ESHHS

European Society for the History of the Human Sciences

18th Annual Conference



Florence 3rd-7th September 1999 Centro Studi CISL S. Domenico (Fiesole)

Università degli Studi di Firenze Dipartimento di Psicologia

Brunelleschi, Cupola, S. Maria del Fiore (1434)

Programme

FRIDAY, 3rd SEPTEMBER

15.00-19.00 Registration

19.30-20.30 Dinner

SATURDAY, 4th SEPTEMBER

09.00-09.15 Opening of Conference

09.15-10.45 Psychology, Physiology, and Society (I)

Herbert Fitzek (Cologne), Goethe and Freud: Two anniversaries and what they have in common

Irina Sirotkina (Moscow), Image of the future man in 19th psychology and physiology

James Good (Durham), William Stephenson and the bifurcation of British psychology

10.45-11.15 Coffee Break

11.15-12.15 Psychology, Physiology, and Society (II)

Roger Smith (Moscow), The scientist and the poet: Sherrington and the values of science in inter-war Britain

Ruth Benschop (Maastricht), The WSE Peep show: On the embedded nature of isolated visual perception

> 13.00-14.00 Lunch

14.15-15.45 The Dignity of Memory

Douwe Draaisma (Groningen), The splendour of the Bologna stone: Robert Hooke (1635-1703) on memory

Gesine Grossmann and Hans Pols (Berlin), Trauma and memory: Connections between psychology and history

Victoria J. Challiner, M.T. Brancaccio (Amsterdam) and L. J. Griffiths (Swansea), Theories that fail and therapies that work: Electroconvulsive therapy and the making of the psychiatric profession

16-18 Trip (facultative) to Florence: Places of Memory (Dante and Beatrice, S. Croce)

19.30-20.30 Dinner

SUNDAY, 5th SEPTEMBER

09.00-12.15 Symposium: Psychologization

Ruud Abma (Utrecht), Psychologization and psychology

Arjan de Boer (Leiden), Psychology and public policy: The effect of psychological research on Dutch traffic policies

Jaap van Ginneken (Amsterdam), Psychologizing social orientations: Changes in public life and the emergence of opinion and attitude research

> Jeroen Jansz (Leiden), Psychologization and the mission of psychology

> > 11.00-11.15 Coffee Break

Paul Voestermans (Nijmegen), Psychologizing culture: From Volksgeist to the patterning of behavior

Ido Weijers (Utrecht), Psychology and law

13.00-14.00 Lunch

Train from Florence 15.00 (Pisa: 16.06) Train from Pisa 19.16 (Florence: 20.14)

20.30

Dinner (Ristorante Quattro Leoni, Firenze)

MONDAY, 6th SEPTEMBER

Aula Magna of the University of Florence Piazza San Marco

10.00-10.45

Journals for the History of Psychology, Behavioral and Human Sciences, and Related Sciences (John Burnham, James Good, and Michael Sokal)

> 10.45-11.00 Coffee Break

11.00-12.00 Business Meeting

> 13.00-14.00 Lunch

14.15-15.45 History of Psychology: Challenges and Critical Issues (I)

Horst-Peter Brauns and S. Mayer (Berlin), Some remarks on the history of psychology

Trudy Dehue (Groningen), From deception-trials to control-reagents: The introduction of the human control group a century ago

Gabriele Chiari (Florence), Simonetta Gori-Savellini (Florence), Maria Laura Nuzzo (Roma), Francesco Velicogna (Florence), Hermeneutic constructivism: A paradigm for the psychology of the future?

> 15.45-16.00 Coffee Break

16.00-17.30 History of Psychology: Challenges and Critical Issues (II)

Ian Lubek (Guelph), Professional (in)visibility, gender and the writing of theory: Post-war trends and cases from social psychology

Pieter van J. Strien (Groningen), The prediction paradigm vs. the encounter paradigm: David van Leppen, a case in the americanization of personnel selection

Jacob van Belzen (Amsterdam), Psychology and personality as shaping factors in the history of psychology: On the non-involvement in psychology of religion by Dutch calvinists

> 17.30-19.30 Russian Psychology

Andrei V. Brushlinsky (Moscow), The subject-activity paradigm in psychological science

> Renate Topel (Magdeburg), On S. L. Rubinstein's philosophical-psychological concept

Vladimir M. Roussalov (Moscow), Human individuality: Recent developments of the Teplov-Nebylitsyn ideas

> Luciano Mecacci (Florence), Vygotsky and Eisenstein: psychology and film perception

> > 19.30-20.30 Dinner

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Session

"PSYCHOLOGY, PHYSIOLOGY and SOCIETY" Abstracts

R. Benschop, M. T. Brancaccio, V. Challiner, H. Fitzek, J. M. M. Good, L. J. Griffiths, L. Lieberman, I. Sirotkina, R. Smith

The WSE Peep Show: On the Embedded Nature of Isolated Visual Perception

Ruth Benshop University of Groningen, The Netherlands

Abstract

In 1969, some experiments were conducted producing what has become known as the Word Superiority Effect (WSE). This effect has baffled experimental psychologists ever since it was first summoned -properly -- in Reicher's experiments. Over the years, cognitive experimental psychologists have tried to get to grips with Reicher's counter-intuitive finding by performing a myriad of ever more sophisticated experiments. Questions about pattern recognition, (iconic) memory, attention, and reading have assembled around the effect. The cardinal debate in cognitive science between classic models of information processing and connectionist models erupted in attempts to explain the effect. The type of psychological research to which WSE experiments belong has always known vehement opposition. Critics argue that rather than producing valuable insights into the nature of cognitive activities, the

object under investigation is thoroughly distorted by the laboratory environment in which it is examined. However, rather than criticizing the WSE experiments for the way in which they negate the situated nature of visual perception, I prefer to use this criticism to examine the *kind of embeddedness* involved in these experiments which aim precisely at *isolating* visual perception from the world which

it normally inhabits. Thus, using the critics' objections as a set-off point, I will examine the WSE experiments to investigate what kind of object is investigated in WSE research, and how this object is achieved within these experiments. In conclusion, I will return to the critics of experimental research to see what my empirical explorations have yielded.

There have always been critics of lab research in experimental psychology. I want to focus on one recent and very interesting critic of cognitive psychology: Edwin Hutchins. Edwin Hutchins' *Cognition in the Wild* (1995) is a beautiful book in which Hutchins convincingly shows the materially and socially distributed nature of human cognition. His anthropological fieldwork on a big naval ship shows how the cognitive activities involved in navigating the ship are produced by the working together of different media, such as individuals, groups, instruments, maps etc. etc. However, I want to focus not on his magnificent and detailed examination of what he calls <u>cognition in the wild</u>, but on what his counterpoint is, to which he contrasts cognition in the wild: cognition in the lab, or experimental (cognitive) psychology. He writes:

I have in mind the distinction between the laboratory, where cognition is studied in captivity,

and the everyday world, where human cognition adapts to its natural surroundings.' (xiv) Cognition in the lab, according to Hutchins and other critics has a number of rather unpleasant features. Rather than producing valuable and generalizable insights into the nature of our cognitive activities, the object under investigation is thoroughly distorted by the laboratory environment in which it is examined. These artificial circumstances produce knowledge about a destitute, captive, isolated and unhooked object. Cognition in the lab is:

- reduced

- isolated

- artificial

- non-material

- unnatural

- and therefore particularly uninformative about cognition as is occurs naturally.

In short, you get the distinct impression that if you were cognition, you would not want to live in the lab. So, I decided to go and visit cognition in the lab to see just how bad it's living-conditions were. I decided to use his analysis of Cognition in the wild to examine cognition in the lab.

WSE experiments

In fact, I did historical research of a series of experiments on the Word Superiority Effect (WSE) performed from 1969 onwards. I won't go into the nitty gritty of these early cognitive psychological experiments, but will try to give you some sense of what they are all about. The WSE intrigued a generation of experimentalists for its ability to defy early cognitive psychology's very core. In this effect, visual perception of letters mingles unconcernedly with more complex and meaningful characteristics of words. However, according to early cognitive psychology, these two *cannot* mix: visual perception comes *before* and is *a requirement of* the following more complex interpretation stages of information processing. Thus, accepting the WSE meant rejecting the budding cognitive psychological perspective. On the other hand, being able to reject the WSE as an artefact, and thereby rendering it harmless, would save the project of cognitive psychology. It should be clear, the stakes are high in the experimental research of the WSE.

I will first say something about the theoretical stakes in these experiments. Then I will give you an idea of the experimental procedure. And thirdly I will tell you a bit about one of the instruments used in the research, the tachistoscope.

What is the WSE? - Theoretical Concerns

I have too little space here to explain what the WSE is, suffice it to say that in these experiments the purpose is to get at visual perception. And, visual perception has to be distinguished from higher cognitive processes which occur *after* initial perceptual processes and, when given a chance, *contaminate* those initial stages. So, in these experiments, it was attempted to exclude the influence of interpretation or knowledge to get at a <u>perceptual</u> effect. In these experiments, perception appears as something rather difficult to get at. As something that has to be protected, and something that appears before and in front of those higher processes. Only when treated well, can one obtain insight into visual perception. In this protection of visual perception from what comes after, a differentiation belonging to the classic stage model of cognitive processing can be recognized. This classic stage model has a modular structure. Visual perception is defined relative to higher cognitive processing and not in terms

of the (physical, social, cultural) environment in which visual perception takes place. The depictions of the modular structure of information processing reveal this in the way they represent the external environment. It is usually represented by a single arrow called `input'. The input is in no way differentiated. It is located at the edges of the figure and functions as such. What happens with the input *inside* is important. Where `inside' is located in the world, or how the `transition' is made from the world outside to that inside, is not considered interesting. The task facing cognitive psychology is to trace the fate of the input *inwards* into the mind.

So, reminiscent of Hutchins' characterization of cognition in the lab, the theoretical aim in the WSE experiments is to get at pure perception. To do this, perception has to be differentiated from higher cognitive processes, it has to be treated well and to be protected. And, to be able to focus on what happens on the inside, rather than the outside in a WSE experiment, the outside -- the experimental situation, the apparatus -- is carefully arranged.

Experimental Procedure - The Practice Session

Before a WSE experiment can begin, a lot has happened. The experiment has been designed and set up. Pilots, runthroughs and checks have been done; introductory texts and procedure manuals have been written; schedules have been made and a budget has been worked out; subjects have been recruited and appointments made.

The trial begins with a practice session. During this training period, the experimental subject is familiarized with the procedure of the experiment. In the course of the practice period, the subject becomes acquainted with the stimuli and learns how to respond to them in the way he is supposed to. He also learns how to conduct himself during experimentation. This is important, because the conduct that is allowed during experimentation is specific and limited. So, during the practice period, first of all, a lot is excluded. The behaviour a subject might engage in is divided into acceptable behaviour and unacceptable behaviour. These divisions are constructed in several ways. The experimenter literally instructs the subject. The computer running the practice session sets out a particular course in which the subject is required to act in certain intervals.

The adjustment achieved during the practice period, *sets* the actions of the subject, the instrument as well as the experimenter. Thus, during the practice trials, the rhythm, the course and the moral code of the experiment are communicated to and practised by the subject.

During experimentation proper, experimenter and apparatus continue to monitor the subject's performance. The pace set during the practice period must be maintained during experimentation. This pace must not only be kept up by the subject, but also by the experimenter monitoring the subject and the instruments, and by the instruments displaying stimuli and recording responses right on time. To make sure that all elements in the experiment continue to perform correctly, computer and experimenter must remain diligently alert to keep the subject from straying.

Experimenter, instruments and subject are all attuned to one another by the preparation and practice before experimentation. During the experimental trials, the experimenter, instruments and subject move through their respective paces. Every element supports the delimitation of the subject's responses. Any breach in this delimitation, any diversion is monitored and brought back in line, or as an ultimate measure, leads to the exclusion of the results. Thus, during the unfolding of the experiment, all elements work together to elicit the correct behaviour from the experimental subject. Several possibilities are eliminated beforehand in the practice period, by the instructions of the experimenter and through the organization and order of the experiment. In addition, the subject who does not succeed in responding within the specified parameters, can always be remonstrated or removed from the analysis during experimentation or upon examining the results during the interview afterwards.

In the theoretical reasoning about the WSE, an object of research was defined: pure visual perception. To get at visual perception, it had to be separated from the influences of higher cognitive processes. In what happens during the experiment, the *range* and rhythm of the subject's possible actions is severely limited. All the elements in the experiment, as well as the course they follow, serve to elicit particular behaviour, at particular moments within the experimental trials, for as many times as the experiment requires. Without the format and cadence of the experiment, the experimenter's firm instructions and continued monitoring, the apparatus' alertness and production of data, such behaviour would not occur. Thus, it may be possible to get at pure perception. And, as Hutchins' description of cognition in the lab

suggests, reduction occurs. But it is a very specific kind of reduction, achieved by hard work, practice and coordination of all elements in the experimental situation. Only in this mutually attuned situation, can the proper behaviour of the experimental subject emerge.

The Tachistoscope

In these experiments (as in many many others) an instrument is used called a tachistoscope. The tachistoscope presents visual stimuli very briefly and at exactly the same point every time. The particular type of apparatus used in these experiments has the shape of a box.

On one side of the box an opening is made for the subject to look into. In two of the other sides, openings are made in front of which cards can be attached with stimulus-material. The inside in painted black to exclude outside light. During the experiment, the subject puts his faces against a face-piece attached at a side of the box. Inside the box, baffles are placed in between the mask and the exposure-fields which help to concentrate and guide the subject's vision. The eye is guided by the baffles and they make sure that over different trials and varying stimuli, the position of the stimuli will always be the same, the eye will encounter the stimuli in the same manner every time.

The instrument is aimed at producing an unchanging setting in which distances are set, eyes are focused, and vision is guided. An environment is arranged in which only eyes can function meaningfully. The face-piece attached to tachistoscope literally narrows the experimental subject down. What is left is eyes on one end, and a stimulus-field on the other. Any other ways the subject might want to deal with the task set before him, are set aside. He may not use his hands, he may not remember, or think, he may not make notes, use tools or books.

The subject's seeing eyes, moreover are immobilized. They do not move and have no time to blink. Visual perception becomes a single, fixed perspective upon the world. With its face-piece, baffles and box-like shape, the tachistoscope creates a distance between what is to be perceived (stimuli on one side of the box) and the perceiver (on the other side of the box). By this construction, the perceiver is removed from the world which is to be perceived, represented by the stimulus-cards. The subject's perspective upon the world, is itself no longer part of the world. For that matter, the tachistoscope *itself* is no longer really part of the world. The environment which is arranged especially so that these stimuli may be presented in a particular way, is itself not part of that which is to be seen by the subject's eyes. The situation in which the subject is glued to the black box, focused on perceiving the stings of letters facing him from the opposite end of a tunnel, brings Hutchins' description of cognition in the lab to mind: A mind inside, the world outside and a bridgeable gap in between. The tachistoscope is that gap. So, again, pure perception may be obtained. It may be obtained by protecting visual perception from higher cognitive processes. It may be obtained by the practice and attunement of experimenter, experimental subject and instruments. And, it may be obtained by using the tachistoscope, to exclude all but the subjects eyes and to help him see in the necessary way: in box-like fashion.

Conclusions

My concern here has been with Hutchins characterization of cognition of the lab as something radically different, radically impoverished and rather undesirable. Does cognition in the lab resemble this characterization? Three observations:

First, the quick answer is: yes it does. Hutchins is right, unlike the creative, rich materially and socially distributed cognition in the wild as Hutchins found it on the ship he studied, cognition in the lab is a decidedly odd thing.

What I have explored, is *how it is*, that Hutchins is right. How is such a sorry cognitive state achieved? With a few empirical examples, I have tried to suggest that in order to produce such a specific state of visual perception, close and very specific ties are developed between visual perception and the theoretical, experimental, and instrumental setting in which it emerges.

Hutchins - and others - criticize lab-research for locking up cognition and thus ending up with a distorted image of how cognition works. It is interesting to see that the efforts to achieve such a reduced cognition, are themselves all but impoverished. The impoverishment the lab induces, is achieved in exceptionally rich, and creative ways. Visual perception may be reduced, but in achieving that reduction, the theorizing about the effect, the designing and running of the experiment, and the constructing and disciplining of apparatus and subject prosper. To reduce visual perception, the tasks

of experimenter, theory, experiment design, apparatus construction are challenged and enriched. So, cognition in the lab may be reduced, but saying that misses the rich ways this reduction is achieved and the intricate connections between the cognition that is isolated and the setting in which this isolated cognition is embedded.

Romantic Residue

The phrase "cognition in the wild" refers to human cognition in its natural habitat -- that is, to naturally occurring culturally constituted human activity. I do not intend "cognition in the wild" to // be read as similar to Lévy-Strauss's "pensée sauvage," nor do I intend it to contrast with Jack Goody's (1977) notion of the domesticated mind. Instead, I have in mind the distinction between the laboratory, where cognition is studied in captivity, and the everyday world, where human cognition adapts to its natural surroundings. I hope to evoke with this metaphor a sense of an ecology of thinking in which human cognition interacts with an environment rich in organizing recourses.' (xiii-xiv)

Second, a romantic residue seems to linger in Hutchins' discussion of the wild. Cognition in the wild, it seems, is free to adapt to the requirements of everyday life. And, Hutchins suggests, such free adaptation to one's habitat, a rich context, is the way cognition *normally* behaves in the everyday world. It may be questioned, however, whether cognition in the everyday world resembles Hutchins image of cognition in the wild. The dim light of the labroom, the dullness, irrelevance, restrictiveness and meaninglessness of the experimental task, the concentration and boredom of the experimental subject, the monotony and repetitiveness of the work the experimental subject is faced with in the cognitive psychology lab may in fact resemble many a natural habitat in the everyday world (for example, the factory).

And finally, Hutchins compares cognition in the wild with cognition in the lab. But cognition in the lab, in his rendition, is also cognition in the making. Cognition in the lab, on the other hand is described as ready-made cognition. The reduced, unnatural, poor cognition described by Hutchins is an object resulting from experimental research. Thus, it is not surprising that cognition in the wild differs greatly from cognition in the lab. I am not contesting that they are different (although that remains an empirical question). I am suggesting that the difference between the two is a difference between the impoverished quality of after the fact ready-made cognition and the richness of cognition in the making. The cognitive psychologist's lab cannot be excluded a priori as a site for research, even if the *effect* of cognition in the lab compares to cognition in the wild, cognition in the lab should also be studied in the making, as I have tried to do here. To be sure, cognition in the lab may then turn out to be quite different from what Hutchins calls cognition in the wild, but surely no less wild.

Reference: E. Hutchins Cognition in The Wild, Cambridge, Massachusetts; MIT Press, 1995.

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Theories that fail and therapies that work: ECT and the making of the psychiatric profession

V. J. Challiner

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<u>Abstract</u>

The history of psychiatry may be represented as a search for therapies that are effective against mental illness, supported by theories which explain these effects. Although this characterisation of psychiatry may be said to be representative of the practice at some points during its history - most notably in the development of treatment for neurosyphilis during the 1939s and 40s - for most of its history psychiatry might more accurately be described as a profession employing therapies that are to some extent successful for a period of time with a sample of patients. Most therapies have failed to achieve cures for all patients sharing the same diagnosis, and a central frustration of many psychiatrists has been linked to an inability to develop truly effective treatments which can be reliably linked to a theory that adequately explains the treatment's effects. ECT provides a classic example of a treatment that is claimed to be remarkably successful for some patients, but may have little impact - or even cause damage - to others. This controversial treatment, however, is one that has been effective enough to survive not only the pharmaceutical revolution, but also intense criticism from both within and outwith the profession of psychiatry. In present day psychiatric practice, ECT remains the treatment of choice in cases where severe depression appears to resist pharmacological intervention. It is clear that this treatment is not well theorised but is considered effective enough to have survived intense criticism and still be in use more than sixty years after its invention. There are few other medical specialities where such a controversial and under-researched treatment may be retained, and the investigation of the role of ECT in the development of modern psychiatry is clearly a research priority.

Paper

Discovering the causes of mental illness and finding effective therapies has been, and still is, both the key aim and the greatest challenge for mental health professionals. The history of psychiatry, therefore, may be represented as a search for therapies that are effective in combating mental illness, supported by theories which explain therapeutic effects.

Miles (1987) documents the history of mental illness in Western society as evolving from the belief that the mentally ill were witches or possessed by the devil, whose afflictions could be cured by the exorcism of evil spirits by priests, to the contemporary view of mental illness as being similar to that of physical illness, and thus treatable by doctors. The consequence of this conceptual shift has been an increase in medical intervention in many conditions.

