence provided by Morauta's analysis and the subseuent debate suggests that the echoes of colonial conditions within social science are still in need of closer analysis. This will be discussed with reference to Roger Keesing's (1994) case studies of Melanesian framing of anti-colonial discourse in terms of colonial discourse.

Disentangling oneself from hegemonial discourse: Evidence from an emerging written literature

Novelists from former colonized African countries have, over recent decades, provided rich evidence of the subtle after-effects of the inferiorizing discourse of domination and the difficulties of disentangling oneself from its webs. The emergence of a written Papua New Guinean literature promises to soon allow for a comparison with Oceanic experience.

Here I can but provide some background to this emerging literature (Schild 1981). Up to 1945, education in Papua New Guinea was mostly provided by the missions; even after the Australian government formalized education to some extent, mission schools exceeded by far the number of government schools. According to a census of 1969, the foundation year of the University of Papua and New Guinea (UPNG), there were 30 000 primary school graduates, 14 000 secondary and 2 high school graduates. The university was meant to educate for independence. Ulli Beier, the first "lecturer in literature of developing countries" and later (1974-78) Director of the Institute of PNG Studies, built upon his Nigerian experience, teaching students to

collect oral literature, and introducing creative-writing courses, the results of which were published in his specially founded journal Kovave (1968-1973). According to Schild (1981, 132), the first PNG novel was a result of a creative-writing course (Eri 1970). A Bureau of Literature founded in 1968 by the missions and government also published a journal New Guinea Writing (1970 renamed Papua New Guinea Writing) and organized writing competitions.

In my talk, I will discuss some observations on A. Maori Kiki's autobiography Ten Thousand Years in a Lifetime (1968), Vincent Eri' s novel The crocodile (1970), and stories from (yet unavailable to me) local journals which have been extensively quoted in Schild's (1981) report.

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ANDROGYNY: Changes in the relationship between the sexes in german literature and philosophy (ca 1750-1815)

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In German literature and philosophy after 1750 interesting changes can be discerned in the representation and ideas concerning the relationship between the sexes. The traditional concepts of masculinity and femininity, but also the concept of man, are provided with a new meaning. The new concept of man is a product of:
a. the sexualization of the traditional and purely rational concept of man (implicit masculine)
b. the admission of feminine traits, spiritual and physical, into the traditional concept of man.

In German literature of the period different concepts are used to indicate these changes. Some of these concepts have positive (androgyny), other have negative connotations (*Machtweib, Verweiblichung*. 'Androgyny' and 'hermaphroditism' are two of the most important terms used to signify and connotate the 'new' relationship between the sexes. Sometimes they are used as synonyms, but more often they can be found to have a clearly different meaning.

The changes in representation and ideas of the relationship between the sexes reach their climax at the end of the century in early German Romanticism. Then the "soul" of man gets a new compostion: consciousness besides subconsciousness, rationality besides sensibility, masculinity besides femininity. The new psyche becomes androgynous. This process of androgynizing and psychologizing of man can be divided into three stages: Later- Enlightenment/Classicism, early-Romanticism, high-Romanticism.

1. (1770 - 1790) Later-Enlightenment / Classicism
From an apparent 'asexual' conception of man (implicit masculine) to a sexualised conception of man, were traditional feminine traits such as : sensibility, softness and round forms of the body are accepted as human.

Examples:
a. Lessing - Laokoon (1766)
In aesthetic experience the capacity to be moved is an essential one. To be moved to tears is used by Lessing as a norm to make a distinction between civilised men and barbarians.

b. Winckelmann - Geschichte der Kunst des Altertums (1764)
Hermaphrodite statues of Greek gods and heroes become in Winckelmann's aesthetic theory the expression of the classic ideal of beauty. The most perfect and beautiful body is a body with masculine as well as feminine sexual characteristics.

2. (1770 - 1790) Early-Romanticism
In the love-conception of *Sturm und Drang* the sensibilty (Empfindsamkeit) of the man in love becomes the proof of the beauty of his soul. Feminine characteristics are not only accepted as human, but are also seen as a source of ethical and aesthetical amelioration of the human kind.

Example:
Goethe - Die Leiden des Jungen Werthers (1774)
Goethe's hero, the most conspicuous representative of the sensibility of this philosophical-literary movement, is a lover who through his being-in-love develops a superior sensibility and creativity. These (for men) new characteristics lead him to a radical rejection of the social conventions of his time and to a conflictual relation with his world and with his own soul.

3. (1790's - 1815) High-Romanticism
At the end of the century, the mixture between the masculine and feminine plays an important part in the constitution of the new human psyche. The admission of the feminine in the conception of man introduces a new dimension in the human soul. The new psyche is a, certainly not always harmonious, mixture of consciousness and subconsciousness; of rationality and sensibility and of masculinity and femininity.

Example
F.W. Schlegel - Lucinde (1799)
Schlegel's Lucinde, a novel which upon publication caused great commotion among the critics because of its supposed amoral conception of love and the peculiar relation between the sexes, can be used as an illustration of the androgynizing of the human soul. Julius, the hero of the story, but also Lucinde his girl-friend and lover, can be interpreted as androgynous personages.
However, that androgyny, as conceived of in German Romanticism, does not mean equality between the sexes, can be shown by the differences between Julius' and Lucinde's androgyny. Lucinde is androgynous from the start. She is one, perfect and harmonious: feminine but also rational, intellectual, intelligent, and active. Julius on the contrary, is not androgynous, but becomes androgynous through his experiences with different types of women. His becoming androgynous is also the last stage in the development of his soul (psyche). Julius has a developing, striving, contradicting psyche. Lucinde is harmonious but static at the same time.

Androgyny in German Romanticism means no longer just a physical compound of masculine and feminine characteristics, nor is it merely used as an aesthetical criterion to determine the beauty of body-forms. Androgyny becomes at the beginning of the 19th century, a way of expressing the dualistic character of the human psyche. As such it changes from a concept which signifies an external mixture of masculine and feminine traits to an internal stage in the development of the psyche. This process of the androgynizing of the human soul, although it could be expected, does not imply an equal representation of the relation between the sexes. Men are equipped with a more-dimensional soul in development with feminine traits; women on the contrary, remain static creatures.

The androgynizing of the human soul is an aspect of the constitution of the 'modern' psyche. This implies a peculiar and consequential thematization of the feminine in literature and philosophy. The new romantic interest in women and the feminine has led indeed to a more complex and mixed concept of man and woman in general. Nevertheless this did not imply that the new enriched concept of man could be arbitrarily filled in by 'man' as well as by 'woman'. Philosophically spoken, this way the emancipation of women was inhibited halfway.

Isabel Suarez - 12-6-94

From the laboratory to the outside world

The theoretical assumptions of early psychotechnics

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Abstract

In this paper it is shown that the conception of the assessment of fitness for school or vocation as the prediction of some outside criterion did not originate at once with the rise of applied psychology. In early psychotechnics not only the apparatus but also the "essentialistic" search for fundamental factors of classical laboratory psychology (the Wundtian programme) was preserved, even where the new name test was used. The judgment of non professional referents was suspected, and though the test scores were compared with the appraisal of superiors or teachers, "psychological truth" was primarely sought in the mutual coherence of the tests. In this sense construct validity preceded criterion-validity. The gradual transition from the laboratory to the outside world within the Heymans tradition in the first quarter of the century is used as a case in which the theoretical assumptions of early psychotechnics appear in a clear way.

In the history of psychology the origination of applied psychology at the turn of the century is often seen as the emergence of an alternative programme, aiming not at the study of the generalised human mind but at the prediction of future behaviour of individuals or groups. Without denying that the goals are different, I will show in this paper that the indebtedness of early applied psychology, especially in the form of *psychotechnics*, to the old Wundtian laboratory psychology is much stronger than is usually assumed.

The most obvious link of psychotechnics with the established laboratory psychology is of course the use of instruments developed for the measurement of general functions for the determination of inter-individual differences. What previously counted as error variance became now, as William Stern remarks in his *Differential Psychology* (1900), a problem in its own. In this paper I will give a number of examples of the use of laboratory apparatus for psychotechnical purposes.

However, it is not only the laboratory equipment that is preserved in the new psychotechnics, but also much of the theoretical assumptions. The basis for the appraisal of a subject's mental health or fitness for a school or vocation is not sought in the construction of valid predictors, but in the tracing of the mental functions that are required for a certain performance. In this context the first use of correlation coefficients is not to establish the correlation between a test and a criterion, but to establish the mutual correlation between the different functions that are measured. In this sense, construct validity precedes criterion-validity. There is an "essentialistic" search for fundamental factors on which other, more complex, variables depend.

Industrial psychotechnics is a relatively latecomer in the birth order of the respective branches of applied psychology. The first applications of psychology were in the psychiatric clinic (Kraepelin) and in educational psychology (a. o. Ebbinghaus and several others in his generation). Especially in the psychiatric clinic the methods used were for a number of decades those of the psychological laboratory, and the research mainly consisted of determining the way in which patients differed in their functioning from normal subjects. It was primarily for practical reasons that more simple *paper and pencil tests* were introduced in the testing of school children: they can be administrated in a group, and it is easier to take tests to the school than the children to the laboratory. Yet, at first the data were interpreted in the habitual essentialistic way. It was only in post-Binet I.Q.-psychology that correlations between test and criteria of school success were determined. The breakthrough of industrial psychotechnics occurred only around World War I, and here too it took some time before, first in America, the predictive use of tests supplanted the search for fundamental determinants of vocational performance. Correlation statistics was mainly used for determining the covariance between tests; for the rest the statistics used consisted of the determination of norms of performance and of the determination of norm-profiles which could be compared with a criterion-profile.

The development just sketched is clearly illustrated by the course the research in the Heymans Laboratory at Groningen University (founded in 1892) took in the first quarter of our century. Heymans' primary concern was the establishment of the fundamental laws of consciousness. He published a series of papers on *mental inhibition* in the *Zeitschrift für Psychologie.* Around the turn of the century his interest changed, and he turned his attention to differential psychology. On the typology in which his research resulted I have reported at the Cheiron-Madrid meeting (Derksen & Van Strien, 1992). The trait-ratings in his questionnaires were correlated with three fundamental dimensions: *activity, emotionality, and secondary vs. primary functioning (Heymans 1908).* With the collaboration of his assistant Henry Brugmans he took in 1911/1912 a next step, aimed at the study of fundamental determinants of intellectual achievement (Heymans & Brugmans, 1912). To this end a *practicum* was organised, in which his students were subjected to a series of laboratory experiments covering the following psychical functions: *memory, liveliness of fantasy, concentration, secondary functioning and intellect.* For each function a series of experiments or tests were administered, and their mutual correlation was determined. The indebtedness to the traditional laboratory psychology is apparent in the use of apparatus like the metronome and the mnemometer of Ranschburg for the examination of *memory,* the colour disc, the light screen and the light-inhibition experiments of Wiersma for the determination of *secondary functioning*, and the dotting apparatus (Burt) and a phonometer for the determination of *concentration*. The aparatus used is depicted in the catalogue of the Netherlands Foundation for Historical Materials

(Hartevelt, 1989), and partly also in the catalogue of the centennary Heymans Exhibition (Draaisma, 1992). The other tests consisted of simple tasks, like the recognition of colours, the description of pictures, repeating a story, word and picture puzzles, and the like. Though he certainly did known them, Heymans made no use of the Binet scales for the measurement of intelligence, and he did calculate no correlations between intelligence and the other functions or between those functions and some criterion of academic success. Apparently Heymans and Brugmans were convinced beforehand of the significance of the five functions, probably because prior inquiries into academic performance (especially of woman) had shown them to be relevant (Heymans, 1910)

The same conviction of the foundation of inter-individual variation in a restricted number of mental functions can be found in a paper Heymans presented in collaboration with Stern and Lipmann at the *6. Kongreß für experimentelle Psychologie* in Göttingen, april 1914 (Heymans, 1914). The paper is a plea for international collaboration between psychological laboratories in bringing the results of experimental and psychgraphical research on individuals together. Voluntary concentration of attention is selected as a central trait to be measured both with the help of a number of simple experiments and by psychographical ratings of concentration in everyday life. To attain a perfect standardization, sets of experimental equipment and questionnaires were made available on request by the *Institut für angewandte Psychologie* at Berlin-Potsdam, and this Institute also offered its services for processing the data. I have not been able to find out how the proposal of the trio was received, but probably the intended collaboration was thwarted by the outbreak of the First World War a few months later.

Looking at the type of "experiments" proposed, we find that they have more the appearance of tests in the present sense than of laboratory experiments: The Bourdon cancellation test, backward reading, proofreading, analogies, etc. No instruments are required apart from little cardboard weights which have to be compared. In this sense the project is a step away from the laboratory on behalf of an easy distribution and administration. The theoretical aims, however, still are the same as in the Heymans-Brugmans project: the empirical substantiation of fundamental traits.