Since its emergence, psychiatry has been torn between two opposing visions of mental illness: one postulating that the cause of mental disturbance is some kind of brain disease, the other assuming a psychological cause of mental disorder. In the course of time, these competing interpretations have alternately assumed a central position among psychiatrists. Over the last thirty years, for example, there has been a shift in thinking about the causes of mental illness. Whereas experience and environment were long thought to play a major role in most mental problems, it is now commonly assumed that brain chemistry has a dominant role in the aetiology of such kinds of disturbance.

Different theories as to the cause of mental illness have obviously informed different therapeutic interventions, some of which have proven ultimately ineffective or even dangerous and have been sooner or later abandoned. ECT provided a classic example of a treatment that is claimed to be remarkably successful for some patients but may have little impact or even cause damage to others. This controversial treatment, however, is one that has been effective enough to survive not only the pharmacological revolution but also intense criticism from both within and outwith the profession of psychiatry.

ECT emerged as a treatment for mental illness at a time when dissatisfaction with the curative and therapeutic effects of the asylum were being voiced (Miller, 1986). At a time when asylum psychiatrists were becoming increasingly wearied by the lack of research opportunity and the relative therapeutic powerlessness that their role entailed, their colleagues working in university clinics and research centres were engaged in an intense period of research and experimentation aimed at finding the causes of, and cures for, mental illness. The expansion of information and communication systems contributed to a greater individual mobility of young doctors of varying nationality, who covered wide geographical distances in order to acquire their specialisation in prestigious and scientifically advanced European Universities. The progressive internationalisation of professional circles led to a more global dissemination of new ideas. In this lively intellectual environment, physicians - either individually or in groups - began to experiment with new methods for the treatment of mental illness.

One of the early attempts took place in the 1920s and was named the "sleep cure" (as it consisted of inducing prolonged sleep in manic and psychotic patients). Its

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diffusion was limited, however, as it proved of doubtful therapeutic efficacy and of high risk to patients. Other therapies (insulin-coma therapy and ECT) introduced in roughly the same period were seen - in retrospect - to be the great breakthrough in the treatment of mental illness and rapidly spread into mental hospitals.

Convulsive Therapy was introduced to the field of psychiatry by Dr. Von Meduna in 1933. It arose from his observations that none of his schizophrenic patients was ever epileptic, while none of his epileptic patients was ever schizophrenic, which consequently led him to chemically induce convulsions in his schizophrenic patients. The chemical treatment was already losing credibility when experiments were conducted with electrically induced fits, the rationale being that the production of fits was beneficial in the treatment of schizophrenia (Packham, 1984). In 1938, Cerletti, Professor of Neuropathology and Psychiatry in Rome, introduced electric currents to induce seizures in psychotic patients (Fink, 1979). The history of the first ECT treatment has been documented by Impastado (1960), who describes the patient as a 39 year old man with catatonic schizophrenia who had been found wandering the streets and who could only speak "an incomprehensible gibberish". A low dosage of electricity was administered, which resulted in a petit mal seizure and caused the patient to burst into song. Cerletti, encouraged by the results, suggested another treatment at a higher voltage, but his assistants objected. Despite this, Cerletti continued. The patient received a total of eleven treatments, responded well, and one year later was working regularly in his old job (Fink, 1979). ECT was thus born out of one man and over the objections of his assistants, and after a short experimental phase that seemed to confirm the efficacy of this new treatment, ECT was introduced in rapid succession in France, England and the United States. By 1959, ECT had become the "treatment of choice" for depressive illnesses, and since its development as a modern psychiatric procedure, millions of people worldwide have now received ECT as a treatment for mental illness.

Finding a treatment that seemed to cure, or at least provide relief for, major psychiatric illnesses was an important step and a powerful source of professional legitimisation for psychiatrists. At the theoretical level, and for the first time in decades, psychiatrists could claim some - albeit, retrospectively, erroneous - understanding of the relations between mental illness and organic processes, while at the same time having the resources to provide treatment. The acclaimed success of ECT as a therapy for the treatment of mental illness was, however, relatively short-lived. In the 1960s and 70s, when the dominant theoretical trend in psychiatry was oriented toward a psychological interpretation of mental disturbance, accompanied by the development of effective psychotropic drugs, physical interventions such as ECT were criticised for their intrusive nature, their damaging effects, the lack of scientific legitimation, and unproven efficacy.

It is now over sixty years since Cerletti first administered electric shock to the brain of a mentally disturbed patient, and the treatment - which appeared to be controversial right from its onset - is still a highly contentious issue today. Perhaps one of the reasons why ECT is such an emotive issue is because of the persisting images of its barbaric administration, a legacy left by early treatment. Early ECT was administered while the patient was fully conscious, and it was not uncommon for patients to experience fractured bones, lost teeth, and numerous other unpleasant effects (Packham, 1984). Although modern day ECT limits these effects owing to the introduction in the late 1950s of anaesthetic and muscle relaxants to the procedure, such images have been compounded and sustained, not least because of their

endorsement in films such as One Flew Over the Cuckoo's Nest and An Angel At My Table.

Aside from debates concerning the humanity of such a treatment, there still rages acrimonious dispute over whether ECT actually works or not (Clare, 1980). Much of the conflict in this area has arisen due to the inability of scientists to satisfactorily explain how any clinical improvement is brought about by ECT (Packham, 1984). The reality of the situation is that no one really knows why or how ECT works, or what it does to the brain (Boodman, 1996). This is not to say, however, that ECT has been persistently used in the practice of psychiatry without any attempt to discover its theoretical basis. Indeed, over the years a number of theories have been postulated as explanations of how ECT works. For example, biologically orientated explanations of ECT range from explanations concerning the control and modification of the neurotransmitter noradrenaline (Clare, 1980) to the claim that ECT temporarily lifts depression by causing transient personality changes similar to those seen in patients with head injuries - namely confusion and memory loss (Boodman, 1996). Psychoanalytic theory propounds that ECT has its effect by assisting the process of regression (Miller, 1968). In other words, ECT helps the patient to bury the original reasons for the depression, which is what psychotherapy seeks to uncover.

During the 1960s and 70s, when the psychological approach to the understanding and treatment of mental illness began to take favour, the appeal of ECT began to wane and psychiatric institutions bore witness to a decline in its use. Concomitantly, pressure groups campaigning for the abolition of ECT and for the rights of mental patients began to form on both sides of the Atlantic. Although unsuccessful in securing a total ban on the use of ECT, in most countries there now exist strict regulations governing the conditions and modalities of its administration. Curiously,

despite such fervent opposition to the use of ECT in the treatment of mental illness, the therapy is still used throughout Europe and the United States today, and furthermore it seems to be enjoying a resurgence in popularity, with the critical front of the past years seemingly extinguished.

ECT is a therapy that seemingly does not work for all patients exhibiting similar psychiatric symptoms, yet it is still accepted as a legitimate treatment where others with equally variable success rates have been rejected. There are few other medical fields in which such a controversial and somewhat mysterious treatment would be tolerated, and so the role of ECT in the development of modern psychiatry is clearly a research priority. ECT is a rich field for historical and sociological investigation. It warrants reflection on how interpretative frameworks about mental illness have gained authority in our culture, and how a treatment that has sustained such overt, intense criticism can withstand such an attack and still be widely accepted as a treatment of choice for so many patients.

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بقبر

Goethe and Freud: Two anniversaries and what they have in common

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In August and September the scientific community is aware of two anniversaries: Goethe's 250th birthday and the completion of Freud's "interpretation of dreams". Watching these two events, a profound connection can be revealed in the scientific works of Goethe and Freud. They both traced out the plan of a method, according to the nature of scientific subjects. My paper will follow the consequences of this definition and show what the identity of subject and reconstruction implies in the realm of human sciences.

Not only in Germany the year 1999 has been bringing about a new Goethe renaissance. On August 28th we celebrated Goethe's 250th birthday, but Goethe celebrations are held all over the year. Goethe the poet, the prophet, the statesman, the Don Juan and nevertheless Goethe the scientist and philosopher are only some of the headlines of Goethe events. In these days we are approaching a further commemoration day. Exactly a hundred years ago Freud wrote the last pages of his "interpretation of dreams" which was to appear in october, dated in advance on the year 1900.

In my paper I will try to reveal a connection between these two dates. In avoiding superficial arrangements due to celebration dates, I will try to discover a real und almost overlooked analogy of Goethe's and Freud's scientific thinking. Dealing with the connections of a poet's and a psychoanalyst's scientific experience I have to be aware of some traditional reservations coming from the history of academic psychology. In the course of getting autonomous psychology has overthrown many of her ties to philosophy and metaphysics. It has been constitutional for modern psychology to strip off the clothes of romanticism and to dress with the principles of new and dominating scientific disciplines like physiology thus moving forward to a decisive position of mechanism and elementarism. At the climax of this revolutionary turn Emil DuBois-Reymond held his famous Berlin rectorate speech on "Goethe and no end" trying to expel romantic influences which still seemed to burden the scientific spirit during the last decade of the 19th century (DuBois-Reymond 1882).

Celebrating Goethe as an early representative or a pioneer of psychological experience is therefore rather unusual. In the case of Sigmund Freud however, things are even worse. Nobody really denies Freud's contribution to psychology. But the assessment of the quality of this contribution is very ambiguous. Like his fellows in academic psychology Freud had devoted himself to the soon spreading natural science. Like them he had been instructed by the capacities of philosophy and physiology (especially Brentano and Brücke). But apart from them exact and experimental work could not maintain what originally had aroused his fascination in natural science. That was, in the words of the well-known philosopher Eduard v. Hartmann, the promise of "rebuilding the plan of nature" (Hartmann 1901).

This notion did not only lead Freud to the study of nature. It turned out to be source of his lifelong search (and research) for the principles of nature and at last for his change from natural to human sciences: "As a young man I have only been longing for philosophical knowledge, und now, by leaving medicine towards psychology, I am going to fulfil this notion." In a letter to his friend Wilhelm Fliess Freud once reveals what had been the initial motivation of giving up good prospects on a career as a lawyer and turning to the wide field of natural studies. It was in 1873, just before leaving school, when the 17 year old Freud

attended a lesson about Goethe's fragment "Nature". This public lecture held by the morphologist Carl Bernhard Brühl was the starting-point of his turn towards nature and - after decades - the fundament of his work on reconstruction of unconscious processes in psychological experience (Hemecker 1991).

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In what degree Goethe's prospect of nature impressed Freud's work on the whole is open to question. But Goethe's outstanding position within the psychoanalytic pedigree cannot be doubted. As already noticed this close connection gave rise to some criticism from the modern academic psychology. Unregarded the indecent contents of his psychology, they discredit Freud's psychology as speculative or holistic (O. Marquard gives a more distinguished report on that; Marquard 1987). Under the spellbound of the Goethe year it may be allowed to suspend this critical attitude. On the contrary we may assume here that the reference to Goethe as a scientist is honourable and even of some promise.

There is no time nor space here to report Goethe's complete scientific concept. So I will concentrate on one crucial experience in Goethe's morphology which is to be traced afterwards in Freud's psychonanalysis. Sketching his morphology as a whole Goethe stresses that morphology does not introduce new subjects, but a new kind of treatment of the subjects familiar to science. Morphology is defined by handling organisms according to their own appearance.

Studying his plans for a morphology of nature we find Goethe repeating this principle in various formulations and modifications. Morphological work does not only refer to natural organizations. It is characterized by the qualities of natural organizations as well: as an interplay of particular figurations (in German: "durch die Bildung und Umbildung von Gestalten"). Adjusting his methods to the "methods" of nature Goethe defined morphology as a scientific tool developing patterns out of patterns. With the scientific practices of contemporary scientists, with their cutting up and taking apart natural organizations Goethe could not agree.

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Turning back to Sigmund Freud we find him in a similar (contra)position to the powerful natural sciences of his epoch. But it was a different epoch in history, and Freud worked on different subjects - not on the organism of living beings, but on feelings and behavior of human beings. Tracing morphological thinking in psychology, we have to ask in what respect natural versatility can be noticed in Freud's work. In fact, Freud began his academic career as a physiologist and zoologist, soon being startled by the antinomy of scientific methods on the one hand and holistic frame theories on the other. The promise of rebuilding the plan of nature could not be maintained while dissecting corpses. It took the acquaintance with Charcot to rediscover his ideal in concrete scientific work.

More and more Freud was attracted by psychological points of view. But in the 1870s and 1880s experimental scientific work was prevailing, like in other scientific disciplines. So Freud decided to search his own psychology. Left to his own resources, he began to use psychological experience in a totally different way as contemporary psychology. Especially the dramatical reports of his patients leaded him to a totally different theory and practice of

psychology. Attending their stories he practiced what Goethe had called "listening to the revelations of nature".

Freud's technique wasn't a simple gathering of phenomena. It justly can be called a sort of experimental psychology (Fitzek 1999). But his experimental design did not exclude the variety of experience. He did just the opposite. When Freud used experiment he tried to widen the realm of experience including and focussing phenomena which were traditionally taken as less considerable, like dreams, mistakes and symptoms. In so far the analogy of science according nature in the work of Goethe and Freud is not superficial, but the basis of a corresponding scientific paradigm.

It is not difficult to discover Goethe's programme in the Sigmund Freud methodological approach. In tracing the origins of symptoms, generating faulty acts out of complex determinations, and following the metamorphosis of dreams, Freud adopted Goethe's method of reconstruction according to nature, as illustrated by J.J. Grandville in his transformations of dream images.

"Rebuilding the plan of nature" is not only the intention of Goethe's morphology but also transferred to psychology - the basis of psychoanalytic method. That means at first: trying to approach the appearance of nature as near as possible. But there is another consequence of Goethe's and Freud's common points of view, a consequence more profound and more difficult. If the identification of nature and reconstruction is not only a metapher, but an essential of morphological work, this work does not only profit from the reference to its subjects, but is at the same time handicaped by it. Scientific method is then actually "subjected" to experience.

If it is true that scientific methods continue the modes and methods of nature, psychology must take into account all kinds of human weakness in methodological approach. In consequence the mechanism of Traumarbeit and of defence must be found in scientific reconstructions as well. In his work on the artist and scientist Leonardo da Vinci Freud traced scientific work back to human characteristics, like the "thirst for knowledge", "scopophilia" and curiosity for sexual life. From a morphological point of view these characteristics matter in science and the work of scientists on the whole.

In morphology the correspondence and even mingling of scientific subject and subjective science is constitutional and not to be avoided. The ethnopsychoanalyst George Devereux has reported about that in his ingenious work "From Anxiety to Method in Behavioral Science" (1967), revealing the close contact of subject and reconstruction in the behavioral sciences. The scientist is subject (material) and analyst of human experience at the same time. According to that, anxiety and frustration - in the Freudian sense of unappeased wishes - cannot be excluded from scientific work.

Devereux shows that this difficulty is not to be cleared away. But it can be covered or evaded. Yet it is not made uneffective, it will only get unconscious and more dangerous. This may be the reason why Freud's and Devereux' argumentation, though basically accepted, is hardly taken into account in practical psychological work. Scientists still avoid reflecting their own wishes and fears, their longings and taboos.

Goethe and Freud can be seen as witnesses of a self-reflective concept of science. They were outsiders, but especially for the human science they should not be forgotten. In Germany their

paradigm of a self-reflective science is increasing. Especially "qualitative" psychology (Breuer 1996) refers to the context of psychological discoveries objecting to the traditional "ideal of immunity of interference ("Ideal der Störungsfreiheit" according to Mruck & Mey 1996). At the university of Cologne we have had a sequence of lectures on psychological research stories ("Forschungsgeschichten") dealing with our personal experience in psychological work. This experience has been an exciting impulse of getting aware of the way we scientists are handled when we try to handle our subjects. Breuer, F. (ed.)(1996): Qualitative Psychologie. Opladen

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William Stephenson and the Bifurcation of British Psychology

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In this paper I focus on a significant period in the development of British Psychology - roughly 1929-1948. The paper concerns the work of the physicist/psychologist William Stephenson, best known in the UK for his development of Q-Methodology. My intention is to show that by the time Stephenson left the United Kingdom in 1948 to take up an appointment at the Department of Psychology, University of Chicago, the direction of British Psychology had taken a decisive turn, a change of direction that in part can be attributed to some of the consequences of Stephenson's failure to secure the Chair of Psychology at the University of Oxford where he had been Director of the Institute of Experimental Psychology and Reader in Experimental Psychology. With the departure of Stephenson for the United States, British Psychology lost one the leading figures in the study of Individual Differences. Arguably Stephenson can be seen as the natural successor to Spearman and Burt for both of whom he had worked as a Research Assistant. Despite strenuous attempts by Hans Eysenck and some of his colleagues to undermine the schism, with the setting up of the Experimental Psychology Group in 1946 (later to become the Experimental Psycholgov Society - the EPS) a bifurcation was reinforced between experimental approaches to psychology and the individual differences tradition, а division that was later to be elaborated by Lee Cronbach in his 1957 APA Presidential Address.

How "caucasoids" got such big crania and why they shrank: From Morton to Rushton

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In the 19th Century,

S.G. Morton reported cranial measurements for [Caucasians] larger than among [Mongolians] and [American Indians,] followed by Negroes. In the 20th century a nonhierarchical view emerged under the leadership of anthropologists. Starting in the 1980s, J.P. Rushton turned Mortonlls conclusion topsy turvy by reporting that [Mongoloids] have larger cranial capacity, and linking cranial size to intelligence, as measured on IQ tests. Variations in intelligence are then used by Rushton to explain [lrace] differences in behavior (illegitimacy, crime, social class, intercourse frequency, divorce, etc, ect, ect.). This paper discusses the social context in which the turn around in the hierarchy occurred and critiques the "scientific" basis of the new hierarchy. The analysis addresses the use by Rushton of the invalid concept of race, the numerous variables affecting brain measurements, the comparative meaning of brain size differences, and Rushtonlls evolutionary scenario, and his connection to the pioneer fund, Rushton Ds five percent explanation, and sociocultural influences. Conclusion: the question of which BraceB has the largest cranial size and intelligence is devoid of conceptual and empirical merit and encourages racism. The function of cranioracial hierarchies is not to identify who is number one, but rather to biologically justify the placement of persons of "Negroid" ancestry at the bottom of the hierarchy and to provide false testimony aimed at justifying policies and practices of exploitation and neglect.

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In the late eighteenth and early nineteenth centuries, the French *philosophes* and the German Romantics laid the foundation for two competing visions of a man of the future. These visions were to feed psychological and physiological conceptions and guide the eugenic search for improvement of the human type. The *philosophes* and the Romantics shared a belief in the perfectibility of human nature and projected into the distance their ideals of a perfect human being.

The difference between the two images touches on how the two philosophies conceived of the forces of human development. While the *philosophes*, in the words of Condorcet, invested their hopes in a social order and an educational system which would 'render courage and virtue nearly useless' (quoted in: Alain Drouard, <u>Une Inconnue des sciences sociales: la Fondation Alexis Carrel</u>, 1941-45. Paris, 1992, p. 92), the Romantics believed that 'organic' forces would lead individual genius to endlessly perfecting itself. In other words, if the former were preoccupied with social conditions fostering perfection of human nature, Romantic *Naturphilosophie* sought to achieve it through internal resources. Herder was one of the first to translate the philosophical idea of organic forces into a bodily language when he stated that the growth of human genius would correlate with new structures of the brain.

These images keep reappearing in nineteenth-century psychology and physiology. One line follows the lead of the French thinker, Cournot, who regarded a rationally organised society as a mechanism where genius, saint and hero will give place to perfectly reasonable but mediocre humans. In response to Cournot's pessimistic prophesies, Tarde sketched a utopian society governed by a few creative individuals, while the great majority spends its life in a state close to hypnotic imitation. As is well known, Tarde's work was a major contribution to crowd psychology. As for the nineteenth-century physiologists, they adopted a vision of the rational man which took the form of belief in a hierarchy of functions and control over instincts. Alexandre Herzen (the son of the writer) suggested that the way towards the 'cerebral superiority' of the future man goes through achievement of 'instinctive stability' (quoted in: Marcel Gauchet, <u>L'Inconscient cérébral</u>. Paris, 1992, p. 149).

The neo-Romanticism of the turn of the nineteenth and twentieth centuries favoured a view of the future man as a genius guided by deep organic forces. This encouraged a more positive view of mental illness, making it part of the profundity of human nature and a condition of human existence. It also provoked a bizarre but persistent idea about genius as a biological prototype of the future man. Popularising this idea, a German journalist, Nordau, wrote that geniuses differ from the rest of humanity through possessing special kinds of 'brain tissue' and organs, which are destined later to become typical of the whole of mankind. Meanwhile, geniuses were supposed to relate to an average man in the same way that a virtuoso performance relates to music from a music box: while the latter plays no pieces except those for which it was constructed, 'in the exceptional man, the brain centres ... can play pieces that no one ever heard before' (Max Nordau, <u>Paradoxes</u>. Chicago, 1886, p. 134). The two images – an intuitive genius and a well-controlled rational man - continued to alternate in twentieth-century eugenics and psychology, influencing, for instance, research on creativity and child development.

The Scientist and the Poet: Sherrington and the Values of Science in Inter-war Britain

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It is a commonplace of twentieth-century science writing to distinguish discourses of facts and of values. The facts are the proper subject of the content of science. Values are social realities which inspire and sustain the activity of science. However much philosophers, radical activists or social scientists have criticised the fact/value distinction, it remains central to the public presentation and self-presentation of science.

This form of cultural politics is extremely clear in Britain in the decades from the 1920s to the 1950s. I will discuss how a leader of the scientific community, C. S. Sherrington (1857-1952), then became a figurehead for the virtues of science. The argument is that a picture was created of his person – his body, his writing, his public presence – as the embodiment of the life of science at its best. In his many scientific papers, synthesised in <u>The Integrative Action of the Nervous System</u> (1906), and in his leadership of a research laboratory (first in Liverpool and then in Oxford), Sherrington was a model of the disciplined study of facts. He was also a poet, published on the sixteenth-century French physician Jean Fernel, and composed a highly rhetorical vision of <u>Màn on His Nature</u> (1940). This latter activity established Sherrington as someone who showed how humanist values conjoin with a life in science. The scientific community was able to hold up Sherrington (who was also President of the Royal Society of London, 1921-25, and a winner of the Nobel Prize, 1932) to show that science raises human values to their highest point, and does not, in the accusation of conservative critics, oppose values with materialism.

I describe a culture in Britain in which the importance attached to facts in the content of science and to values in the activity of science were thought to be combined in the life of one man. This man achieved figurehead status because he appeared to live an integrated life giving full force to facts in the scientific papers and full force to the emotions and aesthetic sense in the poetry. If such a man was a leader of the scientific community, it suggested that the national culture as a whole might integrate the sciences and the humanities. I argue that it is not just a play on words to consider the relationship between this ideal of cultural and personal integration and the concept of 'integration' which Sherrington made central to his account of the nervous system. This consideration leads outside the fact/value distinction to an examination of structural and historical connections in languages about body and mind, and science and poetry, as well as facts and values.

Though Sherrington was a physiologist, the discussion is very relevant to the human sciences. He aspired to a unified science of the organism, including 'Man'. In his science, he felt defeated by the contemporary impossibility of conceiving how mind relates to body. In his life's activity, however, so many of his contemporaries thought, he enacted an integration of a poetry of mind and a science of body.

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"THE DIGNITY OF MEMORY" Abstracts

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The Splendour of the Bologna Stone: Robert Hooke (1635-1703) on Memory

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Summary- In 1682, Robert Hooke, Curator of Experiments to the Royal Society, presented a lecture on memory to his fellow members of the Royal Society. In it, Hooke maintained that purely material explanations may be given for all memory processes. Hooke formulated his theory as a long sequence of metaphors, analogies and similes. It is my intention to contextualise this theory, and to do so in a specific way: I will use his metaphors and analogies as a means to understand more of the man, his theory and his intellectual surroundings. My purpose is threefold: first, to make sense of a text which would otherwise remain a procession of somewhat arcane speculations on the workings of memory; second, to demonstrate that research into metaphors is a particularly helpful aid in contextualisation; third, to show that Hooke's lecture may stand strange and isolated within his own work, his time and the reigning literary technologies of his fellow mechanist scientists, but that to the contemporary historian of psychology his theory of memory fits in a what was to become a tradition with some remarkable common features: that of 'a physicist doing psychology of memory'.



Allegorical print by Corbould. The goddess of science, globe in hand, turns to the young chemist who, with a superior smile, is igniting an piece of phosphorus with a burning glass. Experiments with phosphorus were emblematic for the new style of chemistry.

In the summer of 1682, Robert Hooke, Curator of Experiments to the Royal Society, presented a lecture on memory to his fellow members of the Royal Society. The text was subsequently published under the title An Hypothetical Explication of Memory: how the Organs made use of by the Mind in its Operation may be Mechanically understood.¹ In it, Hooke maintained that purely material explanations may be given for all memory processes. It was a strange lecture in more than one respect. Hooke had never before taken up a psychological subject, nor did he afterwards; his theory of memory stands completely separate from his other work which mainly dealt with physics and matters mechanical. Second, Hooke's theory of memory was virtually unanticipated, nor did he have any followers. His theory, says Graham Richards, 'rests in glorious isolation as a one-off piece of proto-psychological theorizing of a kind for which the intellectual climate was quite unripe." Apart from this twofold isolation there was a third strange aspect. In the intellectual circles of the Royal Society metaphors were treated with suspicion. The preferred 'manner of discourse' was simple, literal language. Yet, Hooke's lecture was a long sequence of metaphors, analogies, comparisons and similes. Hooke was acutely aware of the tension between his use of metaphors and the Royal Society's view that nature would only reflect itself truthfully in literal language. He therefore had to negotiate his way out with a handful of defensive remarks on the kind of language suited for the description of memory.

It is my intention to contextualise Hooke's theory of memory, and to do so in a specific way: I will use his metaphors and analogies as a means to understand more of the man, his theory and his intellectual surroundings. My purpose is threefold: first, to let you share in the joy of being able to make sense of a text which would otherwise remain a procession of somewhat arcane speculations on the workings of memory; second, to demonstrate that research into metaphors is a particularly helpful aid in contextualisation; third, to show that Hooke's lecture may stand strange and isolated within his own work, his time and the reigning literary technologies of his colleages, but that to the contemporary historian of psychology his theory of memory fits into a familiar pattern. Or rather, I will argue that Hooke's theory is perhaps the oldest example of a tradition with some remarkable common features: that of 'a physicist doing psychology of memory'.

Phosphorus and Memory- Robert Hooke was an exceptionally versatile man. He assisted Christopher Wren in the rebuilding of London after the Great Fire of 1666, worked on scientific instruments for Robert Boyle, built microscopes, pendulum clocks and pocket watches, wrote treatises on earthquakes, comets and magnetism. His principal scientific contribution was the book *Micrographia* (1665), describing his microscopic observations. Hooke had the reputation of being a difficult man to deal with. A contemporary described his character as 'Melancholy, Mistrustful and Jealous'. It may be said in his defence that his position as a Fellow of the Royal Society was an extremely difficult one. Many of the other Honourable Fellows belonged to English nobility. Hooke, in contrast, was of humble origin. As a Curator of Experiments Hooke had to conduct weekly experiments and demonstrations for the Royal Society. This position made him, in a sense, a servant to many masters. Hooke was recognised, Steve Shapin explained, as a person of 'ambiguous autonomy'.³ Perhaps this may help explain why Hooke was so anxious not to offend the Society's literary conventions. In the following I will analyse some of the central metaphors in Hooke's lecture, starting with his references to phosphorescence.

In Italy, Germany and England a frantic search was underway for chemical processes capable of producing phosphorescent materials. Scientific societies corresponded on experiments and exchanged samples. One evening in 1677, at Boyle's quarters in Pall Mall, Hooke had attended a spectacular demonstration of phosphorescent substances by the German alchemist Johann Crafft.⁴ In the darkness, the shutters closed, the candles removed, the Fellows witnessed how Crafft broke a minute part of matter into tiny fragments and scattered them over the carpet. To their delight the crumbs started to glow, twinkling like stars for a good while, without doing any harm, Boyle wrote, 'to the Turky Carpet they lay on.' Next Crafft had asked Boyle to give him his hand. The alchemist gently rubbed some of the matter on the back of Boyle's hand which started to glow intensely. When he blew on it, the light seemed to extinguish, but soon recovered its luminescence. Equally spectacular was Crafft's demonstration of writing in the dark. He took some of the substance on the tip of his finger and started writing on a sheet of paper. After a short while there appeared, in capital letters, across the full width of the paper, the word DOMINI, so bright that one could discern the fingers holding the sheet of paper. It was a fascinating sight, Boyle wrote, 'having in it a mixture of strangeness, beauty and frightfulness.'

Crafft's process was based on the lengthy distillation of large quantities of human urine. Another phosphorescent substance that cropped up regularly in Hooke's reports was the *Bologna Stone*, a mineral found in the vicinity of Bologna in Italy. Seventeenth-century researchers were fascinated by its ethereal light. Baldwin, for instance, wrote that there resided in his piece of artificial phosphorus an 'innate and invisible philosophical fire, attracting by magical means the visible fire of the sun and emitting and throwing out in the dark its

splendour in return."5

The tiny stars on the Turkish carpet, Boyle's luminous hand, the glowing DOMINI, perhaps even the splendour of a philosophical fire - all this must have continued to glow in Hooke's imagination long after the event, for five years later phosphorescence reappeared in his treatise on human memory. In Hooke's view every sense is linked with a special substance in the brain suited to the retention and reproduction of impressions reaching the brain via that sense. According to this principle, there must also be a substance in the human brain capable of preserving light impressions. In order to lend plausibility to this hypothesis Hooke referred to the Bologna stone. If this substance, he argued, has the capacity to receive the impressions of light, retain them and emit them in the dark, why should one not just as easily find a substance in the brain capable of preserving light stimuli and hence forming the physical residue of our visual memories? The Bologna stone, he reasoned by analogy, may show us 'a Specimen of a certain Qualification not to be found in most other Bodies, which may yet possibly be done much more powerfully and effectually by the Chymistry of Nature in the Digestions and Preparations made in the wonderful Elaboratory of the Animal Body.'

The structure of the argument is clear. If it is possible to produce a substance chemically that can retain light and emit it again, it is also possible for brain matter to absorb and reproduce light impressions in a purely physical way. The light in the Bologna stone pointed to a material property which the human brain - thanks to the 'Chymistry of Nature' - possessed in a perfect form.

At the same time this phosphorus metaphor illuminates the scientific milieu in which Hooke operated. Together with Boyle he had conducted experiments with the 'Pneumatick Engine', showing that phosphorus continued to shine even in a vacuum. Demonstrations featuring phosphorus were considered ideally suited for a wider public than just the research community: acquaintances, servants, women, children. Semi-public demonstrations like those of Crafft, Hooke en Boyle played a part in creating a public scientific culture. Experiments with phosphorus were emblematic of this new style of scientific research. In using his phosphorus metaphor, for an audience of researchers all of whom had either attended the demonstrations and experiments or read about them in the *Philosophical Transactions*, Hooke was appealing to a shared experience. The reference to the Bologna stone enabled everyone to picture the special properties Hooke was attributing to brain matter. The fact that there was still no clear explanation for phosphorescence could be regarded as a further parallel, for there was as yet no explanation for the visual memory either. What better metaphor could there be for the operation of the visual memory than the substance that still retained an aura of mysticism and
refused to surrender its secrets even to the most modern scientific instruments, which did not reflect light, but was itself luminescent, a substance that was one of chemistry's most glorious products? By his choice of metaphor Hooke linked the elusive powers residing in phosphorus with the sense of awe at the miracle of human memory.

'Geometry and Mensuration'- Discussing the mechanisms of memory in general, Hooke invited his audience to think of memory as a 'Repository' or 'Storehouse' of ideas. Our senses act as 'Collectors or Carriers', delivering impressions to the storehouse. Actual storage, however, requires the simultaneous activity of the soul, which gives the impressions a certain shape and motion before inserting them in the common repository, where they are preserved and retained for future use by the soul.

Hooke did not wish to bother with the question of the exact location of the soul. He simply assumed that the soul was 'somewhere in the Brain of a Man'. From this position the soul could receive impressions and form new ideas. Ideas, Hooke pointed out, are preserved in the order in which they are formed, like the links in a coiled chain. The idea most recently shaped, the psychological present, is the beginning of the chain, the oldest idea is the last link. Each new idea shifts the chain one position further into the past. Our estimation of duration depends on the number of ideas interposed between the present and some idea at the far end of the chain. Distances in time, then, are marked by the length of the chain of memories. Thus, time came to be conceived of as a spatial quantity. Hooke was particularly proud that he had brought time and memory under what he called 'the Consideration of Geometry and Mensuration'.

Hooke energetically addressed the quantification of memory processes. The rate at which the soul forms ideas and inserts them in memory may vary widely from person to person, he declared. Some may form four ideas per second, others possibly less than one in two seconds. He then proceeded to estimate the number of ideas in the memory of a man of normal mental and physical constitution. This resulted in a truly impressive piece of number crunching. Hooke multiplied the number of ideas with the seconds in a man's life, reaching the spectacular outcome of 'a thousand Millions of distinct Ideas', correcting this figure for 'Infancy, Old Age, Sickness and Inadvertency' and ending up with one million ideas per year. Upon reflection, Hooke considered this outcome, which amounted to 2738 ideas every day, to be probably far too high. He then switched to a manner of computation informed by introspection rather than by calculus. If a man reflects on how many ideas he may have added to his store in the last month, he will probably find that the number will not exceed two or three hundred a day. So a man of fifty may have deposited 1.826.200 distinct ideas in his storehouse. This would seem a realistic figure, Hooke concluded.

Hooke felt that even the initial figure of one hundred million ideas was not a physical impossibility. Very small quantities of matter may contain innumerably many living creatures, each of them with its own shape and with sufficient room to move. As a pioneer in microscopic research Hooke was familiar with small dimensions. A few years earlier he had observed 'animals' in rainwater that were no bigger than a thirtieth part of the breadth of a hair. This would mean that one cubic inch could contain 'eight millions of millions' of these minute creatures. In the human brain, it would appear, there is no such thing as a lack of space for memories.

The Microcosm of Memory- In order to clarify the position and actions of the soul Hooke introduced a second metaphor, more elaborate than the coiled chain of ideas. The Soul, he pointed out, 'forms to itself a Microcosm, or Picture of the Macrocosm, in which it radiates, and is sensible of every thing contain'd therein, in the same manner as the Sun in the Macrocosm'. The representation of the human mind as a a microcosm of the universe gave Hooke an opportunity to cite all kinds of optical and astronomical laws as analogies for the processes in memory. Forgetting, for example, may be due to the interruption of the sun's rays by other images. Hooke likened this to an eclipse of the moon. Hence an image may be apparently lost and recovered immediately the obstacle is removed. Images that are closer receive more powerful radiation from the soul, 'in a duplicate proportion to their Distance reciprocal, much the same with that of Light', which is why memories become fainter with time. In this ratio we recognize the inverse square law. In a letter to Newton, two years earlier, Hooke had applied the same ratio to the force of gravity. As a law of memory, though, the ratio has an apparent exception: some events make a particularly deep impression and are then remembered clearly, however long ago the image was formed. Hooke's explanation is that powerful impressions are remembered more often and subsequently constantly recreated, so that they always remain close to the centre.

In the passages on attention, Hooke uses metaphors which are a precise reflection of contemporary views on the nature of light and radiation. Those views had been inspired by Descartes' *Dioptrique*, which represented light as a pressure that is displaced instantaneously in a transparent medium or 'plenum'. Descartes clarified this theory with the analogy of a blind man who 'sees' with his stick: when the stick touches an object the blind man can tell the shape of the object by the resistance. In Hooke's microcosm of memory the soul becomes aware of the

content of stored ideas by propagating from itself 'a Radiation like the Sun, by which, as by a Stick, it becomes sensible of all those Ideas that are yet unwasted within the Repository, feeling as it were their Form, their Resistance, and their Reactions to its Radiations.' In short, Hooke has quite directly projected Descartes' optical laws into his theory of memory. His microcosm is a heliocentric cartesian universe.

'Only by Similitude'- Given the audience he was addressing on that June day in 1682, Hooke's *Explication of Memory* contained a potential cause of dispute. The founding fathers of the Royal Society had set high standards for the kind of language used to describe experiments and formulate theory. Samuel Parker, for instance, had advocated a general ban on the use of figurative language in scientific discourse. He expressed a particular horror of metaphors, whose 'wanton & luxuriant fancies climbing up into the Bed of Reason, do not only defile it by unchast and illegitimate Embraces, but instead of real conceptions and notices of Things, impregnate the mind with nothing but Ayerie and Subventaneous Phantasmes.'⁶ Hooke justified himself on two grounds for his departure from the literal style prized by the Royal Society. The first was that processes in the human mind fundamentally elude literal description. The second was that graphic images enable the imagination to grasp the relationships between hypothetical processes. There is more to be said about both these arguments.

In the seventeenth century the philosophical status of memory was linked to the question of human immortality. As a result reflections on its material substratum acquired religious significance. Hooke wrote his treatise against the background of Cartesian dualism and was careful to represent only memory, not the soul as a material instrument. This enabled him to describe memory processes in spatial and physical terms similar to those used in describing the material world. But this also created a conceptual problem. Because how does one 'locate' a non-spatial entity like the soul, in relation to a material - and hence spatial - memory? Hooke, as we have seen, did not pronounce on the exact position of the soul ('somewhere in the Brain of a Man'). His other indications of place are always linked to metaphors: at the distribution point of the warehouse, at the start of the chain of memory, Hooke states, 'It is not, I conceive, possible to be truly understood or described, but only by Similitude.' The relationship between soul and memory are quite simply indispensible.

But Hooke ingeniously made a virtue of this necessity. A full appreciation of the subtleties of his logical dexterity requires a short digression on the mechanistic tradition, as it

had emerged in the Royal Society. Following Boyle, a strict distinction was made between facts and theories. *Factual* discourse comprised statements on observable phenomena, or 'matters of fact'. *Theoretical* discourse comprised hypotheses on the explanation of the 'matters of fact'. Shapin and Schaffer have indicated that the epistemological status of the terms in theoretical discourse was uncertain.⁷ Generally they were described, without further definition, as 'notions'. For example, in his research with the vacuum pump Boyle had introduced the term 'spring of the air' to explain the elasticity of air. This 'spring' was explained in turn by the hypothesis that air consists of spring-like particles. The compression of these tiny springs was supposed to explain the resilience of the whole, just as a bale of wool can be compressed because of the elasticity of the constituent fibres. Boyle - quite deliberately - did not answer the question as to how the resilience of the particles was to be explained: in his view 'notions' should be linked as directly and graphically as possible with 'matters of fact'. A figurative explanation like the 'spring of the air' met those requirements, a hypothetical explanation of the causal mechanisms underlying the 'spring' did not.

In his papers on physics Hooke followed these ideas on research and theory as well as he could. In one of his articles, Hooke defined explicitly how hypothetical mechanisms can be linked to 'matters of fact'. The good experimenter, wrote Hooke, should have some provisional hypotheses prior to the experiment if the experimental outcome is to have real informative value. In order to acquire a sufficiently extensive repertoire of hypotheses, the experimenter would be well advised to compare nature with as many 'mechanical and intelligible ways of working as the mind is furnished with' as possible.

Returning to the treatise on memory, we are struck by the fact that here too Hooke is trying to follow the criteria for physical research. The idea that hypothetical explanations for memory processes are 'notions' which can be understood only through mechanical, graphic analogies, and the recommendation that in formulating hypotheses one should use comparisons with familiar 'mechanical and intelligible ways of working' recur early in the introduction to the *Explication of Memory*, in virtually identical terms. Because nothing is so well understood, Hooke writes, 'as when it is represented under some sensible Form, I would, to make my Notion the more conceivable, make a mechanical and sensible Figure and Picture thereof, and from that shew how I conceive all the Actions and Operations of the Soul as Apprehending, Remembring and Reasoning are performed.

Thus, metaphors and graphic images are not only necessary to the understanding of memory processes, they can also have the same heuristic value as in physical research. Analogies like attention-as-radiation, forgetting-as-lunar eclipse or visual memory-as-phosphorescence give the imagination a hold on what would otherwise remain vague 'notions'. Appealing to the need for graphic imagery Hooke included in his theoretical discourse the same metaphors and analogies which had met which such suspicion in the Royal Society.

This already constituted something of a dialectical triumph over his own principles. But Hooke went still further. He claimed to have brought memory within the scope of 'Geometry and Mensuration', in modern terms, to have quantified memory processes. On closer inspection one can see that Hooke has smuggled in size and number along with the metaphors. By comparing the ordering of images with an ever-lengthening chain Hooke was able to take the distance between a given image and the beginning of the chain as a measure of duration. By placing memories in a quasi-solar system, Hooke could formulate his 'inverse square law' on the relationship between the strength of a memory and its age. Both quantifications only had meaning within a purely imaginary space. Hooke ordered mental processes by acting *as if* they were spatial. Metaphors were the tools he used to create space in a memory that otherwise would have remained closed to the imagination.