A few years later the demands of practical life began to assert their rights. Of course, Stern and his co-workers already had made valuable practical contributions before: in the *Psychologie der Aussage* (the eye-witness), in school-psychology, and in the selection of female streetcar drivers (Stern, 1918). In 1920 a *Social-pedagogical Institute* was founded at Groningen, financially supported by a legacy of the well-known socialist politician Dr. D. Bos, which - in close collaboration with the Heymans Laboratory - set itself the task to lay the scientific basis for vocational guidance and personnel selection. In his opening address Brugmans, who was appointed as director, made clear that the *Bos Institute* did not intend to be a service institute itself, but aimed to examine the

requirements for different jobs, and to select the proper experiments and tests for bringing the right men in the right place. By making this research available to pioneering local bureaus elswhere, the Institute would serve as a Central Institute for vocational psychology in the Netherlands (Brugmans, 1921).

The first job that was studied was that of telephone operator. The first step consisted of a *mental analysis* of the requirements. Ten psychological functions were chosen as being relevant for the task, and each was measured by a number of "tests". The quotation marks used in the original publication (Brugmans & Prak, 1921) are justified, in sofar that many of the tests consisted of traditional laboratory experiments, which now were used for a differential purpose. Next to paper and pencil test like the Bourdon-Wiersma, and simple tasks like auditory and visual reproduction, the tachistoscope, the teleometer, the tremometer, the fall-phonometer and similar instruments were used, and reactions were measured in hundredths of seconds with the chronoscope of Hipp and registered with the chronograph of Jaquet. The tests were administered to 13 women who already worked at the telephone office, and the results of each test were compared with the over-all rating of their supervisor. Correlations were calculated between the ranking of the operators on each test and the ranking of the supervisor. Some correlations were negative (e.g. on the tapping test) and some were zero, but most of them were positive, culminating in .80 for the teleometer, a test which had some resemblance with the operator's job. The probable error of each correlation was also determined. On the basis of the outcomes the value of the respective tests for the selection of operators is discussed, and the results are compared with similar investigations in Geneva (Fontègne and Solari, 1918). Finally an over-all score is determined for each testee and the ranking of these scores is compared with the ranking of the supervisor. The correlation between both rankings appears to be >.90.

In the following years similar investigations are conducted on other jobs: high voltage electric fitters (Brūgmans & Prak, 1922a), assistants in the Laboratory of Philips Electric Works (Prak, 1926a), apprentices in Storks Machine Factory (Brugmans & Prak, 1926) and pupils of the Technical School at Eindhoven (Prak, 1926b). Next to this investigations are conducted in a teachers seminar and in several elementary schools and high-schools. The general approach is the same as with the telephone-operators. A difference is, however, that apart from those on the pupils of the teachers seminar, the investigations were conducted outside the laboratory. This made it difficult to use complicated apparatus. The Hipp-chronoscope was used on site, but otherwise rigorous adaptations had to be made. In the school investigations the tachistoscope and the mnemometer were fitted for administration in the classroom by projecting the stimuli on a screen. In the industrial investigations the psychological functions that were expected to be relevant were occasionally measured with the help of a simple instrument (e.g. the

tremometer), but for the rest with individually administered paper and pencil tests and simple performance tests. Thus, the step from experiment to test and from laboraory to the outside world was taken mainly on practical grounds.

For the rest the basic approach remained the same: comparison of the outcomes of the respective (groups of) tests with the appraisal of supervisors or teachers. In the industrial investigations the number of the subjects was invariably low; only in the school investigations larger numbers were involved. Where possible the appraisal of more than one judge in the work or school setting was invited. Probably under the influence of Prak, who was a keener statistician than Brugmans, rankings were in the later investigations replaced by ratings on a scale and inter-judge reliabilities were calculated. In his investigations at Philips, Prak asked his judges not only to give an over-all appraisal but also to rate a number of traits (interest, keenness, accuracy etc.). Correlations were calculated with both the over-all and the separate ratings, and the best tests were proposed for retainment in a future test battery.

A salient feature of the extensive reports Brugmans and his collaborators have published is that the words *prediction* and *criterion* are absent from their vocabulary. In their report on the high-voltage electric fitters Brugmans & Prak (1922) speak of *disowned tests,* of which the low correlation with the appraisal of the supervisor might be caused by the inadequacy of the last. The purpose of the test, they argue, is not primarily to attain in a few hours an appraisal which for the supervisors takes much more time, but to *improve* the judgment on the site. For the man of industry this improvement is the motive for asking the advice of a psychotechnicien. In his doctoral dissertation Prak (1925) devotes a lengthy chapter to the inferior quality and low reliability of the judgment of laypeople, and in his Philips study Prak pools their ratings in order to attain a more reliable judgment.

Making up the balance of the developments described, we can conclude that the search for fundamental psychological functions, which forms the objective of the Wundtian programme of experimental laboratory research, still guides much of the work of the early psychotechniciens. There is a cautious attempt at external validation, but a reluctance to accept the judgment of laypeople as the criterion. Not prediction of a dubious criterion, but attaining some "psychological truth" (Van Strien, 1966) is their striving. This reluctance is not restricted to representatives of the school of Heymans, but can be found in other quarters as well. Thus Cattell in his *Guide to Mental Testing* regards "sociological validity" (the correlation with some external criterion) as inferior compared to internal validation on the basis of factor analysis. Of course predictive validation in the present sense was gradually emerging already in the tens and twenties of this century - the period covered by this paper - especially in the United States. The use of tests that more had the character of a work sampe than a tool for measuring some

elementary function paved the way. But I hope to have shown that the laboratory paradigm was much more tenacious than usually is assumed by historians.

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THE FORERUNNERS, FOUNDATION AND ROLE OF THE *AUSTRALIAN JOURNAL OF PSYCHOLOGY*

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Abstract The combination of geographical remoteness from other proponents of modern psychology and paucity of the widely dispersed local population posed a conundrum for early Australian psychologists in terms of putting in place their own communication networks. While the delay in contacting overseas colleagues made these especially desirable the personnel resources to sustain them were lacking. Although psychology was introduced to Australian universities over a century ago, the first national journal, the *Australian Journal of Psychology*, appeared only 45 years ago, under the auspices of the Australian Branch of the British Psychological Society, founded in 1944. World War 2 gave a considerable boost to psychology, but academics and professionals had been enthusiastically seeking to advance their discipline for the previous half century, and to this end combined with representatives of other disciplines in formal bodies. Their main peer groups were educationists and philosophers, encountered in the Australasian Association for the Advancement of Science, which all three entered as early as 1893, and the Australasian Association for Psychology and Philosophy, set up in 1923. The latter produced a substantial journal which strengthened the influence of psychologists from the centre which edited it and assisted the discipline to consolidate its own position. After a somewhat slow start, the *AJP* functioned initially as conveyor of matters of professional and societal interest as well as of research material, but now retains only the latter role which nonetheless, as most contributors are Australian, serves to inform members of the local community about each other's work.

THE FORERUNNERS, FOUNDATION AND ROLE OF THE *AUSTRALIAN JOURNAL OF PSYCHOLOGY*

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The first general journal of psychology to be published in Australia, the *Australian Journal of Psychology (AJP)*, made its appearance in 1949, over half a century later than its French counterpart, *l'Annee Psychologique*. The end of 1944 had seen the establishment of the country's first professional body of psychologists, the Australian Branch of the British Psychological Society, and the *AJP* was sponsored by this group, remaining its only official publication throughout the Branch's existence. When in 1966 the Australian Branch was replaced by a formally independent Australian Psychological Society (APS), the *AJP* continued as its major organ, though with a role modified by the creation of the *Australian Psychologist (AP)* in 1966. This took over the publication of material with a professional orientation and the reporting of societal issues, the latter being in turn taken over by the *Bulletin of the Australian Psychological Society* since 1979. The *AJP* is a refereed journal with international distribution and experimental-archival content.

Despite the relatively late appearance upon the Australian scene of its national journal, psychology had emerged within the universities there not much later than in Britain and Europe, and was certainly visible within the public arena by the time of World War 2. It was not however exactly well entrenched; separate departments existed within only two universities and one university college before the war, and although various state governments had begun to create positions for psychologists in the 1920s these were neither numerous nor necessarily permanent and no appointments had been made at the federal level. The war both enhanced and consolidated the position of the discipline in respect of the creation of employment opportunities; the formation of the professional society may be seen as one indicator of this consolidation. An examination of the content and authorship of the *AJP* over its first three decades has already been carried out by others;[1] what will be addressed in the present paper is the question of how and why Australian psychology managed to survive for some 50 years without either a professional society or its own exclusive journal.

A dominating factor in the history of all intellectual activities in Australia has been that of distance - of Australians from their overseas peers and from each other. The significance of the huge distances across the continent has been compounded by the relative paucity of the

population, a factor of paramount importance in the case of a new discipline. The situation was further exacerbated by the political set-up; prior to federation in 1901 the six states were linked only by their allegiance to the British government. The establishment of Australian universities did not greatly pre-date the first institutionalization of psychology overseas; the University of Sydney was the pacesetter in 1850, followed by others in each of the other state capitals by the time of World War 1, but up until World War 2 only these six had come into being. The universities' governing bodies from the beginning made provision for overseas contact with generous systems of sabbatical leave, but set no formal mechanisms in place for the strengthening of local communications. The scholars themselves rapidly moved to construct their own mechanisms, which took the form of learned societies modelled along British lines (by 1886 there were 38 of these across Australia and New Zealand). The most ambitious 19th century venture in this direction was the Australasian Association for the Advancement of Science (AAAS, later the Australian and New Zealand Association, or ANZAAS), set up in 1888. The AAAS provided a forum for both meetings and publications, via its gatherings (usually biennial) and the proceedings thereof, produced as bound volumes in which official addresses, abstracts and sometimes whole papers would appear, as well as accounts of the business of the Association and its committees. At its first meeting in 1888, 19 disciplines were drawn together within 10 'sections'. 1893 saw the addition of two more disciplines under the rubric of Section J, Mental Science and Education, headed by Henry Laurie, Professor of Philosophy at the University of Melbourne.

'Mental Science' incorporated the subject matter of both philosophy and psychology, which were only formally distinguished in 1926 when the Section was re-named Education, Psychology and Philosophy. The liaison of these two with each other and with education was both necessary and durable. Philosophy was the pioneer discipline within the university system, and it was philosophers who actively promoted the other two. Within the AAAS, however, the key political grouping at once emerged as being between psychology and education. For 30 years the platform and proceedings of the Section J offered a major outlet for the airing of social science issues in Australia. While individual papers did not necessarily have a cross-disciplinary thrust, the four committees appointed by the Section during this period certainly did so, combining the interests of psychologists and educators with their various inquiries into the means of promotion of psychophysical and psychometrical investigation in Australasia and later of anthropometry, into the education of mental defectives and later into techniques of vocational guidance. It was not until the late 1960s that Psychology and Education finally split into two Sections, though the relationship

had been an uneasy one throughout, marked by threats of separation from its very inception, the dominance of the Education partner and Psychology's other liaisons.[2]

The main other such liaison was of course with philosophy. In most universities psychology grew up within departments of philosophy and initially many individuals engaged in both inquiries. Laurie had been responsible for the first introduction of psychological subject matter into an academic syllabus, making it a separate course element in 1896. He was succeeded in 1895 as president of Section J by Francis Anderson, Professor of Philosophy at Sydney. Anderson had also followed in Laurie's steps in the inclusion of psychology in his undergraduate philosophy syllabus, where experimental psychology was taught from 1913. He was however a more active and outspoken political figure than the other man, campaigning publicly for the inauguration of separate university departments of psychology, education, sociology and anthropology.

The first full-time academic appointment in psychology came about only in 1921, with the conversion of a University of Sydney post in philosophy to one in psychology. In the same year Bernard Muscio, who took a strong interest in industrial psychology, became Professor of Philosophy, and at once moved to found an Australasian Association of Psychology and Philosophy (AAPP), which first met in 1923. An additional impetus to this action may have been that during the war the meetings of the AAAS had lapsed. Under the auspices of the AAPP psychologists continued to meet with philosophers for the next 20 years with any interested person being admitted to membership irrespective of qualification. One of the group's major activities was the publication of the *Australasian Journal of Psychology and Philosophy (AJPP)*, 'the purpose of (which)', it was announced, 'is to deal with such subjects as philosophy proper, ethics, and the philosophy of religion, psychology and political philosophy, and the application of psychology to industry, education and law'.[3]

The Professor Emeritus of Philosophy, Francis Anderson, was the *AJPP's* first editor, from 1923-1927; for the next seven years it was edited by the head of the Sydney Psychology Department, Henry Tasman Lovell, but thereafter the task reverted to a Sydney philosopher. In his opening editorial Anderson remarked that such a journal

> 'runs a special risk, for it addresses two audiences, and may fail to satisfy both.....In Australia and New Zealand, until specialisation shall have increased with greater differentiation of studies, and until increase of population shall have brought with it a greater body of cultured opinion....one Journal must attempt the double task....But unless it undertook the twofold task, it would not survive the year.'