A Physicist's Memory- Graham Richards' included Hooke in his section of 'forgotten anticipations of psychology'.8 Considering the topic of his lecture this is a sorry paradox. Hooke's theory of memory, Roger Smith has noted, had no influence whatsoever.9 But even a theory which stands isolated in its time may have some representative features in the eye of a historian who can see a longer stretch of tradition. I would argue that Hooke's work is an early example of a style of theorising which over the centuries has constantly associated itself with new technologies. His theory exemplifies a psychology in which state-of-the art techniques for storing and reproducing information came to provide metaphors for hypothetical processes in memory. A nineteenth century example would be Francis Galton's use of his own invention of composite photography as an analogy for the way human memory forms prototypical images or generalized ideas. More recent examples are Feigenbaum's use of computer programs as models of memory, Van Heerden's proposal to view holograms as analogies for memory or the theory formulated by the theoretical physicist John Hopfield, specifying that the properties of what is now called a Hopfield network may explain the neurological substratum of human memory. Within the compass of this short presentation we cannot begin to explore what links all these later 'physicists of memory' with Robert Hooke, but a telling resemblance would certainly be the confidence with which they extrapolate the properties of a physical state of affairs - be it a luminescent mineral or something so articulate as a hologram - to the domain of memory. Their explanations are formulated with the help of literary technologies derived from contemporary

physics, and more often than not these hypotheses seem to profit from the awe and enthousiasm raised by new inventions.

I have no intention to criticize this strategy. Rather, I wished to demonstrate the feasibility and profitability of using metaphors as an instrument of contextualisation and historical understanding. After all, Hooke's metaphors reflected his social and scientific environment. Without being used with that intention, metaphors capture an intellectual climate. In a sense they themselves function as a memory. In metaphors we find preserved what the author saw around him when he was looking for graphic images for hidden processes in mind and memory.

Seventeenth-century still-life painters sometimes hung a gilded globe somewhere in a corner, which afforded a glimpse of the artist himself, half hidden behind his easel. The *Explication of Memory* reflects the personal universe of a researcher with a predilection for quantification and graphic examples. In his theory of memory Hooke designed a microcosm of what inspired and motivated him.

1. R. Waller, The Posthumous Works of Robert Hooke, London, 1705.

2. G. Richards, Mental Machinery, London, 1992, p. 66.

3. S. Shapin, 'Who was Robert Hooke?', in M. Hunter & S. Schaffer (eds.), Robert Hooke. New Studies, Woodbridge, 1989, pp. 253-85 (256).

4. R. Boyle, 'A short Memorial of some Observations made upon an Artificial Substance, that shines without any precedent Illustration', in R.T. Gunther, *Early Science in Oxford*, vol. VIII, Oxford, 1931, pp. 273-7.

5. Quoted in J.V. Golinsky, 'A noble spectacle. Phosphorus and the public cultures of science in the early Royal Society', *Isis*, 80 (1989), 11-39 (20).

6. S. Parker, A Free and Impartial Censure of the Platonick Philosophie, Oxford, 1666, p. 76.

7. S. Shapin & S. Schaffer, Leviathan and the Air-Pump, Princeton, 1985, p. 50.

8. Richards, Mental Machinery, p. 66.

9 R. Smith, The History of the Human Sciences, London, 1997, p. 29.

Trauma and Memory: Connections between Psychology and History

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Abstract submitted for the European Society for the History of the Human Sciences Conference, Florence, Sept. 3-7, 1999.

During the last few years, both the disciplines of history and of psychology have witnessed a strong interest in issues related to trauma, traumatic experience, and the way individual and collective history is represented. In this paper, we argue that the interests in trauma and related issues in both disciplines can be related in many interesting ways, enriching both. Psychological theory and research could gain an historical awareness which it is often lacking; additionally, moral and political angles could be developed. Historical research could be enriched when it is informed with psychological theory as well.

The philosopher of science Ian Hacking, in his study of Multiple Personality Disorder, has drawn attention to the historical nature of psychological syndromes. In his study *Rewriting the Soul*, he attempted to trace the historical conditions for MPD to become such a prevalent disorder (in particular in the United States) as well as the general preconditions of memory becoming an object for historical research. He relates the prevalence to MPD to an increased awareness of the sexual abuse of children, which strongly increased during the 1970s. Only after this was recognized in the public opinion as a pressing problem could individuals use these terms to describe their own experience. A similar point in made by the trauma-researcher Judith Herman in her *Trauma and Recovery*. In this book she claims that only after public awareness of certain forms of trauma (war neuroses, sexual abuse) has arisen can individuals work through their individual experiences.

Today, historians pay a great amount of attention to issues of trauma, collective memory, and the way collective memory is organized through commemorations and representations in the media and film. Commonplace is the attention given, as for example by Sturken in her *Tangled Memories* to the representation of the Vietnam war in the Vietnam monument and in film. The role of monuments in remembering and commemorating World War I has been analyzed by Winter in his *Sites of Memory, Sites of Mourning*. The recent experience around the Enola Gay exhibit in Washington, DC, which aimed at providing a critical perspective on the throwing of the first nuclear bomb, revealed that exhibitions and other commemorations can lead to controversy and heated emotion. The recently concluded ten-year debate around the Holocaust memorial in Berlin illustrates the same point. Central issues for historians are how the past is represented by individuals, in particular when it contains traumatic elements. Here they often seem to apply psychological principles on a broad scale.

Already for a long time, a number of clinical psychologists have focused their attention on the effects of traumatic experiences of Holocaust survivors, in particular children. Initially, these individuals were explained on an individual level by labeling them as suffering from psychiatric problems and syndromes such as schizophrenia or psychosis. In Germany, immediately after the war there was widespread awareness of the horrors of the Holocaust, which disappeared during the 1950s when the reconstruction of the nation and working towards a better future were high on the national agenda. In the late 1960s, the *Inability to Mourn* appeared, which focused on the inability of average Germans to come to terms with the past and the result collective repression of war and genocide. At the same time, survivors and their children began to tell their stories. Increasingly, psychologists started to relate the suffering of some of their clients to the experiences they suffered through

the Nazi regime.

More recently, clinical psychologists and system therapists have become interested in patterns of inter-generational transmission of trauma within families as children and grand-children of both Holocaust survivors and perpetrators came forward discussing their experiences. This was supported by psycho-drama therapy groups in Israel and Germany. A pioneering effort in Germany was made by the sociologist Gabriele Rosenthal, who with her collaborators interviewed families from the former East and West Germany and Israel to conduct a narrative and interactional analysis of how they articulated issues related to the Holocaust. Recent research in this tradition has focused on how trauma is passed on through the generations. In general, the second generation displays little awareness and interest in the experiences of their parents while the third generation is more likely to show an openness to explore the past, at times together with their grandparents. In a recent social-psychological study, Harald Welzer presented historical film-clippings and the like to elicit memories in elderly experimental subjects. He was mainly interested in the way interviewer and interviewee interact with respect to taboos and forbidden topics as well as those issues both were willing to discuss.

In all this psychological work, there is a strong awareness of the influence of actual historical events on individuals and how this awareness is transmitted between the generations. As a result, new insights have been presented on the way the different generations deal with these experiences. While psychologists focus on individual experience and family interactions, historians can add a valuable perspective in outlining how, on a more collective level, representations of historical events are created and transformed.

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ESHHS European Society for the History of the Human Sciences

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Symposium

"PSYCHOLOGIZATION"

Abstracts

Ruud Abma, Arjan de Boer, Jaap van Ginneken, Jeroen Jansz, Paul Voestermans, Ido Weijers

Psychologizing social orientations

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What was the key contribution of social psychology to the 20th century? It lay primarily in the psychologizing of the social orientations of individuals, in making them amenable to attempts at 'measurement, prediction and (attempts at) control'. Opinion and attitude technology cleared the ground for the information, communication and persuasion strategies which came to be a major feature of 'free' societies.

Between the Middle Ages/ Renaissance and the Enlightenment/ Industrial Age, the West gradually shifted from a traditional/ collectivist to a modern/ individialist type of society. The onset, speed and disruption of these processes differed between major established Western European countries such as Great Britain, France and Germany on the one hand and a North American immigrant society such as the United States. Similarly, the urge and need to manage 'collective subjectivity' differed greatly.

But all around, social orientations gradually became problematic in a number of domains. The economic domain: with its mass marketing of consumer goods. The social domain: with its surveys of various aspects of 'the social question'. The political domain: with mounting stakes riding on the outcome of democratic elections. The military domain: with 'the draft' changing the nature of warfare. The communication domain: with media becoming ever more important channels for everyday messages (in contrast to mere face-to-face interaction).

It was against this specific background, that empirical psychology developed from the late 1870s on, and modern sociology from the late 1880s on. Yet they employed rather different notions and methods, and soon found it hard to agree on what united rather than separated them. Rather than reaching agreement on the 'missing link' between them, psychologists developed a psychological social psychology (focusing on individual actions) whereas sociologists developed a sociological psychosociology (focusing on social practices).

This becomes immediately apparent if one compares the social psychology of Floyd Allport and the psychological behaviorists with the psychosociology of William Thomas, the Chicago School and the sociological interactionists. Although they soon made 'opinion' and particularly 'attitude' central concepts, both their theoretical approaches and their practical research were rather different. The hidden premises of the attitude concept as used by the behaviorist psychologists, for instance, have been well dissected by Kurt Danziger.

The decisive developments, however, were those translating these rather vague ideas into a well-defined technology, somewhere between the mid-twenties and the mid-thirties. One

4

breakthrough was the development of various early techniques, culminating in Leon Thurstone's famous manifesto article 'Attitudes can be measured'. Another breakthrough was the development of better methods of survey sampling, culminating in the correct prediction of the 1936 presidential election result by George Gallup and others. From that moment on, 'O&A' technology was put on the map and on the agenda.

It spread in the US, to the UK and other Anglosaxon countries. In England, it superseded competing approaches such as 'Mass Observation'. The development of similar approaches such as Paul Lazarsfeld's research in Vienna, and Erich Fromm and other's research from Frankfurt, were soon slowed down by the coming to power of the nazi's and the outbreak of the war. While some such research was still done in Germany, France and other countries during the war, it did not develop real sophistication. In the US and the UK, by contrast, 'O&A' technology came to play a major role in the managament of the war itself, of military and civilian morale, etcetera.

After the return to peace, the new tools quickly spread to the rest of the western world along with American goods and companies, management and marketing techniques. By 1955, the basic principles were written out and available everywhere; by 1965, they were employed on an ever wider scale. Today, economic, social, political, military and media life are unthinkable without 'O&A' technology. Yet in recent years, there has been a fundamental critique of its sometimes reified, reductionist and mechanistic nature.

On the one hand it seems as if the 'measurement, prediction and control' (or at least management) of public perceptions has been increasingly perfected. On the other hand, periodic communication 'crises' have drawn attention to the fact that something very important seems to elude researchers. New approaches deriving from chaos and complexity theory, in combination with mass psychology and collective behavior sociology, shed an entirely different light on these seeming anomalies. They offer a much better explanation of why infinitely small details may throw the entire 'complex dynamic system' of public opinion off-track.

Psychologization and the mission of Psychology

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This paper is concerned with the wide dissemination of psychological knowledge, instruments and professional expertise in the Western world. In the first part of the paper, it is discussed how psychologists themselves conceptualised the public function of Psychology. The 'mission statements' of Heymans (1909) and G.A.Miller (1969) will be presented, next to the proposals made by Stanley Hall and Watson in their utopian treatises of the 1920s.

The second part of the paper addresses the actual societal and cultural success of twentieth century Psychology. The diverse proliferation will be summarized by Psychology's two key successes. Success Number One is the measurement of individual (mental) differences, in particular with respect to what psychologists have called intelligence. Psychology has been able to replace the imprecise 19th antropometric measurements of bodily shape and size of the skull by a clear-cut score that did not take the detour of outer expressions of inner qualities. The mental, or psychological test successfully claimed a direct entry into the hidden, inner characteristics of the individual. Psychology's Success Number Two emerged in the decades after the Second World War with the development of short-term psychotherapy, in particular the humanist practice of advisory support called counseling.

In the last part of the paper, the effect of Psychology and all kinds of 'psychologies' on Western societies in the 20th century will be subsumed under the banner of psychologization. The historical process of psychologization refers to two developments: First, the growing emphasis on the individual's 'inner' world of thoughts, feelings, and wishes that is discernable in Western culture. And, second, the corroboration of this 'turn inward' by scientific accounts (theories?) of the so-called 'inner' determinants of 'outer' conduct. From the inception of Psychology as a new academic discipline on, psychologists like Wundt and Fechner were busy to disentangle the natural laws of consciousness. And so was Freud in his own, particular way. And despite anomalies like behaviorism, twentieth century Psychology has shown a strong dedication to the excavation of the inner workings of the human mind. Both the 'turn inward' and the 'scientific accounts' will be evaluated as aspects of psychologization.

Psychologizing culture: from Volksgeist to the patterning of behavior

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This paper is about psychologizing culture; how psychology,s participation in debates about civilization and culture along quite some detours resulted in viewing culture and its participants increasingly in psychological terms. The nature of the participation depended on the degree in which psychological knowledge was put to use, either embedded in other disciplines like medicine or anthropology, or, later on, as part of an established discipline, dealing with racial issues, heredity, personality development, and individual or group differences. In the early 19th century culture debate among the English, French and German intellectuals, each country,s scholars chose a different emphasis: economy and utilitarian thought in England, the social system and political thought in France, education (Kultur) in Germany. Focusing on the German educational approach, I will start with Herbart's emphasis on Volksgeist, the spirit of a nation or people, which provides the mind with content. Herbart emphasized education as the process in which various minds get attuned to one another.

I will show how Herbart, s theory about a non-associationistic mental machinery of conscious and unconscious forces in the mind resulted in an interesting psychological perspective on culture. Yet, his view remained empirically empty because in the ensuing Völkerpsychologie of Lazarus and Steinthal, Herbart, s Volksgeist theory was not psychologically developed further. It was used instead to counteract racism by emphasizing thecultural instead of biological origin of mental endowment. I will show that culture became the origin of the psyche by postulate and not so much by doing actual research. In order to combat the idea that people were subjected to biological-hereditarian racial pressures, which made them less fit for civilization or otherwise inferior, culture theory became a moral alternative. That is to say, emphasizing the cultural origin of mind was just a disclaimer with respect to the irresponsible use of blood, racial features, and other physical characteristics as the determining factors of human worth. Culture became psychologically equated with contextual influence on individual and group differences, set off against hereditarian ones. The ensuing nature/nurture debate and the resulting research practice got momentum from the Galtonian research style. The Herbartian legacy got obfuscated. Biological determinism and hereditarianism won the battle, so to speak. Psychology became involved in as an accomplice. Of course, there were deniers of the biological determinism, but they retreated in launching an psychological anthropological program under the leadership of Boas, which ended up as an alternative to the evolutionists,

7

view of one single civilizing process. This happened at the fringe of psychology, not in the hard core area. The anthropological notion of culture (relativistic in nature and emphasizing peoples, way of life) came into existence. From that moment on culture functioned as a context for personality development.

Viewed against the background of what thus became of culture theory, I will argue that it took quite a few decades to put the educational involvement with culture on the research agenda again. This was done by Marxist scholars under the banner of the Russian Cultural-Historical school in psychology. The cognitive and related psychological aspects of Œthe other, were studied as part of the others, cultural practice. I will argue that this was a counter-force of great historical impact, producing a psychologization of culture of a different kind: the behavioral patterning ensuing from established cultural practices became the object of psychological research.

My historical account of the emergence of this new approach (while the other ones remained on the scene) leads to the hypothesis that the hypostatizing of culture in the Völkerpsychologie (Herbart's initial non-associative approach notwithstanding) was as a somewhat powerless 'interlude', which kept psychology as a science (with useful analytic tools) away from dealing with culture, particularly Western culture itself. In the final part of my paper I hope to show that the psychological approach of multiculturalism still suffers from the failure to take culture as a proper subject of psychology.

<< Visit our Web Page<http://www.socsci.kun.nl/psy/cultuur/>>

Psychology and law

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Today, psychological assessments are important sources of information in a variety of legal contexts, from criminal courts to juvenile and family courts to civil courts. This presence of psychology in legal contexts is of a quite recent date. Forensic psychology as a relevant and autonomous kind of expertise in court has been emerging only at the end of the nineteenth century, following the successes of the two oldest forensic specialities, medicine and science. In the preceding century medical authority had succesfully acquired the role of necessary advisors to the criminal courts about madness; scientific expertise began to develop in the civil law courts during the Industrial Revolution, when engineers and chemists were involved as forensic experts in compensation cases. Following these examples the role of the psychologist in court has been rising in the twentieth century as an expert witness with an continuously expanding field of expertise whose breakthrough came recently, in the sixties.

A social history of psychology and law is a history of a complex process of demarcation. First, it is a history of a boundary dispute with the authority of the legal profession. Second, since psychiatry had been involved in a roughly analogous dispute in the last century and since psychiatrists had cleared the path and claimed full authority concerning all 'mental' questions in court, it is also a history of a boundary dispute with the authority of the medical profession. Third, it is not only a history of scientific demarcation processes, but also of political disputes about criminal law. Since forensic psychiatry became engaged almost naturally with the Enlightenment tradition of humanitarian jurisprudence and since the strongest opposition to an important role of psychiatry in court came from conservatives who believed in deterrence, forensic psychology right from the start has tried to steer a middle course. It is the central thesis of this paper, that forensic psychology claimed such a politically neutral course on the basis of its scientific objectivity and the verifiability of its judgements. Fourth, more then any other field of psychological interferences in social life, psychological intervention in the field of law has been continuously subject of public debate, in which basic and changing views of human responsibilities, rights and risks come up. My paper will draw attention to these complex and continuing demarcation processes and ongoing disputes. I will focus in particular on the relation between forensic psychiatry and psychology. While forensic psychiatry was viewed as 'weak', that is both weak in political terms - in connection with welfare policies - and in scientific terms - objectivity -, forensic psychology claimed a strong, 'hard' and 'neutral' position: that is both

'political neutrality' and 'hard science'.

From the middle of the nineteenth century the alienists had developed their profession, their medical authority and their social prestige for an important part via the field of law, first by creating formal organisations and defining qualifications for membership, second by struggling for recognition in the judicial system and its legal professionals. Psychiatry established itself as the leading administrative power concerning mental health and law in this boundary dispute with the authority of the legal profession, focussing on the insanity defence. Half a century later, a new discipline, 'forensic psychology', started to follow a comparable route. Entering the field of law as a social science, though, meant not only a struggle with legal authority, but also a boundary dispute with the now established authority of the medical profession. Nineteenth century's fortune of 'medical psychology' in court served as a model for forensic psychology. However, the roots of this discipline did not lie in the insanity defence, since at the end of the nineteenth century this problem was viewed as a medical question. Forensic psychology sprang from the psychological laboratory, that is, it arose directly from the work of Wilhelm Wundt. The spirit of experimental psychological methodology was applied to legal problems: the identification of the offender and the reliability of eyewitness testimony were the first issues which attracted the interest of the new psychological researchers.

A complex of social factors with different national influence and meaning seem to have made the time ripe for the flourishing of psychology in court in the Anglo-Saxon world in the twentieth century. One factor has been the repeated wartime experience, which paved the way for a broadening social acknowledgement of the practical usesulness of applied psychology. Another, more direct factor has been the introduction of new legislation which strongly promoted recruiting psychologists in positions which brought them in court as expert witnesses. Another factor was the successful application of psychological perspectives in a major court case. Finally, by 1960, there was the introduction of some pathbreaking legal decisions which, after more than half a century of 'preliminary work', explicitly confirmed the autonomous role of the psychologist in court. Both in America and in England the success of forensic psychology as a flourishing practice of applied psychology came for a large part to hinge on the acknowledgement of this discipline's expertise independently of medical authority. These legal decisions make the crucial dimension of a boundary dispute between psychiatry and psychology crystal-clear.

In the fifties the boundary dispute between psychiatry and psychology had become to concern all dimensions of mental health where psychiatry had claimed expert authority since its first interference with criminal justice in the last century. The use of the Hearsay Rule acknowledged an autonomous role for the forensic psychologist next to the psychiatrist. By the end of the fifties in America both psychiatrists and psychologists were engaged in questions of competency and insanity, two legal concepts without any perfectly corresponding mental health

10

concept, notwithstanding their far-reaching consequences for the individuals concerned. Since apart from testing eyewitness testimony most activities of American forensic psychologists have to do with questions of competency and insanity, psychologists appear to have at last succesfully invaded here the province of psychiatry by the middle of this century.

My paper will focus on the vicissitudes of psychology in the field of criminal law and finally I will make some remarks on differences in legal contexts. My paper focusses on the developments in the Anglo-Saxon world, but I will make some comparison with the continental European model.

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Session

"HISTORY OF PSYCHOLOGY: CHALLENGES AND CRITICAL ISSUES"

Abstracts

M. Andrews, N. Bauer, J. A. van Belzen, H.-P. Brauns, G. Chiari, T. Dehue, A. Febbraro, M. Ferris, S. Gori-Savellini, I. Lubek, M. L. Nuzzo, S. Mayer, P. J. van Strien, F. Velicogna

Ideology and personality as shaping factors in the history of psychology; On the non-involvement in psychology of religion by Dutch Calvinists

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While it is often said that the psychology of religion flourished around the turn of the century, went under in 1912 (with the cessation of the publication of the American Journal of Religious Psychology and Education) and only reemerged after the decade of the sixties (Beit-Hallahmi 1989; Gorsuch 1988), this assertion certainly cannot claim universal validity. In German-speaking countries the psychology of religion continued to grow till right into the thirties, but up until now has not made a comeback; in Scandinavia there exists a modest but unbroken tradition in the context of religious studies. As far as The Netherlands is concerned, the general comments made until now in the literature about the 'demise' of psychology of religion in the first decades of the 20th Century as having been "quick and total" are not applicable and are not at all appropriate for the segment of history this paper deals with: the pre-WWII era at the Free University at Amsterdam.

Paradoxically, the only Dutch works in the psychology of religion from that era still being read, were written at the Free University in Amsterdam, where – until very recently the subject was not taught, but even opposed. The paper tries to explain for this paradox, and dealing especially with the pre-war era points out that the (non-) development of this particular branch of psychology can only be understood when ideology and personality are being taken into account as contextual factors in their own right.

Hermeneutic constructivism: A paradigm for the psychology of the future?

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In late years we are observing an unusual kind of dialogue. Biologists, cyberneticians, computer scientists, anthropologists, psychologists and psychotherapists, converse with each others, and (more surprisingly) all of them converse with hermeneuticists. What makes such a dialogue possible is a common dissatisfaction with the traditional, rationalistic answers to the question "what it is to know", and a widely shared metatheoretical framework about a possible alternative answer. It is difficult to embrace with a single term the view of knowledge emerging in social and human sciences. If we disregard the specific, particular differences, we can see a similar approach to cognition and reality in discourses labeled as post-modernist, post-structuralist, neopragmatist, deconstructivist, narrativist, discursive, conversationalist, constructionist. enactive, constructivist. These discourses are frequently intertwined, and can be seen as specific elaborations of a similar metatheoretical framework in the application to different fields of inquiry. The paper is aimed at outlining the distinctive, common features of what we call collectively hermeneutic constructivism, by tracing back to its historical roots and anticipating its possible developments, with particular reference to psychology and psychotherapy.

Some remarks on the subject of the history of psychology

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It is the thesis of this paper that the history of psychology has a subject as long as there are systems of statements in different times containing certain classes of contents and/or claiming to be psychological in a peculiar sense. As a point of departure in order to strengthen this general view the first emergence of a term of scientific psychology is chosen, namely in writings of Goclenius (1590; 1598), Hawenreuter (1591) and Stupanus (1596). It can be shown that at least the first two authors explicitly connect their concept of psychology with problem areas dealt with by Plato (Timaeus) and Aristotle (de anima) so that a tradition-line over about 2000 vears is constituted. Further evaluating of these documents in a synchronic perspective reveals a well circumscribed concept of psychology, and in diachronic perspectives a psychological conception which is incommensurable in a strict Kuhnian sense (Brauns, 1997) with later conceptions. Incommensurability in regard to a concept of psychology can be further demonstrated by inspecting time samples of later times: Wolffs (1751) concept of psychology as well as the Wundtian (1874) one e.g. Obviously, this diachronic view gives remarkable support to the historical fact of long lasting tradition-lines clearly marked by incommensurable concepts of psychology. When taking into account the problem areas included by these various psychological conceptions the fuzzy outlines of a consensual subject of psychology are pointed out. Nevertheless, the view of a special subject of psychology with a changing identity down the ages and as one of its consequences a correspondingly substantiated history should be supposed. So, finally psychology allies to all those sciences whose traditions start in antiquity and agrees in showing a subject or classes of subjects which in turn are the indispensable foundation for any historical discourse above them.

From deception-trials to control-reagents The introduction of the human control group a century ago

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The present history amends previous histories of the control group. New information is added on the earliest introduction of controls as a vital methodological requirement in psychological experimentation. The essay discusses why controls were launched about a century ago, and how come this occurred in psychology. Moreover, it investigates how the earliest example fell into historical oblivion.

In 1872, Francis Galton, the British statistician and biometrician, published an article under the provoking title "Statistical Inquiries into the Efficacy of Prayer" (Galton, 1872). Arguing that the profit of piety should be assessed on the basis of its earthly revenues, the paper was a notorious contribution to nineteenth-century science. So it was however, for another reason. In making his case, Galton introduced a new research strategy. As he explained,

The principles are broad and simple. We must gather cases for statistical comparison, in which the same object is keenly pursued by two classes similar in their physical but opposite in their spiritual state; the one class being spiritual, the other materialistic. Prudent pious people must be compared with prudent materialistic people and not with the imprudent nor the vicious ... We simply look for the final result - whether those who pray attain their objects more frequently than those who do not pray, but who live in all other respects under similar conditions (Galton, p. 126).

Galton thus explicated a methodological design that was to become common practice in contemporary psychology and many other disciplines. Nowadays it seems evident that the results of actions should be evaluated via comparison of groups equal in all respects but the action concerned.¹ It even has become obvious that, ideally, researchers themselves assign subjects to groups rather than draw conclusions on *post hoc* comparison, as in Galton's example.

In Galton's times, however, the basic notion of group comparison needed extensive

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¹ Until today, research on the efficacy of praying is widely conducted according to this scheme. However, it has not proven as simple as Galton thought it would be (see Sloane, Bagiella & Powell, 1999).

explanation. Historians of science have traced but a few scattered pre-twentieth century examples concerning cultivated plants, laboratory animals, or hospitalized patients (Boring, 1954; Cochrane, 1976; Coleman, 1987; Marks, 1997). Moreover, historians of psychology and social science have not found a single pre-twentieth century case of human controls. In an historical introduction, the experimental psychologist Richard L. Solomon noted that "We were struck with the fact that the use of an experimental and a control group ... is a very recent development" (Solomon, 1949, p. 137). Solomon found the earliest instance in Thorndike and Woodworth (1901). However, the authoritative historian of psychology Edward G. Boring discarded these experiments as a proper example. Thorndike and Woodworth did not seem to have looked upon control as a methodological necessity. They referred to a single trial with a control group only casually and after having discussed a range of experiments without controls (Boring, 1954, p. 584). Ever since Boring's reconstruction, historians of psychology have ascribed the first introduction of control groups to Solomon's next case of a 1908 British educational experiment. The earliest use of the *word* "control" in relation to human controls is located in American experimental psychology of the middle twenties (Solomon, 1949, p. 138).

The present essay amends the foundational work by Solomon and Boring. It recounts the deliberate employment of experimental and control "reagents" in early twentieth-century experiments by the Stanford psychologist John Edgar Coover. In a 1907 article, Coover explicitly introduced his control groups as a methodological necessity, and ... as a *critique* of Thorndike and Woodworth (1901). Moreover, the essay demonstrates that the emergence of control groups in psychology only seems "strikingly late" when one ascribes twentieth-century aspirations to nineteenth-century practices. To this purpose it begins with a brief discussion of the social developments that eventually brought about experimental group comparison.

From aristocratic to bureaucratic social policy and social science

Throughout the nineteenth century social change and social knowledge-creation were two separate spheres. Amelioration was a matter of local philanthrophy and the production of social knowledge an affair of men of letters. Statisticians focussed on population universalities rather than human diversity. By calculating the averages of repeated measurements they established the typical cases as intended by God or Nature (Hacking, 1990; Desrosières, 1998).

In turn-of-the-century Western countries, however, the common ideology of laissez-faire

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politics was countered with that of planned social change. Private charity was gradually replaced by centralized government. This process was to induce significant changes in both the *relation* between social policy and social knowledge-making and the *kind* of knowledge produced. Whereas the authority of nineteenth-century local officials depended on their personal trustworthiness, the rising class of bureaucratic officials had to justify its plans in terms of impersonal standards, procedures, and data. Thus with the shift from aristocratic to bureaucratic domination a large-scale alliance developed between social government and social research. Social scientists began to focus on population varieties rather than communalities, and to produce technically useful data based on standardized methodological procedures. Heralded by Francis Galton, the former "intellectual-statistician" was turned into a "government-statistician."

Under these new circumstances, and particularly in the United States (Ross, 1991), a widespread demand rose for impersonal assessment of the efficacy of administrative actions. In the 1910s, F. S. Chapin, an engineer and sociology graduate from Columbia University, argued that the *differences* made by social enactments should also be scientifically established (Chapin, 1917). Chapin conceded not to know yet how the effects of administrative policies could be assessed without arbitrariness. Simple before and after measurement would not do, he reasoned, as many other events than the policy in question may influence the results.

In the meantime, *psychologists* were dealing with this problem by comparing experimental and control groups. Coover, who first discussed this comparative group design as a methodological necessity, was a canny methodologist indeed. Yet, the design was not an instantaneous creation by a single genius. Just like conventions in society at large, the new research methodology developed gradually from preceding traditions. The psychophysical roots of experimental psychology can explain why it was via this discipline that experimental group comparison was introduced into social science.

From psychophysics to educational psychology

Psychophysical experimentation began as a typical endeavor of mid-nineteenth-century scholars interested in the design of human nature. In an attempt to understand the relationship between physical stimuli and mental sensations, they tried to establish the tiniest difference between two stimuli that could still be perceived. For instance, experimentors lifted series of unequal weights or pricked their skin with two pins at varying distances, in order to find the "threshold of perception" below which distinctions are imperceptible. The averages of series of

measurements were taken as representations of general human functioning (Boring, 1943; Danziger, 1990).

The first psychophysical researchers acted as their own experimental subjects, but in the 1870s the idea rose that it in fact takes two to experiment. For instance, in relation to the pinprick experiments it was argued that knowing the pins' real distances in former trials might influence the perception of the distances to come. An "assistant" was needed who would provide the stimuli and write down the responses. And, most importantly in the present context, the assistant was also required for the sake of *control* trials or "deception" trials in which the skin was secretly touched at one point only. Such trials were inserted because the knowledge that there always is a distance might induce some observers to *think* they felt two pricks even if the distance was actually below the threshold of perception. With one-prick control trials such observers could be caught. Let the original stimuli be indicated as "S", the randomly inserted control trials in which the actual stimulus was omitted as "*noS*," and the responses as "R." Then the scheme of these experiments was S-R-*noS*-R-S-R-S-R-S-R-S-R-S-R (etc.). Eventually, the insertion of control *groups* would emerge from such control *trials*. In order to understand how that could happen yet another psychophysical phenomenon needs to be taken into account (for a more extensive discussion of the present section, see Dehue, 1997).



Aesthesiometers for pinpricks experiments. The one with three pins was especially designed for control trials in which the observer was not really touched with two pins (from Schulze, 1938, p.5).

From the 1850s, the issue was also discussed that the threshold was artificially lowered when the experiments were conducted with observers who became too skilled. Switching to the other arm or leg did not solve the problem because the unwanted training seemed to transfer automatically to other parts of the body. By the end of the century, this so-called "transfer of training" phenomenon was intensively studied. Moreover, from a suspected source of bias, the

transfer effect became a psychophysical issue of its own. Experiments were conducted to study the effects of training in all kinds of sensory, motor, and cognitive skills. In these experiments the established experimental scheme had to be adapted. Once the stimuli had become *trainings* ("T"), they no longer could be haphazardly undone. As sequences of T-R-*noT*-R-T-R-T-R-*noT*-R (etc.) were not practicable, the design was limited to a particular "cutout" of the original scheme: *noT*-R - T-R. First, an ability ("response") was tested without preceding training, next the training was given, and subsequently the ability was tested again.

Transfer of training experiments became an important resource in psychology's turn to the production of regulatory knowledge and the nearly simultaneous introduction of the control group. At the turn of the century a debate emerged in the newly established circuit of educational administrators on the utility of teaching subjects such as Latin and formal mathematics. Whereas some pleaded to abolish such redundant subjects from the school curriculum, others argued that "formal discipline" strengthens general mental capacities. This issue reminded psychologists of the psychophysical transfer of training phenomenon. The 1901 experiments by Thorndike and Woodworth (mentioned by Solomon as the earliest instance of a control group) provide an early example of the use of psychophysical transfer experiments in the context of educational debates. They disputed the educational profit of formal discipline on the basis of experiments according to the just mentioned scheme of (noT-) R-T-R.

In the next decades thousands of American teachers were taught by reference to these experiments that formal discipline is a myth. Until the 1930's the outcomes were described as "a veritable bombshell into the educational camp" (Kolesnik, 1956, p. 7 and 34), and still today historians refer to their detrimental consequences for the belief in formal discipline (Dewsbury, p. 1122; Beatty, 1146). Yet, soon after publication the validity of the experiments was severely criticized. John E. Coover from Stanford University rejected them for their lack of control groups.

Coover was a student of the psychophysical experimenter Frank Angell who ran a oneman department of psychology at Stanford University. Before Coover began his studies in psychophysics, he had worked as a school principal (Coover, 1912, p. 308). He soon gave Angell's academic interests an educational twist. Coover took part in the debate on formal discipline, which in 1905 resulted in a master's thesis. In a 1907 article on the topic, co-authored by Angell, he publicly reported on his experiments with "experimental reagents" and "control reagents." He pointed out that the results on the second test might be caused not only by the training but by other factors as well. As he argued about other studies,

It is to be regretted that the authors do not carry on a "control" experiment along with their tests to ascertain the training effect of the tests themselves and to throw additional light on the changes taking place in the training intervals (Coover & Angell, p. 329).

Thorndike and Woodworth's experiments in particular were discussed as "rough experiments" of "very little value" (Coover & Angell, p. 330). Coover *supported* the notion of formal discipline at the bases of observed differences between the results of reagents who had been given training and control reagents who had not. In reporting these experiments, he repeated the reasons for employing control groups three more times.

With this design the usual sequence of noT-R-T-R (a test without a training, the training, and next a test after training) was cut in two. The control group underwent the first part of noT-R, the experimental group the other half of T-R. In other words, the classical misleading control trial in which the stimulus was withheld, was now applied to special control subjects.²

Epilogue: Coover's unacknowledged impact

Control groups emerged only after the transformation of social science from a largely intellectual to a regulatory undertaking. Moreover, it does not come as a surprise that the human control group was first introduced in the context of research on transfer of training. Originally, transfer was itself an intervening variable. And the volunteers in laboratory research could comparatively easily be kept *under control*, as could the schoolchildren who subsequently became the principle subjects of educational experimentation. When it came to the evaluation of other administrative actions, not every social researcher was convinced that group comparison offers a feasible possibility. Chapin, for one, never accepted it, arguing that experimentally controlled allocation of subjects to social legislations collides with the humanitarian mores of reform (Chapin, 1947). Other social scientists, however, such as the powerful political scientist Charles E. Merriam, attracted psychologists as methodological advisors and employed control groups in their own research projects (Ross, 1991).

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² Coover again added pretests for both groups (R-T-R and R-*noT*-R). It is intriguing that Solomon acutally wrote the article with a historical introduction on the control group for discussing the common practice of pretesting. As he pointed out this practice diminishes the *external* validity (Solomon, 1949).

Space limits prohibit an account of the further dissemination of the control group design (Dehue, in preparation). The present article ends with the question of how Coover and Angell's article could have been omitted from the historical record. Thorndike and Woodworth's experiments were an important preliminary step in the development of the control group, and they indeed did informally refer to a trial with group comparison (Thorndike & Woodworth, 1901, p. 558). However, it clearly was Coover who introduced group comparison as a vital condition in psychological experimentation. How could it happen that his rightful place in the annals was overlooked?

Solomon's article provides an intriguing clue. In his historical overview he refers to some pages in Woodworth's renowned textbook *Experimental Psychology* (1938, p.178-181). In these pages Woodworth too appears to support group comparison in transfer research, and to sum up the authors who first introduced this view.³ It seems that Solomon borrowed his data directly from Woodworth, including the reference to Thorndike and Woodworth as the earliest instance.

Even if this story took place in the present, conclusive proof that Woodworth deliberately omitted Coover and Angell would be hard to find. Yet there is circumstantial evidence. First of all, Coover was not particularly a man without reputation. After a PhD on formal discipline in 1912, he received enormous sums of money from an endowment meant for psychical research. Both the story of this donation and Coover's astute parapsychological experiments widely set tongues wagging (Dommeyer, 1975; Mauskopf & McVaugh, pp. 253-254 and 285). Coover also figures in quite a few histories because of his extraordinary personality. His Stanford colleague Quinn McNemar remembered him as "owl-wise Coover" who would help with the most complicated logical problems (provided one had the patience to listen to his endless elaborations). But Lewis Terman, Angell's successor at the chair of psychology and a Woodworth ally,⁴ considered Coover "an irritant" and an "Angell holdover" whom he could not dismiss (McNemar, 1980, p. 316-317; Minton, 1988, p.135).

³ Remarkably, Woodworth, nor Solomon or Boring, referred to the use of control *animals* in Thorndike's dissertation research (Thorndike, 1898).

⁴ See Woodworth on Terman in McNemar & Merrill (eds.), 1942, pp. 3-11.



Members of the Department of Psychology at Stanford University in 1932 on "History corner Steps." Quin McNemar (left), Lewis Terman (front, middle), and John Coover (right). Courtesy Department of Special Collections, Stanford University Libraries.

If Woodworth hardly could have overlooked Coover, could he still have missed his 1907 article? Severe critique in a prominent journal is not likely to escape the attention of those struck by it. Neither is it very likely that Woodworth had simply forgotten the incident once he began writing *Experimental Psychology*. Mimeographed versions of Woodworth's text were available from 1909, and the 1912 version consisted of 285 pages (Winston, 1990). Moreover, elsewhere in the book Woodworth overtly expresses annoyance with Coover's 1912 PhD on formal discipline: "Do these results indicate transfer? ... The results have been sometimes cited as evidence for positive transfer effect ... but the reader must use his own judgment" (Woodworth, 1938, p. 198). To all appearances Woodworth did first learn about group comparison as a vital condition from Coover and Angell. However, acknowledging them in *Experimental Psychology* would have meant highlighting an article that seriously contested the experiments that first brought him fame.

Generations of American students were taught how to do research with the help of Woodworth's "Columbia Bible" (Winston, 1990). Assuming the accuracy of these suppositions on Coover and Angell's relegation to oblivion, the present story clearly demonstrates that lack of recognition does not always testify to little impact.

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Professional (in)visibility, gender and the writing of theory: Post-war trends and cases from social psychology (1)

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As a follow up the paper presented at the Durham ESHHS meeting (Lubek, Ferris, Bauer, Samotowka, Edmonds, Ross & Thoms-Chesley, 1998) Febbraro, we again explore some of the mechanisms of invisibilization of the ideas and practices of men and women social psychologists (often with effects on their professional careers). Previous work has focussed upon those invisibilizations linked to limitations and biases in such professional databases as Psychological Abstracts, or Social Sciences Citation Index, e.g., how the career-long citations of women may be systematically undercounted while a matched sample of men are over-counted, and recently Newberry et al., 1999, have presented additional evidence. Some invisibilization processes reflect voluntary and not-so-voluntary publication and avoidance strategies by authors themselves (Febbraro, 1997; Ferris et al., 1999) . In this paper we look at how the choice of writing theoretical, non-empirical papers , for men and women, may make their authors less visible to their sub-discipline, especially in its mainstream journals. A series of 300 theoretical articles (1949-1997) are evaluated for their degree of adherence to an individualistic approach, their political tendency, and their connectedness to either pure theoretical or real-world concerns; we summarize the consequences for visibility of such choices about theorizing by both men and by women.