In the event it survived for 21 years, but became over time increasingly the vehicle of the philosophers. Professor John Anderson, editor after Lovell, unlike his two predecessors in the Sydney Philosophy chair had little time for empirical psychology. Although a section headed 'Researches and Reports' and devoted to empirical and technical studies continued to appear until 1941, this declined in size from 1930, and the bulk of the papers dealt with theoretical issues in philosophy, psychology, religion and ethics. A large amount of space was occupied by book reviews, and the journal also functioned as the Association's newsletter.

Until 1945 the *AJPP* provided the major publication outlet for psychologists in the country; 86 out of a total of 292 items published in the previous 25 years appeared in its pages, and only 10 in the *AAAS Proceedings* and 22 in overseas journals. The pre-eminence of the Sydney department as the central source of publication is unchallengeable; of these 86 articles only 17 came from outside New South Wales and all but 10 of the remainder were products of Sydney academics and their students. The two largest groupings of papers were those of the state-of-the-art genre and those concerned with matters of industrial psychology. The existence of the on-campus journal undoubtedly enhanced the efforts of local academics, though authors were quite enterprising in their search for other local outlets, utilizing books, pamphlets and a variety of other periodicals, such as commercial magazines. The decline in the journal's importance for psychology in the 1930s reflected not only the shift of editorship back to the philosophers but also the growth of psychological interest in centres other than Sydney.[4] Its very success as a publication outlet in the 1920s may have also been a factor, as psychologists took advantage of the opportunity it provided to construct their own identity.

World War 2 created a setback in the usual channels of scientific communication; 1941-45 saw a drop in the number of psychological publications. Perhaps partly because of this lapse, even when it was finally mooted the *AJP* did not get off to an easy start. At the first Annual General Meeting of the Australian Branch, in August 1945, the Executive reported that it had resolved that a journal should be published under the Branch's auspices, and proceeded to appoint William Matthew O'Neil, the new Professor of Psychology at the University of Sydney, as its editor. As at that stage still the only professor of psychology in Australia, coupled with the fact that the Society was Sydney-based, O'Neil was the obvious choice but possibly not the wisest one. His heavy workload with his new department and its already commencing post-war expansion must have detracted from the commitment required to establish a new (and pioneering) journal, and in August 1948 he resigned the editorship

before a single issue had appeared, citing lack of time for 'extracting such materials from members'. His editorial committee had however taken the correct formal steps, first inviting papers from members of the Branch known to be actively working on particular problems or to have material available for rewriting, and then from 'eminent Australians abroad'; later, when this was productive only of promises, they circularized all members.[5] Clearly O'Neil had underestimated the demands of the position; equally clearly Australian academics felt no great pressure at that stage to get their names in print. The Executive's next choice was Donald McElwain, then of the Psychology Department at the University of Melbourne and from 1955 to 1983 the first Professor of Psychology at the University of Queensland. McElwain brought out the first issue the following June, and continued as editor until 1960.

All in all there have been eight editors over the 45 years of the journal's existence, with none since McElwain retaining the responsibility for more than five years. All have however shared with him two major career characteristics: active involvement in the running of the professional society (of which most have had a term as president) and professorial status; as well, almost as a corollary of the foregoing, all have been male. The regional domination so marked in the editorship of the *AJPP* has not been so obvious with the *AJP*, though half of them have at one time worked in the Sydney department. The 1983 review of the content and characteristics of the *AJP* over its first 30 years uncovers a somewhat similar picture of the contributors. The top 10 contributors over this period were all male, holding senior academic appointments; six had had 'a substantial relationship' with the Sydney department. Over 90% of the authors altogether were male; during the first five years only male authors published. This state of affairs was changing only gradually, so that in the third decade only 13.5% of authors were female - yet at the end of this period approximately 40% of APS members were women.[6] Thus, as is only to be expected of an establishment journal, the *AJP* has been very much the mouthpiece of its senior members, and has also been slow to throw off the effects of the earlier pre-eminence of Australia's first psychology department.

What are the implications for standard and content of being a 'national' journal, particularly one issuing from a country far removed from the birthplace of modern psychology? The editor for 1972-78, R. A. Champion, reported a high submission rate of papers from overseas authors, the quality of some of which led him to suspect that the *AJP* was a last port of call. The 1983 review shows that editors have not allowed the journal to be used in that way. The majority of papers have emanated from tertiary institutions, and increasingly these have been Australian, as were all non-tertiary sources. As regards content, a smaller review of 140 articles by Australian authors published between 1969 and 1973 analysed their

major themes in terms of the national origin of the previous work which had inspired them, and revealed a substantial local influence: 35 (or 25%) of them had an Australian provenance, compared to 7 British, 54 American and 44 unclassifiable.[7] These data suggest that the journal has clearly played an important role in the research culture of Australian psychology as a national phenomenon, but need not be interpreted as parochialism. Australian researchers with well-established international reputations continue to submit papers to the *AJP* ; perhaps in consequence some well-established overseas researchers do likewise. In recent years an effort has been made to raise the international visibility of the journal. At the local level, with today's system of nationally organized grants, reviews, refereeing and employment, the *AJP*'s function of acquainting Australian psychologists with the existence and general interests of each other is probably as least as important as acting as a vehicle for the exchange of ideas among specialists.

Notes

These are necessarily brief, relating mainly to secondary sources. Further details are obtainable from the author.

1. See K.D. White, P.W. Sheehan & P.J. Korboot (1983) The *Australian Journal of Psychology*: the first 30 years, *Australian Psychologist* 18, 261-272.

2. See A.M. Turtle (1988) 'Education, social science and the common weal'. Pp.223-246 in R. MacLeod (ed.) *The Commonwealth of Science*. Melbourne: Oxford University Press.

3. Notice in *Hermes* 28 (1922), 161.

4. See A.M. Turtle (1993) The development of institutionalized psychology in Australia between the wars: bibliographical compendium and commentary, *Historical Records of Australian Science* 9, 241-256.

5. Minutes of Annual General Meetings of the Australian Branch of the British Psychological Society for 1945, 1946, 1947 and 1948, and Minutes of Meetings of Executive Committee 12 November 1947 and 26 March 1948. Archives of the Australian Psychological Society held in University of Melbourne Archives, LS7/1/5-11, Boxes 1 & 2.

6. As for Note 1.

7. K.F. Taylor & R. Taft (1977) Psychology and the Australian zeitgeist. Pp.35-51 in M. Nixon & R. Taft (ed.) *Psychology in Australia - Achievements and Prospects*. Sydney: Pergamon Press.

Fairbairn and the moral dimension

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Abstract

The work of W. Ronald Dodd Fairbairn can be considered perhaps as a single true serious attempt to harmonize the psychoanalytical thinking and the Christian idea not only because nobody raised so radically and so consistently the question of nature of human morality in relation with the development of personality and the maturity and structure in the ego like he; but also the role of conscience was put in a new light by he.
In this paper we try to reconstruct that new basic approach in the psychical thinking which though is rooted in the Jewish-Christian cultural mentality, in the world of Bible and which for Fairbairn undoubtedly served as a basis in exploring the introspective nature of the ego-structure nevertheless remained strange for the psychoanalytical thinking.
It isn't known if Fairbairn made a self-analysis or not but on the basis of his theological education and biblically determined thinking we guess that his work is such a synthesis-attempt which means an epoch-making link between the psychological knowledge and the human-related revelation of God for the modern people.
In the history of psychology may be exciting to analyze the work of a scientist who undertook his Christian roots grounding the modern mentality but who at the same time made every effort to

understand the challenge of his age. He tried to approach to the psychoanalysts and psychoanalysis with an attitude being in the centre of his theory: unconditional acceptance and the assurance to live one's own right.

Fairbairn's personality is extremely interesting as well because he was a representative person of the English object-relation school where the members definitely expressed their sympathy in relation with Christian idea and faith. (Sutie, Winnicott, Guntrip).

The problem in the case of Freud

According to the psychoanalysis the moral is a part of the culture; the Superego is the product of the culture. It is neither more nor less. It is a being created by a certain culture; it is a Golem inside the individuality (Tamás Nyiri, 1994.). It come from outside and so it is inherently strange. The psychic principle terrorizing the worried soul with its strong voice was called censor by Freud. According to Freud the neurosis was caused and remained by the tyrannical character of this rigourous judge because it cannot produce any understanding in the human. The culture enters the individual life through the Superego and subverts the personality and the unique of the individuality depriving it of personal liberty by this way.

According to Freud the morality is a necessary ability. It is neither good nor bad in itself but the reality of the life causing for the people more suffering than would be required (Das

Unbehagung in der Kultur). [1]

The ultimate questions of morality are included in the concept of instinct for life and death. These instincts are manifested in every people and by determining their actions the moral world become social pragmatism. Living under the rule of these human instincts could be unable making a conscious decision about the moral quality of an action. The qualification of an action might be only later by the person only after the qualification of the society. In fact with this simplification the frightful Golem-cult of the Middle Ages was revived by Freud. The interior moral imperative was imagined as an other person built inside the human and being socially a perfect machine (Descartes) without any individual features and private will. [2]

Fairbairn's solution: the moral personality

In contrast with Freud according to Fairbairn the morality is an inherent human ability. He speaks about a premoral stage where an basic sense exits in the infant differentiating between good and bad. Fairbairn goes back to the biblical ancestral situation of the human that is to the choice between good and bad where this inherent sense is the basis of the moral ability; his fundamental idea is the Christian anthropology; he denies the determination of the biological character in the human nature. When Fairbairn analyses the decisions of an infant in the premoral stage in relation with his mother we can discover in his thinking the fact of God being psychologically perhaps an

precondition only: "(The non-Jews don't have the law. But when they freely do things that the law commands without even knowing the law, then they are the law for themselves. This is true even though they don't have the law. They show that in their hearts they know what is right and wrong the same as the law commands. And those people also show this by the way they feel about roght and wrong. Sometimes their thoughts tell them that they did wrong, and this makes them guilty. And sometimes their thoughts tell them that they did right, and this makes them not guilty.) All these things will happen on the day when God will judge the secret things iside of people. The Good News that I tell people says that God will judge people through Christ Jesus." (Romans, 2:14-16.)

The infant doesn't know the law yet, he is heathen. Nevertheless in this sense he is able to act good or bad according to his developing conscience. Still on the basis of this elementary sense he is able to distinguish in his premoral stage if his gradually divided primitive ego-stimulation and his movements and acts are pleasant or not for him or his environment. Fairbairn discovered that as soon as the Other begins to separate from his primitive ego exists the basic conflict of the personality which is called schizoid position by him and it remains throughout the life. In fact the first 'moral' conflict in the infant is the manifestation of the basic human conflict to the world. This conflict is recapitulated by Fairbairn when he describes about the position of the schizoid personality preceding the oedipal situation being primary at Freud. (See in the references!) [3]

The central position of morality in Fairbairn's personal-theory is a solution of that epistemological problem which is covered in Freud's psychoanalysis by adopting the instinct-determinism. Having an inherent attitude to distinguish morally between good and bad is an anthropological specific independent of the cultural directions. So this characteristic has priority over the cultural contents - that is over the moral regulation. The culture may have a tyrannical or repressive role but this have to be preceded by a decision in the personality with regard to follow the good or bad. The personality has priority so it is able to make decisions; so the Superego may not be only the creature of the culture. The human dignity and the ability of making conscious and voluntary decisions is rehabilitated by Fairbairn in the fact that the measure of personal maturity is that moral act being determinant according to the one's conscience too. [4] This measure of course depends on the culture but not alone. That is to say it couldn't have that Golem-role at all attributed to it by Freud.

By Fairbairn the morality appears as a substance of the human phenomenon including the intention of God according to which the human being has a prominent position in this created world. In his theory the Superego is replaced by a principle called persecutive ego-part; it is an important but not alienated suppressive part of the ego structure. By Erikson it means a negative conscience or rather an suppressive conscience. It produces the principle in the personality which fighting with the inherent human conscience and ability to distinguish between good

and bad since on the basis of identification with suppressive bad object it creates the cultural demands. This is such a channel for the suppressed bad object which canalizes the self-destructive efforts in the personality oppositing it with the healthy ego-parts as they were in a dock.