And since some invisibilization is directly the result of active editorial gatekeeping, we examine a recent case study (Lubek, 1999) which involves a discussion of the comparative abstracting policies of two ground-breaking journals in the 1990s, Theory & Psychology (liberal policy) and Feminism & Psychology (conservative). We will compare the consequences of these contrasting policies for the Visibility of the men and women authors contributing to each; we shall compare these results with the effects of the abstracting policies of various mainstream journals. Some revisibilization strategies are also discussed.

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The prediction paradigm vs. the encounter paradigm: David van Lennep: a case in the Americanization of personnel selection

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Abstract:

This paper is part of a project on the Americanization of North-West European psychology. It focusses on the resistance of European psycho-diagnosticians - exemplified by the phenomenology-based Utrecht psychologist David van Lennep, head of the Netherlands' largest testing firm - to give up the 'Encounter paradigm' followed in their practice, and to exchange it in the course of the 1950's for the psychometric 'prediction paradigm' as used in American personnel selection practice (prediction of a pragmatic criterion of success). It is shown how van Lennep, eager to legitimate the testing practice used in his institute towards the scientific community, gradually and reluctantly surrendered to the new paradigm, not sufficiently realizing the fundamental gap between both approaches. In the conclusion of the paper, the paradigm shift is placed against the background of the decreasing reliance on personal bonds in industrial relations in Europe after World War II, and the ascension of a pragmatic society in which functional relationships prevail.

As is generally known, there has been in the period after World War II a drastic reorientation in Northwest European psychology from a largely phenomenological Continental style of thinking to an Anglo-American empirical-analytical style of thinking. In personnel selection and vocational guidance this reorientation manifested itself in the shift from consultancy on the basis of intuitive 'clinical' understanding to prediction of success on the basis of statistical formulae. In this paper I shall call the old, European based approach the encounter paradigm, and the new Anglo-American practice of statistically validating tests against on-the-job criteria of success the prediction paradigm. The term paradigm should be understood here as paradigm of practice, as defined by me elsewhere (van Strien, 1978, 1992). The prediction paradigm came up in America in the early 1920's, but conquered the European continent only after World War II, as part of the general 'American colonization' of European psychology (van Strien, 1997). The aim of this paper is to contribute to a better insight into the micro-historical processes which have led to this reorientation. I shall use applied psychology in the Netherlands as a case. At the ESHHS meeting at Durham (1998) I have shown how the selection centres of the Army - re-modelled after the example of the NATO partners - served as a bridgehead in the introduction of Anglo-American assessment methods in the Netherlands (viz. van Strien, 1998b). Once several of the brilliant young psychologists who had fulfilled their military obligations at one of the Army Centres, were appointed professor, the prediction paradigm soon became also current standard at the universities. It fitted in with the empirical-analytical views of the authoritative methodologist Adriaan de Groot at the University of Amsterdam.

In the present paper I shall focus on the rearguard of the transformation process, by analyzing the way one of the champions of the *geisteswissenschaftliche* 'Continental' approach, David van Lennep, gradually and reluctantly, surrendered to the prediction paradigm (at least as the sole basis of an advice), after first having dismissed it. This will give us a view of the presuppositions and social conditions of both the old intuitive clinical diagnostic style, and the new pragmatic predictivism. I shall try to show that, compared to the old one, the newly adopted approach is not more sound as such, and superior in a methodological sense, but that the old approach perfectly fitted the demands of the time in which it was developed, but was abandoned only because the social and economical context in which personnel selection operated changed, and also because the pressure of new academic standards made itself felt.

The setting on which I focus in this paper is the Netherlands Foundation of Psychotechnics (Nederlandse Stichting voor Psychotechnick) at Utrecht, headed by David van Lennep, perhaps the most intriguing practising psychologist of his era. The 'Stichting' (which still exists, though under another name) is one of the oldest bureaus for personnel selection and vocational counselling in the Netherlands (van Strien, 1998c). Its history goes back to 1925, the year in which the municipality of Utrecht established a bureau for vocational guidance under the directorship of van Lennep. To be able to further extend its services, the bureau took on the corporate personality of a private foundation in 1927, and only a few years later it had grown out to the largest private testing firm in the Netherlands with a nation-wide clientele. Yet there was not a single psychologist among its staff. Van Lennep, born 1896 as a descendant of the lower Dutch gentry, was a theologian with some reading knowledge of psychology, who had trained at a psychiatric clinic for some months, and had made a study

trip to Germany upon his appointment at the Utrecht Bureau. He distanced himself from the elementaristic approach that still prevailed in psychotechnics, particularly in the selection for rank-and-file jobs. Instead, he took inspiration from the new *gestalt* movement, and focused on testing for higher level positions. To this purpose he devised several tests, of which the *Four Picture Test* (FPT) (1930), a projective test, is the most well-known. Once the Manual was translated into English after World War II, the FPT found its way all over the world, from the United States to Australia, from Russia to South Africa, and from Brazil to Korea. In their autobiographic memoirs both Jean-Paul Sartre (1964, p. 194-195) and Simone de Beauvoir (1963, p. 132-133) remember their being confronted with some projective pictures by van Lennep during their visit to Utrecht in 1946.

In the first decade after its founding, the Foundation of Psychotechnics followed an eclectic diagnostic approach, that took its main inspiration from the holistic characterology that after World War I superseded the elementaristic approach in the whole German-speaking world. To get as full a picture of the subject as possible, a great variety of diagnostic devices was used, including graphology, physiognomy, and even astrology. In the course of the thirties and forties, however, van Lennep developed a psychodiagnostic view of his own, which clearly showed the marks of the existential phenomenology of these days. The ensuing approach to personnel selection and vocational guidance can be designated as the *encounter paradigm*.

Characteristic of the encounter paradigm is that the testing situation is used by the psychologist to immerse him/herself in the testee, after having first internalized the requirements of the job in the same way. Bringing 'the right man in the right place' thus becomes a matter of diagnostic imagination. To better be able to perform this act, Van Lennep and his associates often practised the job for some time themselves. When, for instance, the Dutch Airways *KLM* commissioned the Foundation with the selection of its pilots, van Lennep got his flying licence himself, to get the 'feel' of navigating a plane. For the same reason van Lennep deliberately recruited his staff from a variety of professions. But first of all, through their empathic understanding, the diagnosticians themselves were proclaimed to be 'their own best instrument'. They first had to imagine the job-requirements as a *gestalt*, then form a diagnostic *gestalt* of the applicant, and subsequently to estimate how far both *gestalt's* fitted in with one another. On the basis of a conference in which the views of various members of the diagnostic staff were exchanged, an extensive report was written. Being himself a fairly good portrait painter, van Lennep liked to characterize the psychological report as a 'portrait

in words'.

To give a picture of the style of reporting of the Foundation of Psychotechnics, I have appended the only English language report I could find to this paper. It is taken from a paper (written in rather poor English) at a conference in London in 1935. Typical of this style of reporting is, that it not only deals with capacities, but also with the person's personal lifestyle: no slave of his passions, not bothering about self-defence, a gentle irony, a detached attitude, etc.. Though the reports always ended in an advice, they were meant to enable the client to make a personal decision. In the case of vocational guidance the clients were the testee's themselves (or their parents), who had to decide on a study or vocation. In the case of personnel selection the reports were addressed to the directors or personnel managers of the firms concerned. They too, after having immersed themselves in the candidates, were expected to take a personal decision. Not infrequently, reports contained hints for the best way of dealing with problematic aspects of a person's character. Though the Foundation 'verified' its advices against job-success, the first that counted was, what I elsewhere have dubbed, the 'diagnostic truth' (van Strien, 1998a). The notion of validity in its present sense, still played hardly any role. Verification meant asking employers after one or two years whether the performance of the persons they had hired was satisfactory. In 80 to 90 percent of the cases the answer was positive, which gave psychologists (and their clientele) the assurance that they were doing well.

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Van Lennep used the stagnation caused by World War II to deepen his diagnostic views. When at the establishment of the Dutch psychological association NIPP in 1938 membership had been denied to him, he of course felt deeply hurt, and, to gain academic respectability, entered upon a psychology study at Utrecht University. After having acquired his degree in 1941, he started working on a dissertation on *projective phenomena*, and after its completion in 1948 he was appointed extraordinary professor of applied psychology at Utrecht University in 1949, a position that he combined with his directorate of the Foundation of Psychotechnics. At that time Utrecht witnessed the heyday of the Utrecht 'Existentialist-phenomenological School' around the charismatic Frederik Buytendijk (Dehue, 1995). Exploring someone's life-world, and meeting him or her as a person, became leading principals here. In his inaugural address van Lennep joined in, by distinguishing three methodical approaches in diagnosing a person: (1) by outer *measurement* of variables, (2) by *observing* the subject's behaviour during the test, and (3) by a person-to-person existential encounter. He did not reject the first approach, but deemed the third one necessary for a full
understanding of the other. The Four Picture Test, which I briefly mentioned already, served as one of the instruments to such an encounter. the pictures. done in watercolours, showed in a vague way one or more persons in various situations: alone at home, or in the street, in a dual interaction, and as part of a group. The testee had to compose a story which emcompassed all of these four existential situations.

As the Anglo-American psychometric approach to personnel selection gradually began to spread in Europe after World War II, van Lennep found himself in a quandary. On the one hand he was strongly convinced that clinical intuition was the only way to say something of real significance about someone's assets and liabilities in view of a future job. He abhorred the 'shotgun empiricism' of making predictions from a handful of tests that proved to be valid as predictors of some success-criterion, without real insight into the *why*. On the other hand, he felt that his testing practices would only find favour in the eyes of the scientific community - to which as a university professor he now belonged himself - when he could boast the same validities as others did. So he had one of his staff trained as a researcher, and accepted that this man applied for methodological advice to no one else than van Lennep's scientific antipode: the Amsterdam professor Adriaan de Groot, the champion of empirical-analytical science in the Netherlands. The staff charged with turning out an advice were asked to give ratings, and after the candidate had been employed for some time, the correlation of these ratings with the judgement of the supervisors was determined.

Though critical, van Lennep became also fascinated by the new advances that had been made in America both in science and technology. He made a study trip to America in 1948, and again in 1952, under a grant from the Rockefeller Foundation. He was one of the first to introduce Rogers' *non-directive counselling*, and brought a *wire recorder* with him for taping therapy sessions. Of course, he visited colleagues working with projective tests, Henry Murray for instance, but also representatives of the psychometric camp. The projective psychologist Shneidman, who followed him around during his three days in Los Angeles in 1952, found van Lennep:

... an amazing person. He has a breadth of interests, a catholicity of taste, and a lack of fear of unconventional psychological ideas not often seen in these parts. (One day he lectured to Bruno Klopfer's class at UCLA about some intriguing, but esoteric, concepts of projective phenomena and the next day he talked to J.P. Guilford at the University of Southern California, across town, about factor loadings, stanine scores, and pilot selection).

(Schneidman, 1952, quoted in Bryant, 1990, p. 30)

Van Lennep's decision to put his diagnostics to the test of validation research meant, in fact, a capitulation for the rival paradigm: predictivism! The need to periodically report on the results of his research on the FPT to the Rockefeller Foundation. that generously sponsored his investigations, pushed him in the same direction. Once the early verification practice of the Foundation was transformed into modern style validation research, the results were devastating indeed. At a NIPP conference in 1957 van Lennep declared intuition to be an unsuitable basis for making predictions. And in 1961, in front of his baffled co-workers, he renounced with admirable intellectual honesty most principles which had governed the Foundation's practice for many decades (van Lennep, 1962/1971). By that time most other testing bureau's had already performed the same paradigm shift.

Van Lennep's tragedy was that he did not realize that by engaging in research on the predictive power of his testing results, he in fact forsaked his original encounter paradigm. Studies like P.E. Meehl's Clinical vs. Statistical Prediction (1954) had presented the issue as solely a contest between two methods of predicting future behaviour: by using our heads or by using a formula. In fact, however, two completely disparate intellectual traditions were at stake: the geisteswissenschaftliche and the empirical-analytical. And along with it two completely different relationships with the human subject were involved. The encounter paradigm did not aim at predicting the future, but at offering insights into the capacities and personality of a person, that could be used for designing their future. The person was addressed not as an object to be measured, but as a free and responsible being who is able to transcend the contingencies of the situation! (van Strien, 1992). The encounter paradigm belongs to a society in which personal relationships sill prevail, the prediction paradigm to am individualistic, pragmatic society, in which functional relationships, and bureaucratic rules prevail - a society that came about in America first. Even when van Lennep had not surrendered to the new paradigm, social forces would have compelled his Foundation to move with the times. And now, indeed, a quarter of a century after van Lennep's retreat, the Foundation belongs, under a new name, to the vanguard of modern psychometric assessment.

Specimen of a psychological report of van Lennep (1935)

.... Age 30. Tested with a view to his fitness as a representative of an engineering works.

His intelligence is very good, but it is almost exclusively adjusted to the subject. He only distinguishes ideas and big outlines, which he expounds lucidly, but not always sufficiently concisely. Practice and detail interest him but little. His philosophical attitude comes to the fore everywhere and makes itself felt in his intellect as well in his character.

In technical work his strong point is to thresh out a problem and let his mind approach it from every angle; when he has to put it into practice he is slow, and his thoughts are elsewhere.

With juridical questions we find the same thing - he will cling to a preconceived idea, however little it will foster in practice. However, there is no question of fanatism; he simply does not see there are practical objections.

In matters of organization one single notion interests him, because for some reason or other it has attracted his attention (e.g. division of labour, employés' participation, etc.). The immediate organizing of details he prefers to leave to others.

He will easily learn a foreign language, the difficulties mentioned above hardly coming into play here.

He has a good 'feeling' for materials, but more insight in organic than in mechanical connection. Here too he shows that he is rather a 'medical' than a 'technical' man.

He is not a 'business man'; business for him is the service of mankind; making a profit out of it is of minor importance and definitively unpleasant.

His personal difficulties no longer cramp him; he is not a slave to his passions. He does not bother about self-defence; his irony is gentle; it never hurts. He stands above men and observes them - without caring a great deal about them. He takes in a lot and realizes what people moves to certain acts, but he does not begin to apply this knowledge to his own advantage.

He is detached and through this attitude will easily influence others. He is never irritating, and he will hardly ever be irritated himself. His honesty and reliability are 100 A.1.

Here follows [writes van Lennep] a practical conclusion with a view to the special circumstances of the business [not specified for reasons of confidentiality].

Quoted from: D.J. van Lennep, D.J. & T. Kuiper, (1935). methods of selection of personnel suitable for higher andministrative positions. Paper Sixth International Congress fir Scientific Management, London, July 1935. Proceedings, pp. 47-52 (Appendix).

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Session

"RUSSIAN PSYCHOLOGY"

Abstracts

A. V. Brushlinsky, L. Mecacci, V. M. Rusalov, R. Topel

The subject-activity paradigm in psychological sciences

A. V. Brushlinsky (Russian Academy of Sciences, Institute of Psychology, Moscow)

Humans and their minds do not constitute two systems. but. integral systems in which the subject is the basis of all kinds of mental qualities. Humans interact with reality on different levels simultaneously: reactions (related to stimuli). actions (related to objects), deeds (related to other subjects) and so on. According to S.L.Rubinstein and his scientific school, humans and their minds are first of all developed and revealed during practical activity (Tätigkeit) that is, the sensory-and-practical contacts of human infants with the reality at different levels of communication are primary, and speech is secondary, since it is formed on the basis of those contacts. Sign (speech) - centred approach (L.S. Vygotsky and his school) insufficiently takes this important implications for fact into account. This has computerization and psychotherapy.

The subject is humanity as a whole within which are to be found individuals and different groups. The original sociality and activity of the human individual as a subject, that is humans at the <u>highest</u> level of their activity, integrity, autonomy etc. follow from this fact. The alternative, internalization theory and sign-centred approach, based on such principles as "from the social to the individual", "from the external (only) to the internal" insufficiently take this fact into account.

It is extremely important in psychology to appreciate the <u>whole</u> variety of relations between humans and society, but not to reduce sociality only to the pressure of society on the individual. There is not only the influence of human society over its members, but also the influence of individual members over society itself. In the latter case the human individual clearly emerges as the subject.





Rubinshtein

Vygotsky



Nebylitsyn

Vygotsky and Eisenstein: Psychology and film perception

Luciano Mecacci (Department of Psychology, Florence, Italy)

Since the early diffusion of cinema, psychologists were directly interested in the mind mechanisms involved in film perception. An associationistic approach was proposed by H. Munsterberg, while a gestalt interpretation was given by R. Arnheim in the classical work *Film as Art* (1932).

The relationship between the Soviet historical-cultural school in the '20s and '30s and the film theory worked out by the director S. M. Eisenstein is less known. Eisenstein was a close friend of L. S. Vygotsky and A. R. Luria, and they co-worked for a research project on film psychology. Eisenstein's concepts of "sense synchronization" and "thought/image" are strictly related to Vygotsky's view of mind.

Particularly, Eisenstein tried to translate into the motion pictures the dynamics of inner processes of mind. A remarkable example is the "construction" carried out in the montage of film *Aleksandr Nevsky*: film frames (vision) and Prokofyev's music (hearing) should be linked together dependently on the higher cognitive processes of attention and expectancy of the observer. Eye movements could reveal, according to Russian director, the dynamics of attention or mind during the perception of film sequences. The technique for recording eye movements were poorly developed in that time, and Eisenstein's hypothesis could not be verified. Current research (V. Tosi, E. Pasquali, L. Mecacci, *Int. J. Neuroscience*, 1997) has shown that eye movements represent a really powerful instrument for investigating film perception.



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Fig. 1. Frames (vision), music (hearing), and mind dynamics in a sequence of film *Aleksandr Nevsky* by S. M. Eisenstein.

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Human individuality: Recent developments of Teplov-Nebylitsyn ideas

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The report shows how the ideas of Teplov and Nebylitsin are implemented in recent research into temperament. The main distinctive feature of their approach is that temperament is an entity of formal-dynamic features of individuality, which are stable in time and consistent across situations, involuntary and spontaneous, reflect energy-dynamic aspects of behavior, do not depend on the concrete current goals of behavior, manifest themselves in all the spheres of human behavior, are 'life indices' of the general properties of the nervous system, genetically determined. It is noted that Nebylitsin clearly understood that the identification of general properties and of the formal-dynamic characteristics of human individuality depended on the use of Anokhin's 4-component model of behavior. Proceeding from this model, it was concluded that the number of basic non-specific general behavioral (temperamental, in our sense) properties should also be four. 1. Ergonicity (energy) which is linked to 'afferent synthesis' component; 2. Plasticity which is related the 'decision-making' component; 3. Speed which is a manifestation of the 'execution' component and 4. emotionality, (or emotional threshold) which is a generalized property of the sensitivity mechanism to 'feedback', or to 'discrepancies' between the real results of behavior and the results anticipated.

The report also show how the Nebylitsin's original views on the possibility of regarding temperament in different spheres of human behavior. Developing ideas of Nebylitsin we could identify three aspects (or spheres) of temperament: intellectual, psychomotor and communicative. So, the number of dimensions of temperament can be theoretically 12.

Using the rules of psychometrics we constructed a special questionnaire which permitted an assessment of the formal-dynamic properties of human individuality (or FDPTQ). The given psychometric tool is a valid test questionnaire with normative data. The method permits an assessment of not only of separate formal-dynamic properties, but also an assessment of stable structures of temperament (indices and types) reflecting a different degree of their integration in intellectual, psychomotor and communicative spheres of human behavior.

At present the FDPTI method is widely used in psychological and clinical research as well as for professional selection purposes.

On S. L. Rubisntein's philosophical-psychological concept

Renate Topel (Otto-von-Guericke-Universität, Magdeburg)

Within the framework of the history of psychology it is undoubtedly Rubinstein who deserves the most attention among the greats of Soviet psychology. Of particular relevance are his papers on the methodology of psychology, his crtiticism of the dominant trends and schools in psychology, as well as his views on well-known philosophers and psychologists, among others Marx, Cohen, Spranger, Helmholtz and Setschenov.

Several far-reaching events come to light in the course of Rubinstein's scientific career. A philosophy course in Germany preceded his preoccupation with Marx's sociophilosphical and political-economic work following the Russian Revolution. The shattering reproach of cosmopolitanism and his undeserved dismissal from all incumbent posts at Moscow University in 1949 almost inevitably followed the public support as state prize-winner (1942) for the first edition of his book, "The Foundations of General Psychology", and his election as corresponding member of the Academy of Science (1943). In spite of this stroke of fate, Rubinstein worked untiringly on his most important philosophical-psychological work "Being and Consciousness," which didn't appear until 1957. His work "Man and the world" which was posthumously published, is increasingly becoming more renowned in philosophical and psychological discussions as a result of the worldwide acknowledgement of Rubinstein's outstanding scientific work on the occasion of his 100th birthday.