The picture of this kind degraded conscience is mostly related to the God idea of Old Testament. This is a threatening God idea giving law for Moses and being merciless to violators. In Freud this picture incites the phantom of rebellion. In the course of his rebellion Freud reaches the point where this ancestral situation is reduced to the ambivalent and conflicting relation with his father as were any connection between them. As if Freud's relation to God could be rooted in his relation with his father. This fusion in his approach becomes important principle in his theory when the kind God is covered by this bad father-imagination. On the other hand in the case of Fairbairn in the psychological system the forgiving love of God is represented by the central position of the unconditional acceptance while the principle of living one's own right is presented in the decision of God giving free will for human being. This latter one indicates the proper operation and role of the conscience too since inherently it is considered as derived from God as for the basic attitude and fitted to the given culture as for its contents. However here can get in the Word of God and can clean from the deviations of divine will. [5] According to Fairbairn by getting the suppressed conscience in the role of persecutive ego-aspect is expressed that the devil

exists inside the personality by the bad objects. The developed persecutive capacity and thinking and anxiety cause the turning away from divine truth and the grace of absolution. Theologically Fairbairn's formulation isn't so clear but according to his therapeutical message the patient have to be loved and accepted. So he dares to undertake the moral importance and consequences of his decisions. In fact this is the message of Gospel. In order to the human may become a useful means for God he have to leave and deny his previous live; he have to be died. 'The person that wants to save his life will lose it. And every person that gives his life for me and for the Good News will save his life forever." (Mark 8: 35) In order to do this one have to experience the love.

Fairbairn's analysis include the revelation of God which is conspicuous when he refers to the determinative role of the good objects and bad ones in the premoral stage - when he mentions that also the infant makes a decision in relation with these objects. We can accompany along his thinking the dichotomy being between the existence of the old human and the new one. According to Fairbairn the basic conflict is caused by the schizoid position of the human being - this theologically is the separation from the God. The dichotomy of this separated life is the basis of the biblical anthropology; the duality of the natural human (homo naturalis) refused the redemption and the redeemed one (homo religiosus) inside the personality is psychologically the battle between the good and bad inside the ego. The biblical human was the basic idea in Fairbairn's

personal psychology. His psychological concepts are corresponded to his biblical human ideas and the role of conscience and different conditions of conscience (dark, deteriorated). And the description of the schizoid position is corresponded to the biblical characteristic of the people being in the condition of judgement [6].

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[2] Sigmund Freud: Das Unbehagung in der Kultur (1930) Ges. Werke, Band 14. 1948 by Imago Publishing Co. , Ltd., London
David M. Woulff: Psychology of Religion
John Wiley and Sons, New York, 1991. pp. 253-316.
[3] "He aslo keeps his love shut in because he feels that it is too dangerous to release upon his objects."
"The first is, as we have seen, that he feels his love to be destructive of those he loves. The second arises when he becomes subject to a compulsion to hate and he hated, while all the time he longs deep down to love and to be loved.
There are two further motives, however, by which an individual with a schizoid tendency may be actuated in substituting hating for loving-curiously enough one an immoral, and the ather a moral motive; and incidentally these would appear to be specially powerful motives in the case of the revolutionary and the Quisling. The immoral motive is determined by the consideration that, since the joy of loving seems hopelessly barred to him, he may as well deliver himself over to the joy of hating and obtain what satisfaction he can out of that. He thus makes a pact with the Devil and says, 'Evil be thou my good'. The moral motive is determined by the consideration that, if loving involves destroying, it is better to destroy by love, which is by rights creative and good. When these two motives come into play, therefore, we are confronted with an amazing reversal of moral values. It becomes a case, not only of 'Evil be thou my good', but also of 'Good be thou my evil'. This is a reversal of values, it must be added, which is rarely consciously accepted; but it is none the less one which often plays an extremely important part in the unconscious - and that this should be so is the third great tragedy to which individuals with a schizoid tendency are liable."
(W.Ronald D. Fairbairn: Schizoid Factors in the Personality [1940], In: An Object-Relations Theory of the Personality, Basic Books, Inc. New York, 1954. pp. 26-27.)
[4] Sándor Veres and István Kapás: Interpretation of Freud's seduction-theory in Fairbairn's theoritical system
In: 11th Conference of CHEIRON, Groningen, The Netherlands, August 26-30 1992, pp. 292-304.
[5] Harry Guntrip: The Attitudes of Freud and Fairbairn to Science and Religion.
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L'Année Psychologique et son réseau :

lectures et fabrication d'une nouvelle discipline

Geneviève Vermès[1]

L'Année Psychologique créée par Binet et Beaunis en 1894 est, comme les autres revues de ce type à fonction d'ouverture et de constitution d'un réseau et d'une discipline, composé de mémoires originaux et d'analyses bibliographiques. Celles-ci sont de 2 types: revues critiques et table bibliographique. Elles occupent les 2/3 des 600 jusqu'à 1000 pages de L'Année. L'examen de leur structuration et du changement de leur importance donne à voir à la fois les matériaux et les machines, le réseau national et international des travailleurs, et l'objet final envisagé et réajusté régulièrement dans les 2 premières décennies de L'Année. La table est normée au plan international, l'organisation des analyses manifeste la dynamique née de la rencontre d'un projet très souvent remodelé et des matériaux que la psychologie extrait des disciplines connexes déjà organisées avec organe régulier de publication. Les mémoires originaux présentent les quelques rares recherches empiriques que les membres du réseau peuvent effectuer. Lorsque Pieron reprendra l'Année, en 1912, il suivra la même politique éditoriale.

Au sein des recherches sur l'histoire des disciplines scientifiques, les place et fonctions de leurs revues sont encore très peu étudiées. Je vais essayer de vous montrer avec l'aide de quelques matériaux,de quelques produits finis[2] et de quelques transparents, que l'Année Psychologique en tant que revue, au sens propre du mot, a, dès sa lancée en 1894, fabriqué, en réseau avec d'autres revues de même type et partageant le même projet , le schéma, la forme et l'extension de la nouvelle discipline.Elle aura néammoins toujours une spécificité propre, liée aux hommes qui la dirigent et aux situations que connait la psychologie en France.

En disant "fabrication" je voudrai porter l'attention sur le travail inlassable de lectures et prises de notes, de mise en fiches, de tris et de rangement organisé de milliers de textes par an (lettres, périodiques, ouvrage) effectué par quelques personnes voire même par une seule personne à certains moments. Des travaux de tout niveau : thèses, monographies, compte rendus d'observation et d'expériences, traités..., ont été choisis, analysés, critiqués, résumés, positionnés les uns par rapport aux autres, hiérarchisés par un groupe de lecteurs dirigés, pour l'ensemble de la psychologie moderne des langues allemande, anglaise et française à ses débuts, par une poignée de personnes clé. Ces circulation

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[2]Je remercie la Directrice et le personnel actuel de la bibliothèque H. Piéron ainsi que Madame Marthe, le responsable des revues aux Presses universitaires de France, issues pour partie de Alcan, et qui poursuivent la publication de l'Année psychologique, Madame Claire Bresson, légataire puis donatrice des archives et de la bibliothèque d'I.Meyerson, et Madame Gratiot Alphandéry pour le récit de ses premiers souvenirs de jeune psychologue dans les années 20.

et production de lectures -compte rendus ou analyses ont organisé, j'en fais l'hypothèse, avec un esprit de corps et de système continûment le "nouveau mouvement académique" de personnes considérés come psychologues, et de thèmes traités, comme psychologiques. C'est probablement ce mouvement ,travaillant à la mise en réseau des personnes et des thèmes, et à leur organisation, qui va porter l'autonomisation et la spécification de la psychologie comme discipline scientifique.

Composé de quelques dizaines de personnes clè au tournant du siècle, ce réseau de travail de définition en comprendra quelques centaines juste avant la seconde guerre mondiale. En effet, tout en développant travaux d'observation et lectures-compte rendus, les premiers ont déployé une activité considérable de créations d'institutions spécifiquement psychologiques à partir des positions qu'ils occupent et peuvent conquérir, compte tenu des contextes et des situations historiques qu'ils vont vivre au sein des grands États où ce mouvement international se construit : l'Allemagne, les États Unis et la France essentiellement, auxquels il faut ajouter certainement la Russie, la Belgique et la Suisse, l'Italie, les pays scandinaves et l'Espagne si l'on suit les travaux de l'époque qui sont entre les mains des lecteurs psychologues de la Revue philosophique et de "l'Année", nom que les américains donne familièrement et avec un très grand respect et admiration à la grande revue de Binet.

A l'époque où un réseau de psychologues se met en place et se présente comme tel, en constituant le "dans le champ" et le "hors champ", le "in group" et le "out group" autour de circulation et d'organisation de lectures, il ne se trouve pas face à un manque. Depuis plusieurs dizaines d'années, les sociétés savantes en France, en Europe sont organisées selon une classification des sciences que les 18 et 19èmes siècles n'ont cessé d'élaborer. Les membres des sociétés savantes se rencontrent quelquefois, mais surtout ils s'écrivent et se commentent. Des grandes zones de curiosité puis de positivité se sont déjà dotées d'organe officiel et régulier d'échanges écrits à usage interne mais aussi avec une fonction externe: diffuser hors du cercle et s'étendre. Chaque savant appartient à de multiples sociétés.

A la fin du 19ème, pratiquement, toutes ces sociétés se publient sur le même modèle. Elles ont une ou plusieurs collections appelées le plus souvent "bibliothèque de..." dans des maison d'édition qui sont plus ou moins en train de se composer des créneaux, et une ou plusieurs revues. Les revues se proposant des bilans annuels, les Années fleurissent. Ainsi par exemple, la revue "l'Année Archéologique", en France, date de 1856. Ces dernières sont organisées de la même manière, en deux parties : mémoires originaux et analyses bibliographiques. Les analyses peuvent être accompagnées d'une table bibliographique et d'index. Elles précèdent rarement les mémoires originaux. Les mémoires originaux

présentent des travaux de première main et des revues critiques qui sont de fait des analyses .

Il nous semble pouvoir faire, en première approximation, l'observation suivante : plus la discipline est jeune, plus la partie "mémoire" est petite par rapport à la partie "analyse" . Ainsi 2 ans après le lancement de l'Année psychologique, Durkheim compose les 600 pages du premier tome de l'Année sociologique, en 1886-87, publiée en 1898 chez Alcan, avec 5/6 d'analyses et 1/6 de mémoires originaux. L'Année psychologique de Binet commence avec 2/3 d'analyses mais progressivement les mémoires se développeront. L'Année de Pieron présentera ce même déséquilibre entre mémoires et analyses et le maintiendra, nous en reparlerons plus loin.

La masse des titres traités par an, dès la départ dans les revues de psychologie[3] comme dans les autres à la même époque peut paraître extravagante. En moyenne entre 500 et 1000 titres. Ces chiffres peuvent monter à 3000 voire 4000 ! Un élément pour rendre compte de ce phénomène est certainement à chercher du coté des pratiques de lecture à la fin du 19ème et au début du 20ème siècle. Les tirages des publications font pâlir de jalousie les éditeurs d'aujourd'hui. Les personnes qui lisent, lisent énormément. L'abonnement à tout type de périodiques est bien établi. Les savants et intellectuels, les médecins et les philosophes, qui lisent tous plusieurs langues, ont non seulement d'importantes bibliothèques, mais reçoivent de nombreuses revues. Les bibliothèques générales des universités se constitent de manière systématique et sont abonnées aux revues, les laboratoires s'abonnent aussi aux revues.

Ce n'est pas seulement pour recevoir l'information, ils la produisent et sont à l'origine et œuvrent en même temps dans de nombreuses publications. Les intersections entre celles-ci sont donc très grandes. L'exclusivité n'est assurément pas, à cette époque, le moyen d'assurer la vie d'un patron, de son école ou réseau, la vie d'une discipline. Binet et son équipe fournissent en revues et analyses la Revue philosophique et l'Année Biologique par exemple, de façon systématique et importante. Les contrats des éditeurs semblent être homogènes, une revue à environ 600 pages par an, si elle parait tous les 2 mois, comme L'Année et les autres revues du même type, chaque fascicule doit avoir environ 100 pages.

En 1894-1895, lorsque Binet lance l'Année psychologique chez Alcan avec Beaunis, à partir du bulletin des travaux du laboratoire de

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[3]Ce que j'avance dans cette partie est tiré de l'examen de l'ensemble,passablement écclectique aux yeux d'aujourd'hui, des collections de revue qui débutent avant les années 1920 et qui se trouvent à la bibliothèque Henri Piéron,. Cette bibliothèque s'est constituée et continuée, quasi exclusivement d'abord puis très largement, à partir, au moyen de ce travail gigantesque de lectures-compte rendus.

psychologie physiologique, ce n'est pas qu'il dispose de plus de travaux et de plus de mémoires originaux, c'est, comme les autres fondateurs de revue à la même époque se trouvant dans la même situation, pour ouvrir et organiser un réseau et constituer un champ nouveau spécifique. Comment réaliser ce tour de force sans moyen, à la fin du siècle dernier, de proposer à ses lecteurs 600 pages par an de psychologie nouvelle, alors que d'autres revues la présentent ? Justement grâce au fait que d'autres revues la présentent. Cette réponse n'est pas paradoxale.