Rubinstein turned his attention to methodological questions of philosophy while he was studying in Marburg. He deals with the question of an objective research methodology in the humanities, equivalent to scientific procedures in his dissertation "A Study of the problem of Method", which was critically evaluated by Natorp and Cohen. However, the adoption of Laplace's one-sided, linear mathematical-physical causality model seems to him to be completly unsuitable for humanistic questions. Action characteristic to the subject is an essential causal factor not to be underestimated. The active subject comes upon a great variety of interactive manifestations from the material and mental world through its' actions, its' cognitive activity, its' forming of its' relations to other subjects, and has an effect on them forming them according to its' own needs and ideas. Thus, even in the early years of Rubinstein's scholarly career, the subject -object- dialectic became the starting point with which indeterminate and subjective concepts could be effectively challenged.

First however, Rubinstein countered the self-activity of the subject as an essential feature of German ideational philosophy. In spite of his father's close friendship to Plechanov, Marxism prior to the Russian Revolution was for him merely a social movement and not recognised as a philosophical trend. It is only later that its` highly epistemological value is disclosed to him in Marx's early writing, which includes the problem of personality. Rubinstein's papers "Problems of psychology in the work of Karl Marx" (1934) and " On the philosophical foundation of Psychology - the early

work of Karl Marx and problems in Psychology" (1958/59) testify to the struggle, which lasted for over two decades, for a new philosophical-psychological theoretical outline orientated towards Marxist methodology. Three theses from the young Marx appear to Rubinstein to be of outstanding relevance for the analysis of ideational phenomena: first, Marx emphasises the important role of theoretical and practical activity in the forming of the personality for its` mental development. Secondly, he stresses that the world, shaped through the activity of the subject, reacts upon the development of the senses, of the consciousness; thirdly he considers the human senses, the human consciousness to be a product of history.

The principle of the unity of consciousness and activity becomes the basic axiom of Rubinstein's philosophical-psychological concept. Basically, it implies that man, anchored in his socio-historical process of development, concretises his essential powers through activity, and with that simultaneously develops his personality. The revision of contemporary psychology, in accordance with this theoretical outline, appears to Rubinstein to be a task which needs to be resolved urgently. The phenotype of a current crisis in psychology becomes apparent to him, as to other leading experts in the field, at the end of the 1920's, although psychology itself was only established as an independent science at the end of the 19th century. However, in no way did he share Bühler and Pongratz's standpoint that the divergent diversity of psychological trends and methodical trends were aspects of a unitary psychology. He thought it necessary to redefine the objectives of psychology anew and to introduce a basic discussion regarding the essence of psychic manifestations.

As a consequence, Rubinstein rejects the contrast of physical and psychic phenomena which, based on Descartes and Locke, had dominated for centuries, and which also expresses itself concretely in the conflict between Wundt's psychology of consciousness and behaviorism. In his opinion, the concept of personality, particular to the psychology of consciousness, which is based on psychological manifestations and circumstances and which disregards the subject's relations to the external world to a great extent, supports a damaging psychologism and subjectivism in the humanities. On the other hand, he also believes that behaviorism reduces the diversity and wealth of the external world to simple stimulus - response relations; man's self-creativity, his value relations which mould his essence culturally, aesthetically and ethically remain With the dialectic of subject and object, Rubinstein supports an unnoticed. interactional view, which opposes both of the traditional trends and which has, in the meantime, become generally accepted in modern personality and development theories. He states with reference to Setschenov: "I. M. Setschenov saw the greatest shortcomings in the old traditional ideational psychology, in that it took the psyche out of its' context with material reality and turned it into a separate sphere instead of treating it as the mental element of a unitary process, which begins with the influence of reality on humans and ends with activity" (1979c, p. 107).

The subject-object interdependency requires a new way of looking at the tiresome problem of causality in psychology. Formerly, there were two predominant alternatives to the solving of this problem, both unsatisfactory; the intra-psychic subjectivist understanding of causality in the psychology of consciousness and the reductionist-mechanistic research strategy of behaviorism; Rubinstein's "deterministic principle", which was later to become the cardinal principle of Soviet psychology, was an essential step forwards in the struggle to establish psychology as an objective science. The principle of determinism, formulated by Rubinstein, implies that external causes affect inner conditions brokenly. He concretised this thesis for teachers and educationalists at the Soviet Union Conference of Educational Sciences in 1941: the effect of an educationalist's pedagogical measures is never the result of this isolated activity, but, is, at the same time, always dependent on the entire development of the child, on its' journey through life up to that particular time (Bruschlinski 1989b). Generally speaking, this means that the active subject doesn't chiefly form his relations to the world on the basis of situational influences, but instead responds to them subject to fixed biopsychic structures and functions, which have been formed in the ontogenesis and phylogenesis. The brain is integrated into this dialectical process as an organ of reflectional activity. Rubinstein counters critics of this position, who reduce the causality concept to direct dependency relations, with the argument that the psyche or consciousness isn't any kind of reflection or representation of the external world. The action of a subject is an inalienable factor in the humanistic definition of causality; the conscious act of will and the creativity of man's creative powers impressively bear witness to this.

According to Rubinstein, consciousness isn't an imaginary phenomenon which is given a priori. The unity of consciousness and action enables it to grasp the essence and function of the consciousness in the life of humans. As a result of these analyses, several features come to light:

- The personality with the help of the consciousness experiences and forms its` relations to the world.

- The consciousness has a cognitive and regulatory function, insofar as the representation of reality, generalised and particular to the consciousness, enables it to plan actions independently from the concrete situation, from time and space, and to anticipate the aims, means and results of activities.

- The consciouness is the unity of the subjective and the objective. External actualities and happenings are perceived, assimilated and integrated into the field of consciousness subject to individually emotional, motivational and cognitive qualities of the personality.

- The consciousness is capable of self-reflection and self-regulation. Man is capable of shaping his own relations to the natural and social environment in a single-minded and self-critical way.

Rubinstein considers the real personality as being responsible for the process of consciousness. Its' genesis takes place subject to concrete-historical living conditions; with that he rejects all of man's abstract determinations. However, while the epistomological aspect of the subject-object-dialectic dominates in his book "Being and Consciousness", whereby the consciousness appears as the subjective reflection of objective reality, this one-sided rational view is revoked in favour of a more comprehensive ontological point of view in his essay "Man and the World". This is determined three-dimensionally and includes man, who shapes the world, as well as his cognitive relations and subject-subject relations.

It is with the analysis of being that the selection of living conditions, important for the development of the personality, comes about. In contrast to the behaviorist reduction of the world to stimulus configurations, which are externally isolated from one another, Rubinstein incorporates the diverse interacting levels of being into his considerations. Besides physical and biological actualities, man, in his diverse cultural-historical and current relations, is for him an integral part of the structure of being. In this sense the aesthetic, the ontology of the sensory being, is for Rubinstein just as much of scientific interest as ethics, the ontology of moral relations. Intensive study of these specific areas of being gives Rubinstein rise to describe not merely the interaction between man and the world as a relation of action, but to include the modality of emotional appearance. With this reversion to classical components of experience he overcomes the one-sided orientation to activity, which is widely accepted throughout Soviet psychology.

The remarkable orientation towards practical activity, indeed often towards the aspect of usefulness, in no way corresponds to Rubinstein's ideas about the development of the personality, just as little as fatalistic views or theoretical concepts of maturation levels can be attributed to him. In his opinion, the nascent personality gradually extends its' own sphere of living through active contact with the environment; it reorganises being as a subject. Originally subjected to predominantly external compulsions, it acquires increasingly practical freedom and mental freedom and changes its' extrinsic motivation to intrinsic motivation.

On the other hand, Rubinsteins' thought is alien to a deductive derivation of psychic manifestations from the self-consciousness, from the ego, the crucial element of Fichtian philosophy. The personality directs its' mental activity towards the demands of life. Individual modifications, but also functionality and development of psychic processes are subjected to this dependency relation. An integral-integrated structure of consciousness of the personality is formed as a result, which is not merely aware of the world, but is also capable of reflecting over its' own position in the world with confidence.

Rubinstein devotes two chapters from his book "Man and the World", published posthumously, to human relations, and in accordance with this, it is not the cognitive factors but factors of drive, needs, opinions, interests and ideals which primarily determine the structure of personality. The ability of man, his abilities and talents, no less significant, are trained and used in order to turn the objectives in life into reality. In the course and outcome of a life-history, the question is asked in the end as to the stabile individual views and tendencies which form the character. Thus, accordingly, Rubinstein believes that three questions suffice in order to obtain information about the essence of man: "What does man want? What can he do? What is he?". In striving to find the meaning of life, it is not, however, material privileges which take precedence, but real moral relations, since the individual only becomes a human in his interrelations to others. Rubinstein is against all forms of ethical individualism and against all activities which harm the dignity of fellow creatures.

Among the classics of Marxian psychology, it is Rubinstein's theoretical views on the personality, which deserve especial attention because of their sound humanistic conception of the world. Unlike the then widespread hypothesis of an identity of social and individual interests, Rubinstein, during his life, refers to the active role of the subject in the moulding of man-environment relations. Thus, Rubinstein believes that the personality, as a result of a life-long social process of activity and learning, is capable of reshaping the complex, natural and social being in accordance with its' own ideas and in this way of gaining personal freedom. This interactive and integrated conception of the personality is Rubinstein's response to the one-sideness of contemporary consciousness, psychoanalytical and behavioural concepts.

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ESHHS European Society for the History of the Human Sciences

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The Florence Laboratorio di Psicologia Sperimentale, 1903-1938

(by Simonetta Gori-Savellini, Dipartimento di Psicologia, Firenze)

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The first autonomous "Laboratorio di Psicologia" was founded in 1903 in Florence. The Laboratorio was instituted by Francesco De Sarlo (1864-1937). Physician and philosopher, De Sarlo passed from a positivistic approach in the investigation of psychological processes to a phenomenological phase, represented particularly in his main book *I dati dell'esperienza psichica* [Data of psychological experience] (1903).





Francesco De Sarlo and the title page of his main book.

Professor of Theoretical Philosophy at the University of Florence from 1900 to 1934, De Sarlo promoted both the philosophical and the psychological investigation. As Eugenio Garin remembered, one reading especially recommended by De Sarlo for his students was Brentano's *Psychologie vom empirischen Standpunkte*. Brentano lived in Italy from 1895 to 1915, firstly in Florence (where he participated to the debates held at the Biblioteca Filosofica) and then in Palermo.

Several psychologists worked in the Laboratorio di Psicologia, notably Antonio Aliotta, Enzo Bonaventura and Renata Calabresi. Aliotta wrote a methodological book (*La misura in psicologia sperimentale* [Measuring in Experimental Psychology]) (1905) and Calabresi devoted herself to the psychology of time perception.



Contributions (1905-1907) from the Laboratorio di Psicologia.

The most brilliant member of this group of psychologists was Enzo Bonaventura (1891-1948), who gave important contributions in different fields of research (introspection, visual illusions, attention, etc.), but especially in the investigation of space and time perception. He wrote also an exhaustive and balanced book on psychoanalysis.

In 1925 the 4th National Congress of Psychology was held in Florence. In that occasion the Florentine group read several papers, and the Laboratorio di Psicologia appeared as a promising center of psychological research. However the political situation arrested this trend: because of his antifascist positions De Sarlo was isolated, and Bonaventura - after the race laws in 1938 - emigrated (he died tragically in Palestina during a gunfight).





Enzo Bonaventura and the title page of his book on time psychology.

Instruments for Psychological Research: The Collection at the Dipartimento di Psicologia, Firenze

From S. Gori-Savellini, La misura del tempo nel laboratorio di psicologia sperimentale, in Misura d'uomo. Strumenti, teorie e pratiche dell'antropometria e della psicologia sperimentale tra '800 e '900. Firenze: Giunti, 1986.

PREMESSA

Vengono sintetizzati nelle pagine che seguono dati relativi a strumenti conservati nel Laboratorio di Psicologia Sperimentale dell'Università di Firenze. Per la loro descrizione si fa riferimento, prevalentemente, a scritti di V. Berrettoni («Come s'istituisce un Laboratorio di psicologia sperimentale». Firenze, 1906. e «Nozioni di antropometria ed elementi di psicologia sperimentale». Sassari, 1910) e di N. Vaschide, É. Toulouse e H. Pieron («Technique de psychologie experimentelle», Paris, 1904).

Le schede iniziali si riferiscono ai primi strumenti utilizzati per la scansione dei tempi in laboratorio e per la rilevazione di fenomeni psicologici elementari (1-4). Seguono poi schede su strumenti per lo studio della percezione visiva (5-10) e della percezione acustica (11-14). Sono ricordati apparecchi per lo studio di problemi di percezione tattile e illusione ponderale (15-18), vi sono quindi schede su strumenti per lo studio della sensibilità ai sapori e agli odori (19-20).

Segue un gruppo di schede che si riferisce a strumenti elementari per la registrazione di dati in laboratorio (21-25), sono successivamente ricordati gli strumenti di Marey per la registrazione di variazioni in parti diverse del corpo umano (26-29). Ci sono inoltre schede dedicate ad apparecchi per valutare modificazioni della mano e dei muscoli (30-31).

Vengono poi esposti dati relativi a strumenti per la rilevazione del modo di organizzarsi di fronte allo sforzo e alla fatica (32-33). Si ricordano infine strumenti per lo studio dei fenomeni mnestici (34-35). Le schede conclusive contengono indicazioni su apparecchi relativi allo studio dei movimenti volontari e involontari, dei processi consapevoli e inconsapevoli (26-33).

Alcuni degli strumenti presentati sono opera originale per lo studio di fenomeni psicologici, altri sono sussidi indispensabili per la scansione temporale e la registrazione delle indagini svolte in laboratorio.

*Le dimensioni degli strumenti sono espresse in centimetri.

TAL L

III. catalogo



III.1. GRANDE CRONOGRAFO DI WUNDT Serve per registrare i tempi di fenomeni psicologici rilevati da altri strumenti.

È composto da un apparecchio motore formato da un insieme di ingranaggi per la trasmissione del movimento, da una manovella per portare in alto i pesi applicati e da un freno collocato sull'alberino del livellatore. Il sistema di registrazione è formato da un cilindro ruotante su cui viene collocata della carta aifumicata posta su un albero che poggia sui dischi dei due supporti e dal lato sinistro termina su una traversa di metallo che si inserisce nell'ingranaggio dell'apparecchio motore. Sulla parte superiore del carrello vi è un diapason collocato orizzontalmente che viene eccitato elettromagneticamente e produce 500 vibrazioni al secondo che vengono iscritte sulla carta alfumicata. L'apparecchio è dotato di tre sistemi a molla indipendenti che, collegati a vari strumenti, ne iscrivono le vibrazioni sulla carta.

Materiali: base in legno; strumento in ottone e metallo cromato. Dimensioni: lung. 43; larg. 30,5; alt. 156,5.

Costruito da: Zimmermann, Leipzig, 1903. Ist. Psic. FI, inv. n. 121.

103 |

III. catalogo

sulla quale scorre una cordicella legata ad un peso. Questi regola la caduta di uno schermo il quale, a sua volta, provoca lo sganciarsi di una tendina che scopre l'immagine da apprendere. È un apparecchio a orologeria, mosso da un peso; la parte superiore è costituita da un meccanismo provvisto di due quadranti. L'ago del quadrante superiore compie un giro di 360° in un decimo di secondo. L'ago del



III.2. TACHISTOSCOPIO DI WUNDT

Serve per misurare le capacità individuali di memorizzare nel minore tempo possibile uno o più stimoli.

Lo strumento è provvisto di una base in legno sulla quale è montata una torretta in metallo al cui vertice è posta una carrucola metallica Materiali: legno e ferro. Dimensioni: lung. 40,2; larg. 32; alt. 110. Ist. Psic. FI, inv. n. 1.

III.3. CRONOSCOPIO DI HIPP

Viene utilizzato insieme ad altri strumenti che intervengono nella parte specifica dell'esperienza stimolatoria della quale consente di evidenziare la durata. quadrante inferiore compie un giro completo in dieci secondi. L'apparecchio viene azionato e fermato da due cordicelle. La base è costruita in legno, la parte superiore in legno e metallo.

Materiali: legno e metallo. Dimensioni: lung. 21; larg. 26; alt. 52,5. Costruito da: E. Zimmermann, Leipzig. Ist. Psic. FI, inv. n. 18.

III.4. CRONOMETRO D'ARSONVAL Strumento fondalmentale nelle ricerche sul tempo. Registra centesimi di secondo.

È portatile, di facile utilizzazione, provvisto di produci-stimoli e tasto di reazione, può essere collegato con altri apparecchi secondo la necessità di vari esperimenti. Il cronometro, poggiato su una base in legno provvista di un cassettino, è formato da un quadrante metallico e da un meccanismo ad orologeria che consente alla lancetta di compiere un giro completo in un secondo. Sul retro del quadrante l'apparecchio è corredato da un meccanismo di arresto elettrico, collegabile ad elettrodi. Tali elettrodi, aprendo e chiudendo il circuito, consentono di rilevare con precisione i tempi di reazione dei soggetti studiati.

Materiali: legno, metallo e vetro. Dimensioni: lung. 18; larg. 17; alt. 23,3. Costruito da: Ch. Verdin, Paris, 1903. Ist. Psic. FI, inv. n. 19.

III.5 STROBOSCOPIO CON FIGURE (ZOOTROPIO)

Veniva usato per ricerche sulla percezione del movimento.

È costituito da un supporto in legno e metallo sul quale viene fissato il coperchio in cartone della scatola che contiene strisce di immagini. Tali strisce, anch'esse in cartone stampato e rappresentanti immagini in successione, vengono inserite all'interno del coperchio della scatola. Le strisce presentano fessure verticali a distanza costante, attraverso le quali, facendo ruotare il coperchio, è possibile vedere le figure in movimento.

Materiali: legno e cartone. Dimensioni: lung. 24; larg. 24; alt. 19. Costruito da: Anschutz, Lissa (Posen). Ist. Psic. FI, inv. n. 59.

III.6.1. STEREOSCOPIO DI BREWSTER

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Serve a studiare le percezioni tridimensionali.

Utilizzando immagini bidimensionali appositamente inserite nell'apparecchio, si richiama la loro percezione tridimensionale. L'apparecchio consta di un visore atto a riconvergere le

due immagini monoculari, preventivamente separate tramite uno schermo perpendicolare all'asse facciale. La riconversione delle immagini è realizzata mediante due lenti prismatiche convesse, atte anche a ingrandire.

III. catalogo

Materiali: alluminio e legno. Dimensioni: lung. 31,5; larg. 12,4; alt. 10. Ist. Psic. FI, senza numero di inventario.



Materiali: legno, vetro e specchio. Dimensioni: lung. 15; larg. 18,5; alt. 11,3. Ist. Psic. FI, inv. n. 50.



III.6.2. Una versione successiva dello stereoscopio di Brewster adotta lo stesso principio realizzando la separazione oculare, non con uno schermo, ma con un sistema binoculare, variabile nella focale. Ad una estremità, dietro uno schermo semitrasparente, si collocano le immagini. L'illuminazione è realizzata

III.7. FÓTOMETRO DI BUNSEN

105 |

Serve a misurare l'intensità di una fonte luminosa.

È composto da una struttura metallica nel cui interno sono due specchi, collocati ad angolo. Essi riflettono la luce che proviene da unafonte luminosa che può scorrere su un'asta metallica posta orizzontalmente. Nel sistema ottico è collocato un diaframma rettangolare di carta speciale che mantiene al centro una macchia d'olio. La sorgente luminosa che si vuole misurare viene collocata in modo da farla riflettere sulla macchia d'olio. La comparazione dell'intensità luminosa viene fatta con una lampada campione all'acetato d'amile.

Materiali: ghisa, ottone e metallo. Dimensioni: lung. 32; larg. 18; alt. 33,5. Costruito da: Max Kohl, Chemnitz. Ist. Psic. FI, inv. n. 45.

III.8.1. TAUMATROPIO A CONO ANAMORFICO

Utilizzato per la conoscenza di fenomeni della percezione visiva.

Collocato al centro di cartoncini rappresentanti immagini deformate, consente a chi guarda dall'alto la visione di oggetti di forma normale.