L'examen de L'Année montre la même démarche que celle que met en place Ribot pour créer la Revue Philosophique, sur le modèle de Mind, de Robertson. Comme tout autre nouvelle revue à vocation constituante, l'Année va profiler un centre et l'alimenter de ce qu'elle dit être à ses frontières. Elle dit comme les autres exclure, mais ne cesse d'inclure à partir de zones de positivité relativement bien constituées. Comme ces zones sont en réelle expansion, cela s'avère possible. Le travail d'une revue constituante, type l'Année, va être d'extraire, réunir en son sein et articuler. Cette dynamique va être lisible, énoncée clairement et avec régularité dans des avant-propos, des bilans. L'orientation, c'est à dire l'exposé du centre et des frontières, va continuellement être peaufinée par le directeur de la revue tant que celle-ci a plus vocation à constituer qu'à exposer.

L'énoncé de cette orientation (bilan et projet) est organisé en schéma : thèmes, sous-thèmes, articulation plus ou moins hiérarchisée. On peut s'attendre à ce que cet énoncé d'un schéma directeur reprenne le plan de la partie majeure de la revue, les analyses bibliographiques. Il n'en est rien. Dans l'Année, le plan des analyses bibliographiques est relativement différent du programme d'une part ; et d'autre part, au sein des analyses bibliographiques, le plan des analyses ne coïncide pas avec le plan de la table bibliographique. On peut s'en étonner, d'autant que le plan programmatique que Durkheim expose dans l'Année sociologique et le plan des analyses bibliographiques de celle-ci se recouvrent complètement ; les chapitres sont affectés à ses "élèves". Chapitres, responsables et organisation sont maintenus tant que demeure l'Année sociologique (1ère série). Il en est de même dans l'Année Biologique par exemple.

Le décalage que l'Année psychologique présente entre les plans de ses différentes parties ne peut probablement pas être considéré comme insignifiant. Il atteste certes de différences d'origine, mais plus profondément, peut-on en découvrir les raisons? Quelle est la situation de Binet, de la psychologie, à cette époque, en France, qui puisse rendre compte du maintien de ce décalage?

On peut reconstituer l'historique des tables bibliographiques de l'Année. Il est à chercher dans le "Zeitschrift für Psychologie und Physiologie der Sinnesorgane" d'Ebbinghaus et Kônig. En 1890, ils publient le premier index des publications psychologiques. Binet le

connait. On sait, en effet, que, dans la bibliothèque du laboratoire dit de la Sorbonne créé en 1889 et placé sous la direction du Dr Beaunis - Binet en est sous directeur et Philippe le préparateur -, ils reçoivent 4 revues "philosophische Studien" de Wundt, l'"American Journal of Psychology de Stanley Hall(1887), le "Zeitschrift für Psychologie und Physiologie der Sinnesorgane",et le "Beitrag fûr experimentalellen psychologie" de Münsterberg.

Le plan de la table bibliographique suivra d'assez près cet index. Sa stabilité et sa généralisation proviennent de l'organisation internationale du réseau. Autour de Warren, le "business manager", et de l'American psychological Association, les publications psychologiques américaines s'organisent, sans concurrence d'abord, et se multiplient en se spécialisant les unes en rapport avec les autres. L'Année y collabore officiellement (Binet, Victor Henry, Nicolas Vaschide, le Dr Philippe, Larguier des Bancels, Revault d'Allonnes puis Piéron). Ils vont assurer le traitement des matériaux français et de leur réseau, et en échange recevront ceux traités par les autres . En 1898, la table est signée par Warren, Vaschide et Borchardt, en 1900 The Psychological Index de la Psychological Review est signé par Warren (Princeton), Larguier des Bancels (Paris), Deaborn (Boston) et Hirschlaff (Berlin) . Elle parait à la fois dans l'Année et et le Zeitschrift. Pour le traitement des informations des consignes précises sont fournies aux collaborateurs. (La fabrique traite, en 1905, 3445 titres ; pendant la première guerre mondiale, l'apport de l'Allemagne et de son réseau disparait, celui-ci réintroduit, l'index traite 4000 titres par an ; à partir de 1926, l'URSS s'y associe).

Il n'est pas évident de proposer un autre index, Vaschide, qui s'y essaye dans une publication autonome en 1902, est vivement rappelé à l'ordre, par exemple dans la Revue philosophique. Sous la direction de Binet puis de Piéron ce plan restera à peu près le même : les questions générales et traités seront placés en tête, les titres des rubriques seront des termes de plus en plus généraux.Mais si on examine ce plan de plus près et qui on le compare au plan des analyses, on en comprend mieux la structuration et ses enjeux.

La première partie : système nerveux, sensation, attention, mémoire, motricité, reste à sa place, mais ces rubriques sont d'une importance très inégale et variable. Cette norme sur laquelle tous s'entendent est suivie d'une seconde série de rubriques qui prennent de plus en plus d'importance. C'est autour de l'émergence de ces rubriques et de leur articulation jamais bien établie que se joue, de fait, le plan des analyses d'une part, et des avants propos et bilans de l'Année d'autre part.

Il est plus difficile d'analyser cette seconde partie de la table qui correspond le plus souvent, dans ses éléments mais non forcément dans leur structuration, au plan des analyses bibliographiques. L'ensemble de ces rubriques va présenter un bouillonnement, des transformations certainement les plus fondamentales et les plus signifiantes pour prendre

la mesure de l'importance de ces secondes séries d' analyses. Celles-ci rendent bien compte de la dynamique de la nouvelle psychologie en France et dans son réseau proche, de ses forces, tensions et contradictions.

Les rubriques : psychologie comparée, psychologie des enfants, psychologie individuelle, psychologie anormale et morbide, auxquelles il faut rajouter, dès 1896, psychologie animale, vont se composer, se décomposer, s'articuler en un incessant bouleversement. Certaines rubriques ne vont pas apparaitre ou disparaitre dans les analyses de l'Année, comme elles le font dans les revues américaines. On va assister, spécifiquement dans l'Année soit à des inflations de certaines rubriques, soit à des récessions.

Le phénomène le plus massif est la fabrication, progressive et complexe, d'une archirubrique: psychologie comparée. On voit la psychologie pathologique perdre de son importance relative et être incluse, comme psychologie de l'enfant, psychologie ethnique, caractère, psychologie animale, sous cette archirubrique. Dumas lui même la reprendra dans son Traité de Psychologie, et Piéron systématiquement dans L'Année, alors qu'elle ne parvient pas véritablement à émerger dans la littérature anglosaxonne. Il faut dire qu'en France, elle a été élaborée par Ribot.

La psychologie individuelle va dans l'Année de Binet, naturellement, aussi se gonfler en intégrant psychométrie, personnalité..., elle se trouvera redécomposée dans l'Année de Piéron. Les dynamiques locales et nationale sont en oeuvre dans ces jeux sur les rubriques et leurs relations. L'Année est à la fois fermée et ouverte, en position de fermeté ou de négociations.

Un phénomène va rendre perplexe les américains: la diminution des analyses dans l'Année de Binet. Ils faisaient de l'Année une bible pour ses analyses parfaites qui rendaient inutiles le recours aux textes, pointaient l'essentiel, et donc économisaient considérablement les efforts de recherche et de traitement des informations. Dans l'Année en effet, les mémoires originaux vont de plus en plus occuper le terrain des analyses, et ce, en trois paliers 1896,1904,1908, jusqu'à devenir portion congrue avec même disparition de la table bibliographique.

Que se passe t'il? Binet, toujours pratiquement sans moyen, ne peut-il plus matériellement occuper tous les terrains? Pour des raisons, dont j'ignore tout, en 1896 (publiée en 1897) l'Année migre chez l'éditeur Schleicher où Binet et V. Henri lancent une collection de psychologie de l'enfant et de l'éducation. L'Année a désormais un secrétaire de rédaction (rémunéré ?) V. Henri. Le laboratoire, outre Philippe qui continue les analyses bibliographiques pour les réseaux national et international dispose de deux nouveaux préparateurs Courtier et Vaschide. En 1897, l'Année publie une énorme quantité de travaux de

Binet et Vaschide, et dans les années qui vont suivre cette nouvelle politique éditoriale se développera.

On peut être tenter d'analyser cette disparition des analyses par l'hypothèse que j'ai faite plus haut : les analyses tiennent seulement la place que les mémoires originaux, les travaux de première main, insuffisants ne peuvent occuper. Il me semble cependant que l'on ne peut qu'en partie soutenir ce propos. Il est probablement nécessaire, mais je ne pourrai, faute de temps, l'exposer ici, de prendre en compte la nouvelle situation de l'Année en France, à Paris. Une nouvelle revue est née avec son réseau : le Journal de psychologie Normale et Pathologique, conçu par Janet et Dumas chez Alcan, d'où n'oublions pas l'Année est partie.

Le Journal devient très vite l'organe de la Société de psychologie qui s'autonomise , pour des raisons de législation associative, de l'Institut Général Psychologique au sein duquel elle s'était organisée. La Société, fermée, dirigée par Janet et Dumas, a, à cette époque et jusqu'aux années 20, très nettement un profil de psychopathologie. Binet n'a pas investi la Société de Psychologie, il est de plus en plus engagé dans des questions sociales d'éducation. Il a créé, avec F. Buisson, la Société libre pour l'étude psychologique de l'enfant, qui crée aussi son bulletin.

Le Journal va, bien entendu, présenter beaucoup d'analyses bibliographiques, sur la base d'un autre réseau plus latin. Le Journal parait aussi six fois par an, il a aussi au minimum 600 pages. Ses compte-rendus sont organisés exactement sur le modêle de l'index international et l'Année, à ceci près que psychologie pathologique s'oppose à psychologie normale qui contient toutes les rubriques de l'index, et que psychologie pathologique, à elle seule, se veut aussi importante que toute la psychologie normale. La psychopathologie a donc enfin sa revue en lien avec la psychologie nouvelle.

Atteint d'une tumeur cérébrale, Binet meurt brutalement en 1911. L'Année est publiée en 1912 par Simon et Larguiers des Bancels, sans aucune analyse bibliographique. Pour la reprise du labo et de l'Année, les collaborateurs de Binet (Les Drs Simon et Philippe), soutenus par Ribot, postulent, ainsi que d'un coté Janet, professeur au Collège de France sur la chaire de Ribot, qui tient à maintenir le lien entre la chaire et le labo, et Toulouse de l'autre qui propose la fusion de son labo de psychologie expérimentale de Villejuif (EPHE 1901) et de prendre Piéron qui y travaille et y occupe des fonctions éditoriales, comme directeur adjoint. L. Liard alors recteur, grand instituant de la psychologie depuis la fin du 19ème, s'oppose à la fusion proposée par Toulouse, et à l'orientation trop exclusivement pathologique de Janet. Piéron soutenu par Gley, professeur de physiologie à la Sorbonne, est élu ; il a 31 ans, a déjà déployé une énorme énergie au sein de plusieurs sociétés savantes, philosophe, il vient de soutenir une thèse de sciences naturelles.

Le premier numéro de Piéron renoue avec l'expérience des pionniers. Quasiment tout seul, il assure 3 des mémoires originaux avec

ses étudiants EPHE; il écrit 2 des revues, et presque toutes les 600 analyses bibliographiques. Ce travail de titan, il va le poursuivre comme Binet, très longtemps avec des moyens dérisoires.

La guerre 1914-1918 va changer la donne au sein de ce duo, Année- Journal où les deux partenaires avaient à se définir l'un par rapport à l'autre. Une grande quantité de psychologues meurent au cours de cette période. Le secrétaire de rédaction du Journal, élève de Dumas, meurt à la guerre. C'est Ignace Meyerson qui est choisi pour le remplacer. Meyerson,étranger, n'est pas officiellement médecin, philosophe, il a aussi fait des études et des travaux de physiologie. Il sera toute sa vie, très proche des philosophes psychologues de la Sorbonne et de toute la dynamique des sciences sociales d'alors. Il va avoir un poste au labo de Piéron où il deviendra sous directeur. Bien qu'il soit polyglotte, énorme lecteur et organisateur pour le Journal et la Société de psychologie, dont il est tout naturellement le secrétaire, d'un énorme réseau national et international, il ne met pas en place d'analyses bibliographiques sur le modêle du British Journal of Psychology (1905) organe de la société anglaise de psychologie. Pièron, à ce moment, réintègre l'Année chez Alcan, qui publie de ce fait les deux revues de psychologie; celles-ci ne peuvent donc qu'être complémentaires.

Les analyses bibliographiques seront effectuées par la seule Année. Elles vont constamment et de plus en plus occuper de place. Leur planification va petit à petit élaborer subtilement une articulation des intérêts français et des intérêts de la Psychological Review Company. Les rubriques vont augmenter en nombre, mais de façon spectaculaire une, et non des moindres, va se rétrécir comme une peau de chagrin. Piéron et Meyerson dirigent les deux organes officiels et spécifiques de la Psychologie française. A la Société, comme dans le Journal, la psychopathologie perd continuellement du terrain.

The spell of the equilibrium model (1870-1918): the case of Gerard Heymans (1857-1930).

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ABSTRACT

European naturalistic immanentism around the turn of the century can be charted by distinguishing three natural scientific discourses which transmitted the naturalistic view of man and his world (society) to the "cultural interdiscourse" (J. Link 1986) of the time: the discourses of (1) evolutionary (psycho)biology, (2) degeneration theory, and (3) quasi-organicistic equilibrium thought ("naturalistic equilibrium monism").

It is argued that (1) Heymans' work belongs to the third type of scientific naturalism and as such is a beautiful testimony of the spell exercised by the equilibrium model; and that (2) situating his work more precisely on the spectrum of European naturalistic equilibrium monisms suggests the need for a change of perspective in Heymans-interpretation, such as to highlight his affinity to epistemological monism and the outspoken anti-romantic features of his work. Paradoxically, carrying through this correction in Heymans-interpretation helps to see that and how Heymans' thought in an indirect way reflected the spell of the monistic equilibrium model which in its metaphysical form originally belonged to the conceptual inventory of "romantic" organicism ("Naturphilosphie", identity philosophy).

The spell of the equilibrium model (1870-1918): the case of Gerard Heymans (1857-1930)

Paper to be presented at the Cheiron Conference, Paris 1994,

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The decades around the turn of the century testify to an episodic flowering of naturalistic views about man and (his) world. The unparalleled valorisation of the natural sciences and the in tandem emerging varieties of so-called "scientific philosophy" (emiriocriticism [Avenarius, Mach; Ziehen]; immanent philosophy [Petzoldt, Schuppe, von Schubert-Soldern, Kaufmann, Rehmke]; inductive metaphysics [Lotze, Fechner, Ed. von Hartmann, Bergson, Heymans, Peirce, James] and so-called "Wissenschaftssynthese" [Mach, Wundt, Spencer]), were matched by a keen interest in and a thoroughgoing reception of, the dogmatic naturalism of Spinoza, Marx, Nietzsche, and (somewhat later) Freud, which, because it was only in part chanelled by way of the academic circuit, could become a major factor in the establishment and spread of a truly European *naturalistic immanentism* around 1900 across the whole range of the cultural spectrum.

European naturalistic immanentism around 1900 - the cradle of all later "naturalistic" attempts at liquidating the authoritarian-paternalistic complex of (post)theological/philosophical traditionalism (in the fields of theology, epis-

temology/philosophy of science, philosophical anthropology, social and political philosophy, ethics and philosophy of law) - can, conceptually speaking, be charted by distinguishing three natural scientific discourses which transmitted the naturalistic view of man and his world (society) to the "cultural inter-discourse" (J. Link 1986) of the time: the discourses of (1) evolutionary (psycho)biology (Spencer, Darwin, and especially Lamarckism), (2) degeneration theory (especially in France: Morel, Moreau de Tours, Magnan), and (3) the discourse of quasi-organicistic equilibrium thought.

Taken together they mediated a threefold attack of scientific naturalism against the bulwark of tradition and traditionalism, deploying the naturalistic offensive along the lines of biologism, psychologism, sociologism. Each of them lent support to (the expression of) a different type of world-view ("ideology") and was, dependent on local circumstances and national needs and preferences, geared to different social-political programmes. Thus, evolutionary psychobiology with a dogmatic core of Lamarckistic heredity theory was important as theoretical basis of the eugenics movement (primarily in America, but also in Europe) and as the underpinnings of reform proposals in the sphere of the administration of law and the dealing with so-called born criminals inspired by the criminal anthropology of Lombroso (especially in Europe). Exclusively European, so it seems, was the tragic vitalistic naturalism characteristic of fin de siècle France and Austria, which was supported by degenerationism cum hereditarianism, and fueled social and political debate in the

context of international comparison in terms of national ("racial") superiority/inferiority. And the third view, which for want of a better designation, I will describe as naturalistic equilibrium monism and which found conceptual support in physics (thermodynamics, energetics), neoclassical economics ("equilibrium analysis"), psychophysics and the science of work (A. Rabinbach 1990), buttressed the programmes of social Helmholtzianism and social energeticism.

The point I want to make in this paper is twofold: (1) that Heymans' work belongs to the third type of scientific naturalism (or "psychologism") and as such is a beautiful testimony of the spell exercised by the equilibrium model; and (2) that situating his work more precisely on the spectrum of European naturalistic equilibrium monisms of the time suggests the need of a change of the older, but still current perspective in Heymans-interpretation, which, on the basis of a superficial resemblance of Heymans' "psychic monism" (or psychomonism) with irrationalistic varieties of (non- or rather anti-scientific) naturalistic monisms popular around 1900, subsumed both under the category of "(neo)romaticism" (cf. G.J.P.J. Bolland, the Neohegelian Leyden philosopher, whose implacable criticism of Heymans, a.o. in his "Schelling, Hegel, Fechner en een nieuwere theosophie" may be largely responsible for this interpretation, and J.J. Poortmann, also a Leyden philosopher, who, building on Heymans, developed a philosophy of his own which intended to provide the philosophical underpinnings of theosophy. More recently: I. Bulhof 1983). Whoever takes the trouble of thinking through the

implications of Heymans' commitment to the programme of "scientific philosophy", follows up the connection of Heymans with thinkers of similar persuasion (epistemological monism in empiriocriticism and philosophy of immanence), and takes due notice of his works in the field of epistemology and philosophy of science (including his first dissertation of 1880, never mentioned in this connection, but in fact a philosophy of science study related to political economy), will appreciate my contention, that Heymans suffered a fate, remarkably similar to that of Fechner, whose philosophy around 1900 became prototypical for an anti-scientific "Gefühlsphilosophie" (cf. K. Lasswitz 1904), and should be placed, alongside the older Fechner, and Mach in the earliest history of the scientific naturalism which bridges German "Naturphilosophie" and the "scientific world-view" of the Logical Positivism of the Wiener Kreis (cf. M. Heidelberger 1993, who convincingly argued this point with respect to Fechner).

Paradoxically, highlighting Heymans' affinity to *epistemological* monism and the consequent stress on the rationalistic-scientific; outspoken *anti-romantic* features of his scientific-philosophical orientation prepares the way for showing that, and how, his thought, in an indirect way, reflected the spell of the monistic equilibrium model which in its metaphysical form originally belonged to the conceptual inventory of "romantic" organicism ("Naturphilosophie", identity philosophy), next was "naturalised" in post-romantic organicism (Fechner's "principle of stability") and eventually achieved prominence by way of thermodynamics (Boltzmann

1886, Stallo 1901) and conceptually related disciplines such as psychophysics and neo-classical economics, in epistemological monism (Avenarius: "Prinzip der Beharrung"; Mach: "Prinzip der Kontinuität"; and especially J. Petzoldt 1904 . To which may be added Heymans' 1921 study "Über die Anwendbarkeit des Energiebegriffes in der Psychologie").

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ABSTRACT

THE GENEALOGY OF THE CONCEPT OF POLITICAL BEHAVIOUR; GRAHAM WALLAS AND THE STUDY OF POLITICS IN BRITAIN.

The very idea that you could study how people behaved politically, find out why people did not vote, or who voted for which party and for what reasons, or how the Senate operated in relation to the Congress, became a social psychological research issue in America and not a historical one (Britain relied on Maitland, Figgis, Cole). Wallas was a participant in the creation of a new modernism which contributed to the articulation of a new liberalism for America following the First World War (Harold Laski is another figure). The complex map of **Political Science as a Social Science** needs to be understood as a dynamic conceptual and institutional relationship between the study of politics and the research of social psychology in the context of a Pre-War society (1900-1914). As a historical point the relationship between politics and social psychology in the terms of the pedagogy of politics in Britain, was not considered appropriate competent apprenticeship or practice for the discipline (Kuhn) until the late 1960s.

My present disposition is to consider the evolution of the concept of political behaviour in Britain in the context of the Fabian Society and the writings of Wallas prior to the inter-war period in British society. The concept of political behaviour in this case is directly associated in Britain with socialism and the need for a socialist movement to contest the old Whig and Tory grip on the social psyche of the nation. This is also an exemplar of a affirmative and critical modernism where the resonance of scientific knowledge it was believed could echo and crumble the marble halls of the establishment. Wallas is a key figure in the history of British Political Science and an important subject for research since he operated within the contexts of the academy, pedagogy and political activism.

The conclusions of the paper lends itself to discussion on the history of the social sciences in terms of;

1.The fluidity and dynamic of disciplinary boundaries (Politics and Social Psychology).
2.The evolution or genealogy of the concept of Political Behaviour in the context of fixed disciplinary idioms in history and philosophy.
3.The interconnections between the study of politics and sociology.
4. Conceptual formations in political science connecting political education, research and action.

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A GENEALOGY OF THE CONCEPT OF POLITICAL BEHAVIOUR
THE PAPER TIGERS : INTELLECTUAL LIBERAL-SOCIALISTS IN BRITAIN

[Malcolm Vout Nottingham Trent University]

Wallas: The Radical Generalist - In An Era of Modernisation and Modernity and the Culture of the Specialist

Reform, reform and more reform on health, education, working conditions was the committed battle hymn or psalm for the liberal-socialist intellectuals of Wallas' persuasion. The aim of political education was to provide guidelines for a new social administrative consciousness and practice. In a sense the new political science advocated by Wallas was in fact a study of social administration with a philosophy of method promoting the importance of social psychology.

Understanding Wallas, involves uncovering the background of intellectual socialism in Britain suited to a liberal- evangelical programme for change through educational reform. In the first half of this century intellectuals were anxious about the ever present forces of a centralised modern state and a capitalist environment. The social and cultural pressures created though this condition of modernisation could result in social disorder. The radical generalists were concerned that the social psyche would be unable to cope without new theories of social organisation backed by practical changes in state education and administration. What is required are new guidelines for political conduct or behaviour.

In this instance the genealogy of the concept of political behaviour is connected to developments in the study of social organisation and social psychology. Marx of course was available but his position was revolutionary and deterministic and dealt with economic rather than cultural radicalism.

In liberal intellectuals circles there was a search for a social synthesis, a Hegelian moment, where the disgust and fear of *The Great Industry* and its inhumanity, could be contained.

The search was for new intellectual and organisational agendas which could

articulate and above all *DIAGNOSE* the problems of social organisation in a large modern state.

I think that a starting point for any genealogy of the social sciences begins with a 19th. century modernism that releases a consciousness in search for a new epistemology, a social epistemology that could address and articulate concerns over mass production and mass culture. The intellectual responses were diverse with respect to this universal, economic and cultural environmental change - the rise of industrial capitalism. Where the pace of change is rapid, questions are raised by intellectuals on the capacity or ability of members of society to accommodate, cope with the environment. It was a question of the relationship between the individual and the economy and how the individual would adapt, evolve, in the face of this capitalist machine of industrial progress. It certainly did concern Beatrice and Sidney Webb - the list of contributors is titanic. Mass Production, creating Mass Society and Culture presented riddles for new 20th. century liberal and socialist intellectuals. Darwin (Malthus) was available but so were Le Bon, Pareto, Michels and others who insinuated that a rationalist, enlightenment view of democracy was an illusion, a bogus, simulated view of human nature and reason. The enlightenment as a fraudulent and inauthentic 'symbol' was taken up later by Adorno and Horkheimer in the *Dialectic of the Enlightenment*. This intellectual conundrum which results in conjecture and often broad rhetorical sweeps on the nature of mass industrial society, is described by Wallas in the following terms:

Where economists have used the term The Great Industry for the special aspect of this change which is dealt with by their science, and sociologists may conveniently call the whole result the Great Society.(Wallas, 1914:3.ff.)

But the whole result, is exactly what intellectual liberal-socialists like Wallas were questioning. Christopher Harvie in *The Lights of Liberalism (1976: 210)* comments that Perry Anderson's 'Components of the National Culture'

(Anderson,1969: 219-228) is valid commentary in the case of university liberals in the late 19th.century since they ignored sociological traditions which could have provided 'centres of inquiry into the nature of modern society and its culture..That task was left to foreign scholars - Ostrogorski, Lowell, Halevy, Mantoux - and the other traditions of education within Britain - Charles Booth, the Webbs, Graham Wallas and Patrick Geddes' (Harvie, 1976,p.210.).

The parochialism of social science scholarship in Britain can in part be understood by the intellectual vice that university liberalism rather than socialist-liberals operated since the late 19th. century. A desire to coalesce, confederate with the landed aristocracy and its structures rather than radically reform society.

Wallas' aspiration to construct a study of politics through sociological categories has echoes in the struggles of political educators like Finer, Mackenzie, Moodie in 1950s Britain who also attempted to inject a sociological dimension into the study of politics. It was a challenge in 1900 and in post-war Britain.

The sociological dimension for intellectuals including liberal-socialists in the late 19th. century, embraces a consideration of biological issues, relating human nature to the environment and biological inheritance (Darwin, Spencer). In politics Bagehot, in psychology Tarde, in sociology Spencer, consideration was given to the view that once the new generation had adapted itself to the new industrial environment, achieved through habit and imitation, it would be handed down to following generations. Darwin of course

> *"disbelieved in the doctrine of the purposive adaptation to environment : he believed in accidental variations, and held that accidental variations which suited environment were perpetuated by inheritance, ultimately because they were the fittest for that environment, but immediately because they were inevitably*

Social change became overnight a question of educating both the governing class and the lower classes towards a higher social morality, a moral sense of political conduct (Political Behaviour). Wallas uses the term social or political conduct rather than political behaviour.

The genealogy of the concept of political behaviour has its ancestry in this argument which advances the case for studies in the machinery through which a collective will is established and enforced.

> *The continued growth of Collectivism depends on the continued efficiency and acceptability of the machinery by which the collective will is ascertained and enforced (Wallas 1914: 316)*

Collectivism depends on an active political consciousness which needs to be supported though an efficient organisation of voting. Political behaviour needs to be monitored and organised in order to encourage and capture the working class vote. In Wallas' terms to create a Will-Organisation, to study and organise political behaviour, is essential for the continued existence of Collectivism in the context of a mass society where people are easily led and manipulated. A dangerous situation is always on the horizon in a country where democracy co-exists with great social inequality. For Wallas, Individualism, Socialism, Syndicalism, are in themselves an inadequate basis for the Will-Organisation of the Great Society *(Wallas 1914: 329-330)*. For the political eugenicists, Mussolini and others, the conundrum or riddle of social change was solved for the evolution of mass society, based on the crisis of international capital and culture creating a mass society, could open gates of propaganda on a scale unthinkable to previous generations. A new generation of political manipulators sustained by a belief that a mass society is the new form of civil society, quickly realised the importance of controlling rather than understanding political behaviour.

A genealogy of political behaviour, it seems to me, should consider the 19th. century environmental and biological ontology that challenges enlightenment

selected for survival in the course of the struggle for life."

(Barker,1924, p.92)

Social Darwinism as a 'symbol' entering the western social psyche highlights images of a process of evolution, adaptation, imitation. Darwin, introjected into a modern social psyche becomes Social Darwinism. The social introjection of images or symbols of human action are important in any consideration of the history and philosophy of the social sciences. This is the framework from which we can understand the human dynamics of culture (*Thompson 1984*). The cultural representation of Darwin pictures human nature in terms of a response to the stimulus of the environment [Spencer, Bagehot, Tarde] through imitation, accident, but above all perpetuated through inheritance. Though being acquainted with this symbolic discourse the problem still to be solved was how could a democratic system of government and administration be encouraged and created in a mass society that was based on mass production and technological innovation. What practical measures were necessary to ensure that social and political consciousness proceeded at the same velocity as the new environment of mass society and industrial capitalism.

Perkin (1989,p.156) comments on Social Darwinism in terms of the 'externalist' interpretations of Benjamin Kidd and Karl Pearson (Pearson being a reference for Wallas) who argued that individualism had failed the nation.

A study of the anthropological and psychological dimensions of the human personality could support collectivist social reform. Wallas articulates the view that Collectivist Socialism had become the most important political movement at the end of the nineteenth century. It encapsulates the view the Great Society could emerge by harnessing the immense wealth and productive power of the Great Industry in order to reduce increasing economic inequality. The 'true socialist' must be superior to class interest (Shaw, Wallas, Webbs, Mannheim) in order to focus on the habits, dispositions, of members of the Great Society.

and rationalist views on human nature (a pre-Freudian challenge). Attention to psychological perspectives, in terms of their ancestry and translation into political science texts is an important resource for a genealogy of the concept of political behaviour. The concepts of social psychology and social organisation translated into a study of politics certainly fuelled the work of Wallas.

Wallas comments in a post- Darwin context that

> *The biologists of our time have forced us to realise that such "acquired characteristics" are not inherited. (Wallas,1914,p.8)*

To the sober and evangelical liberal/socialist reformers in Britain like Wallas, understanding social change involved outlining a new post-utilitarian politics; a new post-darwinian social psychology; a new social logic in tune with images of a humane community for social progress; as a suitable conceptual foundation for the new organisation of *The Great Society*. The impulse to question the mercantile beliefs in boundless enterprise, technological and scientific progress, is rooted in Wallas' scepticism of Bentham's psychology grounded on the intellectualist assumption that

> *"all human action was the conscious search for the means of attaining some "end" or "good" other than the action itself" (Wallas,1914,p.100-1)*

Wallas objected to the hedonism and rationalism of Bentham that humanity could be understood through a monist social logic of pleasure and pain.

It was no doubt a struggle for Wallas to overcome the main civic and philosophical tract of the nineteenth century, to liberate the intellect from a Benthamite persuasion, yet Wallas was determined to dispossess the intellectual property of what we now call *conservative (liberal) rationalist, enterprise culture*. Wallas is an appropriate staring point in uncovering the ancestry of the concept of political behaviour. He attempted to link naked sociological empiricism and social psychology to an investigation of the problems of

organising and harnessing political consciousness in the context of twentieth century mass society and international capitalism.

I suspect that the genealogy or ancestry of research on political behaviour has its roots in a belief that the modernisation of the machinery of representative government needs to keep in line with the modernisation of the economy. In 1915 Wallas comments " Perhaps the most urgent need of our time is an adequate social psychology...that attends to the psychological condition of national and international feeling and action." *(Wallas Hibbert Journal 1915: 224-5).* It is the business of social psychology to set in motion a delicate process of analysis that can delineate an understanding of propagandist committees, local political associations and professional organisations. What is crucial for Wallas is a consideration of issues surrounding the organisation of a democratic collectivist social consciousness - a ethical and workable social psyche that can operate in the context of democracy and capitalism. This aim reflects the intellectual constituency of early twentieth century Western Political Science, and the focus it gives to the machinery of political life - the study of political behaviour.

Themes For Discussion

The translation of social psychological discourse and conceptual fromations into the study of politics as a basis for identifying the study of politics as political science.

Assesment of the problems of employing a genealogical analysis in the study of the history of the social sciences.

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Paper for the 13th annual conference of Cheiron Europe

Topic 6: Psychoanalysis and academic Psychology. A history of mutual influences.

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Psychoanalysis and Gestalt psychology

Psychoanalysis and Gestalt Psychology are related in many ways, a fact that has received only little attention up to now. Common influences on the development of Psychoanalysis and Gestalt Psychology came from the Helmholtz school, G. T. Fechner, F. Brentano and E. Mach. The founders of both disciplines, Sigmund Freud and Christian von Ehrenfels, had been friends for many years. However, since this relationship mainly developed due to Ehrenfels' interest in sexual-reform and not his Gestalt theory, the fact that this relationship was dicontinued by the Gestalt psychologists of the Berlin school (Köhler, Koffka, Wertheimer) did not imply a total break-up. Wertheimer himself, however, was reluctant to accept psychoanalysis, even though there had been many points of correspondence between him and Freud, which some of his disciples tried to impress him, in vain. Some of the colleagues and disciples of Wertheimer (K. Goldstein, K. Lewin, E. Levy, S.H. Foulkes) as well as some students of Freud (I. Herrmann, S. Bernfeld) founded a scarcely noticed but remarkable mutual relationship between psychoanalysis and Gestalt psychology which remains until today.

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Psychoanalyse und Gestaltpsychologie

Zwischen Psychoanalyse und Gestaltpsychologie bestehen zahlreiche sowohl historische als auch theoretische Beziehungen, die jedoch wenig bekannt sind und auch von beiden Schulen bisher wenig beachtet wurden. Gemeinsame Wurzeln lassen sich in der Helmholtz-Schule und in den Lehren von G.Th. Fechner und F. Brentano aufzeigen. Ein gemeinsamer Einfluß ging auch von E. Mach aus. Diese gemeinsamen Einflüsse auf Psychoanalyse und Gestaltpsychologie vermittelten sich primär über die Gründer der beiden Schulen, Sigmund Freud und Christian von Ehrenfels.

Sigmund Freud und Christian von Ehrenfels studierten beide bei Brentano in Wien und machten Wien auch zum gemeinsamen Geburtsort beider Schulen. Zwischen ihnen entwickelte sich ab 1903 eine persönliche Freundschaft, die in einem offenherzigen Briefwechsel dokumentiert ist. Da die Gestaltlehre in der Freundschaft zwischen Freud und Ehrenfels offenbar jedoch kein Thema war, ist es auch nicht unbedingt verwunderlich, daß diese frühe Verbindung zwischen den Gründern der beiden Schulen zunächst keine Fortsetzung erfuhr.

Im Anschluß an von Ehrenfels konzentriere ich mich auf die Berliner Schule der Gestaltpsychologie. Deren Begründer Max Wertheimer studierte bei Ehrenfels in Prag und nahm im Gegensatz zu diesem der Psychoanalyse gegenüber eine für ihn ungewöhnlich heftige und unaufgeschlossene Haltung ein. Obwohl er selbst in seiner Dissertation mit einem Wortassoziationsverfahren eine Methode entwickelte, die der freien Assoziation in der Psychoanalyse nahekommt, warf er der Psychoanalyse gerade auch wegen dieser Methode ein bruchstückartiges Menschenbild vor.

Unter Max Wertheimers Kollegen und Schülern aus dem Kreis der Berliner Schule waren allerdings nicht wenige, die die Psychoanalyse anders beurteilten und sich ihr mehr oder weniger öffneten. Kurt Koffka und Kurt Goldstein rezipierten die Psycho-

analyse deutlich wohlwollend. Kurt Goldstein trug sogar mit zum Aufbau des psychoanalytischen Instituts in Frankfurt bei.

Kurt Lewin war unter den bekannteren Gestaltpsychologen am stärksten an der Psychoanalyse interessiert, durch sie beeinflußt und hat umgekehrt auch am stärksten in diese hineingewirkt. So war er u.a. auch mit René Spitz befreundet, wie auch an den von ihm ab 1933 jährlich organisierten Treffen seiner sogenannten »Topology Group« häufig Erik H. Erikson teilnahm. 1937 begann Lewin mit seinen gruppendynamischen Experimentalstudien, die vor allem von der gruppenanalytischen Schule von S.H. Foulkes anerkennend rezipiert wurden. S.H. Foulkes war ein Schüler Goldsteins, der über diesen Wertheimer und die Gestaltpsychologie kennenlernte. Nach seiner psychoanalytischen Ausbildung in Wien und einer kurzen Tätigkeit am Frankfurter Psychoanalytischen Institut mußte er nach London emigrieren, wo er wenig später die Methode der Gruppenanalyse entwickelte. Zentrale theoretische Konzepte dieser Methode leitete er aus den ganzheitlichen Auffassungen der Gestaltpsychologie ab.

Am Frankfurter Psychoanalytischen Institut nahmen in den Jahren 1929 - 1932 mehrere Schüler und Assistenten Wertheimers an Seminaren teil. Wolfgang Hochheimer wurde später selbst wie Foulkes ein bedeutender und prominenter Psychoanalytiker. In seiner wichtigsten theoretischen Arbeit unternahm er eine gestalttheoretische Untersuchung der psychoanalytischen Methode, die aktuelle psychoanalytische Auffassungen insbesondere zur Behandlungstechnik vorwegnahm. Erwin Levy und Erika Oppenheimer-Fromm versuchten Wertheimer noch in Frankfurt und später in den USA davon zu überzeugen, daß zwischen Psychoanalyse und Gestaltpsychologie beträchtliche Gemeinsamkeiten bestünden. Wie Erika Oppenheimer-Fromm berichtete, wollte Wertheimer davon jedoch nichts hören.

Auch über Max Horkheimer war es damals an der Frankfurter Universität zu Verbindungen Psychoanalyse und Gestaltpsychologie gekommen. Max Horkheimer verwen-

dete sich 1928 für die Berufung Wertheimers an die Frankfurter Universität und ermöglichte nahezu gleichzeitig die Gründung des Frankfurter Psychoanalytischen Instituts im Institut für Sozialforschung. Er pflegte zu beiden Seiten enge wissenschaftliche und freundschaftliche Beziehungen. Auch im Wirken und Leben weiterer prominenter Intellektueller, die weder der einen noch der anderen Schule direkt angehörten, trafen Psychoanalyse und Gestaltpsychologie zusammen: Eng mit Max Wertheimer befreundet war auch Albert Einstein. Dieser stand gleichzeitig in Kontakt mit Sigmund Freud, mit dem er u.a. den berühmten Briefwechsel über die Frage *Warum Krieg?* führte. Jean Piaget kam als junger Wissenschaftler zu Auffassungen, die ihn, wie er später bekannte, zum Gestaltpsychologen gemacht hätten, wenn er die Arbeiten Wertheimers und Köhlers damals schon gekannt hätte. Gleichzeitig machte er eine psychoanalytische Ausbildung und wurde 1920 Mitglied der Schweizer Gesellschaft für Psychoanalyse. Schließlich ging er dann seinen ganz eigenen Weg, auf dem er jedoch seine frühen Verbindungen zu Psychoanalyse und Gestaltpsychologie in gleicher Weise aufrecht hielt.

In der psychoanalytischen Diskussion wurde die Gestaltpsychologie erstmals 1922 systematisch berücksichtigt. Imre Herman hielt auf dem damaligen Internationalen Kongreß für Psychoanalyse in Berlin einen Vortrag über *Die Neue Berliner Schule und die Psychoanalyse*. 1925 und 1927 rezensierte er in der Imago zwei wichtige Arbeiten Wertheimers. Einen systematischen und umfassenden Vergleich zwischen der Gestaltpsychologie und der Psychoanalyse legte 1934 erstmals Siegfried Bernfeld in der *Imago* vor. Er hatte während seines Aufenthaltes in Berlin Kontakt zur dortigen Berliner Schule aufgenommen und sich im Sommersemester 1932 an den Diskussionsabenden einer Arbeitsgemeinschaft von Psychoanalytikern und Gestaltpsychologen beteiligt. Bernfeld äußerte sich in seinem Aufsatz *Die Gestalttheorie* in Übereinstimmung mit Hermann sehr anerkennend über die Berliner Schule der Gestaltpsychologie und arbeitete zahlreiche Gemeinsamkeiten zwischen dieser und der Psychoanalyse heraus. Gemeinsamkeiten sah er gegeben in der gestaltlich-ganzheitli-

chen Auffassung des psychischen Geschehens, dem dynamischen Verständnis des Erkenntnisprozesses, der Bedeutung der Gestaltwahrnehmung für die infantile Entwicklung, der Verknüpfung affektiver und kognitiver Vorgänge im Konzept der Wesenseigenschaften und der theoretischen Verbindung psychischer und physischer Prozesse in psychophysiologischen Modellvorstellungen. Als unverwischbaren Unterschied stellte Bernfeld dagegen heraus, daß sich die Gestaltpsychologie nur auf das "Klare, Reine, Intellektuelle und Normgemäße" begrenze und deshalb nicht das "wirkliche, ganze Leben" erfasse. Insbesondere das Irrationale und Sexuelle würde die Gestaltpsychologie im Gegensatz zur Psychoanalyse ausklammern.

Bernfelds zentrale Hypothese war, daß die für die Psychoanalyse wesentlichen psychischen Zusammenhänge nach Gestaltkriterien organisiert seien. Diese Auffassung wurde später von Rapaport (1961) als Gestalt-Gesichtspunkt der psychoanalytischen Theorie bezeichnet. Bernfeld leitete diese Hypothese aus den Merkmalen des psychoanalytischen Erkenntnisprozesses ab. Die innere Dynamik der Aufdeckung unbewußter Zusammenhänge verglich er mit der inneren Dynamik der Gestalterfassung, wie sie von der Gestaltpsychologie an Denkprozessen beschrieben wurde. Für das "Finden" der Lösung sei ein merkwürdiges "Einschnappen" oder "Ineinanderkippen" charakteristisch, das Wertheimer mit dem Bühlerschen Ausdruck 'Aha-Prozeß" gekennzeichnet habe. Diese Kennzeichnung veranschauliche das "intensive Erlebnis, welches beim Zusammenschießen des diskret Nebeneinanderstehenden zu einem Sinnganzen vor sich geht und das nun auf einmal die mehrdeutigen Stücke verankert". Bernfeld sah darin ein wertvolles Beurteilungskriterium für Zusammenhangsfeststellungen, das über eine induktive Logik hinausreiche und das sich für eine wissenschaftstheoretische Untersuchung der psychoanalytischen Erkenntnismethode anbieten würde.

Gestaltpsychologische Überlegungen zum psychoanalytischen Schlußbildungs- und Deutungsprozeß bilden gleichsam den Faden, an dem sich die weitere Rezeption der

Gestaltpsychologie in der Psychoanalyse aufreiht. Devereux leitete in einer Arbeit aus dem Jahr 1951 aus der Gestaltpsychologie *Kriterien für die Wahl des Zeitpunkts von Konfrontationen und Deutungen* ab. Wie Bernfeld ging auch er davon aus, daß die von dem Analysanden vorgebrachten Materialelemente eine Gestalt bilden, die von einem sensiblen und aufmerksamen Analytiker allmählich erkannt werden. Aufgrund des von Lewin und Zeigarnik aufgezeigten Drangs zur Vollendung unabgeschlossener Aufgaben werden gerade diejenigen Konflikte immer wieder vorgebracht, die in der Vergangenheit nicht gelöst werden konnten und deswegen partiell ins Unbewußte abgedrängt wurden. Der Drang zur Schließung der Gestalt manifestiert sich häufig auch in Versuchen, den ungelösten Konflikt durch eine realitätsverzerrende Phantasie abzuschließen. Devereux bezeichnet diese meist unbewußten Phantasien als das »Schließungselement einer neurotischen Gestalt«. Sie werden meist irgendwann einmal mit der äußeren Realität in Konflikt treten oder auch als ich-fremd erlebt und drängen dann ebenfalls gestaltartig an die Oberfläche. Deutungen zielen auf die Aufdeckung der verdrängten Schließungselemente einer neurotischen Gestalt. Der Zeitpunkt für eine Deutung definiert nun Devereux dann als ideal, wenn das vom Analysanden produzierte Material eine eindeutige gestaltliche Prägnanz erreicht hat. Zuvor sollte sich der Analytiker auf Konfrontationen und Klarifikationen beschränken, die die noch "rudimentären Gestalten zur Entwicklung von Prägnanz stimulieren" sollten.

Rapaport leitete 1961 aus dem von ihm definierten Gestalt-Gesichtspunkt der psychoanalytischen Theorie die Empfehlung ab, jeden Aspekt des psychischen Geschehens ebenso wie dessen zeitlichen und räumlichen Kontext als integrales Ganzes zu behandeln. 10 Jahre später wurde diese Empfehlung durch Schumacher noch verfeinert. Um die gestaltliche Konfigurationen des Analysanden erfassen und sprachlich zusammenschließen zu können, stelle sich - so Schumacher - dem Psychoanalytiker die Aufgabe, sein eigenes Erlebnisfeld nach den Ordnungen und Zentrierungen des Patienten umzustrukturieren. Argelander übersetzte diese Empfehlungen 1979 in eine

von ihm als "Gestaltprinzip" bezeichnete Behandlungsregel: "Benutze alles Mitgeteilte zur Erkennung des unbewußten Themas. Dabei abwarten, bis die einzelnen Elemente in ihrer funktionalen Bedeutsamkeit aufeinander verweisen und sich daraus das unbewußte Thema als eine Gestalt abzeichnet, von der die einzelnen Elemente ihren Sinn erhalten." Dieser Behandlungsregel liegt eine zweiphasiges Modell des psychoanalytischen Erkenntnisprozesses zugrunde, das Argelander aus den vorausgehenden Überlegungen Bernfelds und Schumachers entwickelte: Das unbewußte Thema des vom Analysanden vorgebrachten Materials formiert sich zunächst nach den Regeln der Gestaltkohärenz im kognitiven System des Psychoanalytikers. Erst wenn die einzelnen Elemente durch ihre konfigurative Zusammengehörigkeit eine Gestalt erkennen lassen und damit das Thema bewußt oder vorbewußt im Psychoanalytiker Gestalt annnimmt, ist dieses in der zweiten Phase auch seiner Einfühlung und seinem logischen Denken zugänglich.

Die von Argelander empfohlene abwartende Haltung folgt der von Freud für den Psychoanalytiker aufgestellten Grundregel der "gleichschwebenden Aufmerksamkeit". Diese Grundregel übersetzte Freud einmal wie folgt: "Wenn Sie mit der Analysestunde beginnen, vergessen Sie alles, was Sie wissen, und tun Sie nur eines: so gut wie möglich zuhören". Eine verblüffend ähnliche Empfehlung gab Wertheimer (1957) für die Lösung komplexer Probleme. Er empfahl, beim denkerischen Problemlösen eher eine "selbstvergessene" als eine "angreifende" Haltung einzunehmen. In ihren jeweiligen Formulierungen produktiver Erkenntnisstrategien besteht die offenkundigste Übereinstimmung zwischen Freud und Wertheimer. Erst ihre Schüler konnten jedoch erkennen und nützen, daß sie in ihrem gemeinsamen Ziel, jene Vorgänge aufzuklären, die uns zu erweiterten Einsichten in unsere innere und äußere Welt verhelfen, einander näherkamen, als sie selbst einzusehen bereit waren.

POSTER SESSION

EBBINGHAUS: THE HIDDEN YEARS

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Traditional biographies (Boring, 1929, 1930; Jaensch, 1909; Shakov, 1930) provide little information about the introduction of Hermann Ebbinghaus (1850 - 1909) to the field of experimental psychology. Specifically, the years from about 1875 - 1879, immediately prior to his classic memory researches (Ebbinghaus, 1880), have been ignored. The present poster presentation will try to remedy the situation by providing archival and dcoumentary information about the life of Ebbinghaus in England, France and at the German Imperial Court. Unpublished materials from the Ebbinghaus papers at the Archive for the History of Modern Psychology at Passau University in Germany are our chief sources. Specifically, the following topics from the early life of Ebbinghaus will be covered in 30 DIN 4 panels:

1. England 1875 - 1876

a. The"Gentleman's Academy" in Sussex.

b. The November 1875, Hurricane.

c. Beginnings of memory research.

d. The Royal Grammar School in Guildford.

e. Byron's "Don Juan."

2. France 1877 - 1878

a. Paris.

b. German tutor in Burgundy.

c. A French Marquise.

d. Readings.

e. Studies of memory research.

3. At the German Imperial Court

a. A good friend.

b. Prince Waldemar of Prussia.

c. Psychological readings.

d. Memory research.

e. Unpaid and untenured instructor.

Finally, Ebbinghaus's foreign travels and the intellectual development will be discussewd within the context of the institutional development of 19th psychology.

POSTER

A CONTENT ANALYSIS OF PUBLISHED ARTICLES IN *PSYCHOLOGISCHE STUDIEN* JOURNAL

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SUMMARY

Our work analyses the journal ***PSYCHOLOGISCHE STUDIEN,*** essential for the development of german experimental psychology. This journal, edited by Wundt, represents the divulging organ of the works of the laboratory of experimental Psychology in Leipzig (Germany), and as such, a reliable exponent of the appearing scientific Psychology. We analized the content of all published articles in this journal since its begining till the retirement of Wundt, which allowed us the approach to the situation of the psychology at the end of the nineteenth century and the beginning of the twenteeth in Germany.

The aim of the first half paper had been the building of a taxonomy of the select categories in a general context witht the objective to know sub-topics worked by authors, the methods used and the apparatus did need them to study of this areas.

Thereby its obtein a bright image about the investigates areas and the processes realized.

Binet and Child sexual Abuse: Rousseau
as Subject of Investigation
Jacqueline L. Cunningham
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Binet and Child Sexual Abuse: Rousseau as Subject of Investigation

In 19th century France, as elsewhere, the preponderant psychiatric view on child sexual abuse blamed the children themselves for their victimization (Brouardel, 1909). Under the influence of Jean-Martin Charcot (1826-1893), French psychiatry was predisposed to attribute mental illness primarily to hereditary factors. Accordingly, a corollary of a focus on mental illness in cases of sexual offenses was a failure to ascribe particular etiological significance to abuse in childhood. Adult patients who reported having been victims of sexual abuse when young were believed to be suffering from a pathological suggestibility which resulted from their physiological weakness. By force of association, children involved in such offenses were considered to have a morbid heredity which distinguished them from normal children, who were not subject to such abuse and did not report on such crime (Garnier, 1896).

After 1888, Alfred Binet (1857-1911) saw the need to take a stand against a reductionaistic view of the hereditary basis of disorders of sexuality (Binet, 1888). For reasons which impelled him even more, he also saw the need to repudiate the belief that suggestibility was a pathological process and the product of morbid physiology (Binet, 1898, 1900). In support of these convictions, he looked to autobiographies of historical figures for credible accounts of sexual disorder suffered as a result of having been abused in childhood. A notable example

was that of Jean-Jacques Rousseau (1712-1778).

In a book entitled, "Etudes de psychologie experimentale: Le fetichisme dans l'amour" (Studies in experimental psychology: Fetishism in love), Binet (1888) wrote of Rousseau's sexual masochism which pervaded the course of his life. Rousseau believed the pathological shyness and celibacy of his young manhood had resulted from events which happened when he was eight years old.

Thus, by focusing on reports by esteemed persons of childhood sexual trauma, Binet upheld the credibility of testimony by the abused child. Moreover, by so doing, he narrowly missed claiming the primary etiological factors in disorders involving sexuality are the long-term effects of sexual experience in childhood (Cunningham, 1988).

Perhaps because the Freudian view on the seduction hypothesis obliterated competing perspectives for so long (Masson, 1984), Binet's contrasting position on the veracity of reports of child sexual abuse seems particularly poignant today. The issue will be discussed in a photo essay submitted as a poster presentation.