Materiali: metallo. Accessori: cartoncini rappresentanti immagini deformate di vario colore. Dimensioni: lung. 5; larg. 5; alt. 5. Ist. Psic. FI, inv. n. 57.



III.8.2. SPECCHIETTO CILINDRICO ANAMORFICO

Analogo al precedente consentiva lo studio della percezione visiva deformata.

Materiali: vetro e legno. Accessori: cartoncini con immagini deformate. Dimensioni: lung. 5,4; larg. 5,4; alt. 16. Ist. Psic. FI, inv. n. 58.

III. catalogo

III.9. SERIE DI OCCHI CON IRIDI VA-RIAMENTE COLORATE

Materiale organizzato anche a fini didattici per sottolineare le possibili differenze di un organo di senso.

Tre scatole foderate di velluto contengono cento o cinquanta riproduzioni di occhi in ceramica e vetro. Alcuni, nel tempo, sono stati in parte danneggiati.

Materiali: vetro variamente colorato e ceramica. Dimensioni: lung. 32; larg. 30; alt. 3 (scatola più grande).

Ist. Psic. FI, i numeri di inventario (133-135) corrispondono probabilmente ad un inventario dell'Istituto di Fisiologia.

III.10. APPARECCHIO DI ROTAZIONE

Serve per evidenziare fenomeni visivi connessi con la percezione cromatica.

L'apparecchio poggia su una base in legno e comprende un meccanismo di rotazione messo in funzione da una manovella. Sul disco rotante sono dipinti i colori dell'iride. Aumentando la velocità diminuisce la capacità di discriminare i vari settori colorati, in modo che alla fine non sono più percepiti individualmente e il disco appare bianco.

Materiali: metallo, cartone e legno. Dimensioni: lung. 29; larg. 19,7; alt. 31,7. Costruito da: Max Kohl, Chemnitz. Ist. Psic. FI, inv. n. 47.

III.11. DIAPASON

SU CASSE DI RISONANZA Viene usato per esperienze di percezioni uditive diverse.

Ne sono presenti quattro esemplari che producono suoni differenti. L'elemento vibrante, costruito in metallo, poggia su un perno di legno che va a fissarsi sulla cassa di risonanza, anch'essa costruita in legno.

Materiali: metallo e legno.

Dimensioni:

C, (Do centrale), lung. 31; larg. 11,5; alt. 29,8. C, (Do ottava successiva), lung. 15,8; larg. 9; alt. 21.5.

E' (Mi ottava centrale), lung. 26,9; larg. 11,8; alt. 28,6.

G' (Sol ottava centrale), lung. 20,9; larg. 9; alt. 23,8.

Costruito da: Max Kohl, Chemnitz. Ist. Psic. FI, inv. n. 34.



| 106



III.12. FISCHIETTO DI GALTON Utilizzato per misurare il limite massimo di percettibilità dei suoni, ne può produrre di varia intensità.

È costituito da un fischio a frequenza variabile e da una pompetta in gomma che vi immette aria, provocandone il suono. La variazione della frequenza è ottenuta tramite un meccanismo a vite che varia l'ampiezza della camera di risonanza.

Materiali: metallo e gomma. Dimensioni: lung. 4; larg. 4; alt. 11. Ist. Psic. FI, n. 40.

III.13. PENDOLO ACUSTICO DI WUNDT Serve per lo studio delle differenze individuali nella percezione acustica.

III. catalogo

È formato da una base in legno, su cui poggia una colonna in ottone. Nella parte superiore di questa sono fissate due forcelle metalliche che sorreggono due pendoli di legno la cui asta è avvolta in una guaina di feltro che serve ad evitare le vibrazioni. Queste vengono trasmesse da due sfere pure in legno, che battono sulla parte metallica di un cubo che circonda la base della colonna. Due supporti di legno, collocati dietro alla colonna, sostengono due aste circolari graduate. Con due cursori ad altezza variabile, si portano i due pendoli fino ai fermi e si lasciano, poi, cadere simultaneamente. Questi producono dei suoni le cui frequenze sono direttamente proporzionali all'altezza del cursore.

Materiali: legno (feltro e metallo. Dimensioni: lung. 97; larg. 20; alt. 88. Costruito da: Spindler e Hoyer, Göttingen. Ist. Psic. FI, inv. n. 33.

III.14. APPARECCHIO A CADUTA DI WUNDT

Tende a misurare, mediante la breve interruzione di uno stimolo acustico, variazioni minime nei tempi di reazione.

È costituito da una base in legno su cui è





collocata una piattaforma che sorregge un contenitore cilindrico imbottito da feltro. La piattaforma è fissata con una cerniera dalla parte posteriore; ha due piedini metallici ponteggiati che chiudono un circuito elettrico. Una molla la mantiene sollevata in modo da non chiudere il circuito. Questo viene chiuso quando una sfera metallica è lasciata cadere nel contenitore dall'apertura di due ganasce poste su un supporto in legno applicato ad altezza variabile ad una colonnina anch'essa in legno. Un suono, alimentato elettricamente da pile, si interrompe quando le ganasce che sorreggono la pallina vengono manualmente aperte. Riprende quando quest'ultima, giungendo sul fondo, fa chiudere il circuito abbassando la piattaforma. È possibile così valutare intervalli variabili in un suono.

Materiali: legno e metallo nichelato. Dimensioni: lung. 26,5; larg. 17; alt. 67,5. Costruito da: Zimmermann, Leipzig. Ist. Psic. FI, inv. n. 8.

III.15. STEREO-ESTESIOMETRO DI TOULOUSE-VASCHIDE

Era utilizzato per lo studio delle differenze individuali nella percezione tattile.

In una scatola è contenuta una serie di piastrine, uguali per larghezza, diverse per altezza e lunghezza. Queste servivano anche per valutare la sensibilità nella percezione di pressioni diverse per entità.

Materiali: ottone.

Dimensioni: lung. 12; larg. 18; alt. 3. (All'interno del contenitore piastrine di varie dimensioni). Costruito da: G. Fontaine, Paris. Ist. Psic. FI, inv. n. 132.

Bibliografia: N. Vaschide, E. Toulouse e H. Pieron, Technique de psychologie experimentelle, Paris, 1904.

III.16. TERMO-ESTESIOMETRO DI TOULOUSE-VASCHIDE

Serve per determinare varie «soglie» di sensibilità termica.

È costituito da una base in legno circolare dalla quale si innalzano dei piani metallici che dividono l'apparecchio in tre scompartimenti corrispondenti al caldo, al freddo e alla temperatura costante. Nella parte superiore di ogni scompartimento è fissato un recipiente di rame nel quale è introdotta una bottiglietta che può essere riscaldata o raffreddata. Ogni bottiglietta contiene acqua distillata ed è chiusa da un tappo di gomma che presenta due fori, per i quali passano rispettivamente un termometro e un contagocce.

Materiali: legno, vetro e rame. Dimensioni: lung. 30; larg. 30; alt. 36. Costruito da: G. Fontaine, Paris. Ist. Psic. FI, inv. n. 133.

III. catalogo

III.17. COMPASSO DI WEBER MODELLO DEL GRIESBACH

Questo strumento che deriva da ricerche di Weber rende possibile misurare la soglia di diseriminazione tattile di due punte variandone la distanza, serve inoltre per avere indicazioni sulla soglia del dolore nelle diverse zone della pelle.

Lo strumento permette di effettuare misurazioni mediante due diversi tipi di movimento delle parti che lo compongono. La misurazione della soglia di percezione di due punte è effettuata mediante la variazione della distanza delle due punte medesime; tale variazione è resa possibile facendo scorrere la punta mobile sulla parte fissa dello strumento. Dati sulla percezione dello stimolo esercitato dalle due punte vengono ottenuti variando la pressione di queste sulla superficie cutanea. Il movimento delle due punte è ammortizzato da due molle. La pressione esercitata sulla cute, e di conseguenza sulle molle, è riportata sopra una scala impressa sul supporto delle punte stesse.

Materiali: ottone cromato. Dimensioni: lung. 15; larg. 12; alt. 4,5. Costruito da: I. Brandlei, Basel. Ist. Psic. FI, inv. n. 9. [esposto in sez. IV. 1.¹⁵]



III.18. PESI DI FECHNER

Servono per studiare le illusioni nelle valutazioni ponderali.

I pesi sono costruiti in legno e appaiono identici; invece il loro peso è notevolmente diverso, perché in uno dei due è inserita una pesante massa di piombo. Durante l'esperimento l'operatore solleva quello più pesante e poi quello più leggero, come se avessero lo stesso peso. Immediatamente dopo si richiede al soggetto, proponendoglieli in ordine di peso decrescente, di sollevarli. Viene così evidenziata la costruzione mentale rispetto al compito e l'effetto dell'aspettativa.

Materiali: legno e piombo. Dimensioni: lung. 9,9; larg. 9,9; alt. 13,5. Ist. Psic. FI, inv. n. 129.

| 108





III.19. GEUSI-ESTESIOMETRO DI TOULOUSE-VASCHIDE Fornisce materiale per valutare la sensibilità gustativa.

È formato da una cassetta di legno contenente un gran numero di bottigliette di vetro, uguali, disposte su due panieri di rame di grandezza differente. Il paniere piccolo contiene le bottigliette per la ricognizione dei saporiodori. L'altro contiene le soluzioni per differenziare le quattro specie di sapori (salato, dolce, amaro e acido) e una serie di soluzioni salate. Si suggeriva (in Berrettoni, 1906) «per il dolce: miele e melassa / per l'amaro: caffè forte, infuso di tè non astringente / per l'acido: sale da cucina». Ogni bottiglietta è provvista di un turacciolo di gomma attraverso il quale passa un contagocce.

Materiali: vetro, legno e metallo. Dimensioni: lung. 70; larg. 34; alt. 16. Costruito da: G. Fontaine, Paris. Ist. Psic. FI, inv. n. 156.



III.20. DOPPIO OLFATTOMETRO DI ZWAARDEMAKER

Serve per determinare le soglie di sensazione, di discriminazione, di contrasto e compensazione degli odori.

In due cilindri metallici mobili, la cui distanza dalle narici è regolabile e misurabile tramite due scale centimetrate, vengono posti cilindri di sostanze odorose. Elenco di «cilindri cavi di sostanza odorosa: legno di cedro / gomma di benzoino / paraffina / cera / balsamo del tolù / assa fetida / cuoio di Russia / legno di rose / gomma elastica». Gli odori giungono alle narici tramite due tubicini in vetro che si immettono nelle narici stesse. Il tutto è sostenuto da un pannello in legno che funge anche da schermo per impedire al soggetto la visione dei cilindri.

Materiali: legno e metallo. Dimensioni: lung. 24; larg. 24,8; alt. 36,5. Ist. Psic. FI, inv. n. 134.

III.21. AFFUMICATORE

Lo strumento serviva per annerire la carta da avvolgere intorno al cimografo.

Si compone di due parti: la parte inferiore costruita in ottone è costituita da un cappuccio mobile che permette, tramite un tubo di gomma, di convogliare il fumo sulla carta soffiando all'interno del tubo stesso. Probabile opera artigianale di un tecnico dell'Università di Firenze (E. Guelfi).

Materiali: ottone e gomma. Dimensioni: lung. 10; larg. 16; alt. 21. Ist. Psic. FI, senza numero d'inventario.



21



III.22. APPARECCHIO DI MEUMANN

Operando su due interruttori collocati a distanza variabile questo strumento consente di interrompere circuiti elettrici con intervalli regolabili distribuiti su 360°.

Nelle ricerche di psicologia sperimentale è associato con altri strumenti, ad esempio il tachistoscopio di Enzo Bonaventura e il cimografo di Ludwig-Baltzar. È costituito da un treppiedi in ghisa con un foro centrale e una chiavetta di arresto. È dotato di vari interruttori, montati su materiale isolante, che possono essere fissati sul bordo esterno di un cerchio a distanze variabili e vengono aperti da una sbarra centrale rotante orizzontalmente azionata dallo sperimentatore.

Materiali: metallo. Dimensioni: lung. 31; larg. 29; alt. 33. Ist. Psic. FI, inv. n. 10.

III.23. SLITTA DI DU BOIS-REYMOND

Può avere varie funzioni, in particolar modo viene usata in esperimenti di 'eccitazione' dell'organo visivo e di stimolazione elettrica di punti dolorifici.

Questo strumento è costituito da una base di legno ripiegabile nella sua metà. Sui due lati sono collocati due righelli di cui l'anteriore è centimetrato. Entrambi hanno il lato dalla . parte interna sagomato a sghembo per consentire lo scorrimento di una bobina (circuito secondario). La bobina ha un foro centrale dentro il quale può entrare un'altra bobina (circuito primario). Questo a sua volta contiene un nucleo di ferro dolce, mobile e centimetrato (dodici centimetri). Lo scorrimento della bobina e l'avanzamento del nucleo avvengono manualmente. Il sostegno della bobina a circuito primario è fissato alla base dell'apparecchio. Dietro il sostegno vi è un meccanismo a bilanciere azionato da due elettrocalamite alimentate da una pila a corrente continua. L'ampiezza di oscillazione del bilanciere è regolabile da una vite micrometrica e la frequenza dell'oscillazione può essere controllata da un cursore a sfera. La corrente da utilizza-



III. catalogo

re viene erogata tramite i due serrafilo collocati nella parte anteriore della bobina mobile.

Materiali: metallo, legno e bachelite. Dimensioni: lung. 108,4; larg. 8,7; alt. 23,2. Ist. Psic. F1, inv. n. 124. [cfr. IV. 4.⁵]



III.24. CIMOGRAFO

DI LUDWIG-BALTZAR Un cimografo, in un esperimento, serve a registrare i dati forniti da un apparecchio più specifico.

Di questo strumento esistono varie versioni che si differenziano, più per la forma, che per lo scopo. Il cimografo di Baltzar è mosso da un meccanismo di carica a molla. Tramite un sistema di dischi e ingranaggi è possibile variare la velocità di rotazione e di caduta del tamburo che funge da supporto per la carta affumicata sulla quale la penna scrivente traccia i dati.

Materiali: ottone e ferro. Dimensioni: lung. 40; larg. 20; alt. 52,5. Costruito da: E. Zimmermann, Leipzig, 1903. Ist. Psic. FI, inv. n. 144. [cfr. IV. 4.³⁴]

III.25. METRONOMO DI MÄLZEL CON REGISTRATORE PNEUMATICO

È utilizzato come strumento di registrazione della durata di atti psichici.

È costituito da un metronomo in legno e metallo a cui è applicato un sistema registratore pneumatico, costituito da due membrane circolari agganciate a un supporto metallico. Le capsule sono poste a uguale distanza dal piano verticale centrale dell'apparecchio in modo che il pendolo, nelle sue oscillazioni, batte alternativamente sulle due membrane. Una penna scrivente, in comunicazione con le membrane, segna sulla carta affumicata di un cimografo un tracciato che varierà con la velocità del pendolo stesso.

Materiali: legno e metallo. Dimensioni: lung. 17; larg. 18; alt. 23,3. Ist. Psic. FI, inv. n. 143.

III.26. CAPSULA SCRIVENTE DI MAREY

Serve a registrare il movimento dell'organo che si intende studiare, riportandone, graficamente, a distanza, le variazioni.

È formata da una capsula in metallo chiusa a una estremità da una membrana di caucciù che forma una camera d'aria comunicante con l'esterno tramite un tubicino.

Nel centro della membrana è applicato un disco di metallo collegato ad una leva scriven-





Coglie i movimenti respiratori del torace mediante il tamburo a leva che li registra su carta affumicata.



te che si muove su un piano verticale per impulsi che le vengono trasmessi dalla membrana stessa.

Materiali: metallo e membrana di caucciù. Dimensioni: diametro 5; alt. 1. Costruito da: Ch. Verdin, Paris. Ist. Psic. FI, inv. n. 88.

III.27. CARDIOGRAFO DI MAREY Tende a cogliere e trascrivere il battito cardiaco che si rende evidente alla superficie della pelle in corrispondenza del cuore.

L'apparecchio si applica al torace mediante una cintura e si appende al collo del soggetto

\$ * tramite un nastro. Lo strumento si collega ad una penna scrivente tramite un tubicino di gomma interrotto a metà da una valvola metallica (*clarinette*) che serve, quando occorre, a ripristinare uguaglianza di pressione tra l'interno del sistema e l'aria dell'ambiente.

Materiali: metallo e gomma. Dimensioni: lung. 7,9; larg. 7,9; alt. 10,3. Costruito da: Zimmermann, Leipzig. Ist. Psic. FI, inv. n. 85. [esposto in sez. IV. 4.³³]

III.28. PNEUMOGRAFO SEMPLICE DI MAREY Lo strumento è fornito di un nastro che permette di mantenerlo appeso al collo del soggetto in modo da evitare lo spostamento del punto di applicazione. È provvisto inoltre di una cintura inestensibile applicata a due alette in modo da formare un sistema rigido intorno al torace. Vengono così registrati i movimenti di espansione e contrazione della cassa toracica.

Materiali: caucciù e metallo. Dimensioni: lung. 7; larg. 11; alt. 5. Costruito da: Ch. Verdin, Paris. Ist. Psic. FI, inv. n. 86. [esposto in sez. IV. 4⁴]

111 |



III.29. SFIGMOGRAFO DELLA RADIALE DI MAREY Registra le variazioni della pressione del sangue nelle arterie secondo eventuali mutamenti del soggetto.

Lo sfigmografo della radiale preme sull'arteria mediante una molla di cui si può graduare la pressione con una vite micrometrica. La parete dell'arteria, compressa dalla molla, alzandosi e abbassandosi sposta la membrana dello



28



sfigmografo e attraverso il tubo di conduzione trasmette i movimenti ad una leva che li amplifica. Lo strumento viene legato al polso mediante un nastro.

Materiali: metallo e caucciu. Dimensioni: lung. 13; larg. 6; alt. 6,5. Costruito da: Ch. Verdin, Paris. Ist. Psic. FI, inv. n. 84. [cfr. IV. 3¹⁷]



III.30. GUANTO DI PATRIZI Tende ad evidenziare gli eventuali cambiamenti di volume della mano.

È una cavità di cartapesta con un volume di poco superiore a quello di un pugno medio chiuso e ha l'aspetto di un guanto grossolano. Vi si faceva introdurre la mano del soggetto studiato; quindi la cavità veniva chiusa con mastice. Il guanto diventava una sorta di camera d'aria che, mediante un tubicino, veniva collegata a tamburi scriventi.

Materiali: vetro e cartapesta. Dimensioni: lung. 17,5; larg. 10,8; alt. 10,5. Ist. Psic. FI, inv. n. 120. [esposto in sez. IV 1.³⁵]

| 112



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III.31. APPARECCHIO DI GOLDSCHEIDER Utilizzato per lo studio di sensazioni muscolari.

È costituito da un cuscinetto di velluto montato su un rettangolo di legno che porta una vite nel centro. Detta vite serve per fissare una piastrina metallica scorrevole che porta una scanalatura nel centro. Su uno dei suoi due bordi è collocato, mediante una vite, un settore circolare snodato che, nella parte superiore, è graduato nei due sensi, da 0 a 20. Al centro porta un perno a cui è fissata un'asticella che termina con una ruota libera. Alla ruota è fissato un indice che scorre sulla parte graduata. All'indice è attaccato un contrappeso che imprime il movimento anche in spostamenti minimi. Lo strumento viene applicato, col cuscinetto, alla zona del sistema muscolare di cui si vogliono rilevare le reazioni.

Materiali: metallo e legno. Dimensioni: lung. 10; larg. 10; alt. 21. Ist. Psic. FI, inv. n. 149.

III.32. DINAMOMETRO GRANDE A MOLLA

Serve a misurare la forza che è possibile esercitare con entrambe le mani.

Mediante un meccanismo a molla, la forza esercitata sullo strumento viene riportata da un sistema di lancette su una scala graduata in chilogrammi. Nelle sue linee essenziali questo esemplare può essere considerato il prototipo di strumenti successivi che giungeranno fino alla registrazione automatica.

III. catalogo

Materiali: ferro e ottone. Dimensioni: lung. 24; larg. 34; alt. 6,5. Ist. Psic. FI, inv. n. 81. [cfr. IV. 1²⁷]

III.33. ERGOGRAFO DI MOSSO Ideato da Angelo Mosso tende, attraverso la rilevazione di movimenti volontari, a rendere graficamente il fenomeno dell'affaticamento.

È costituito da un sostegno e da una parte scrivente; il primo consente di immobilizzare il braccio in modo che possano essere mossi i soli muscoli estensori e flessori del dito medio, destro o sinistro. L'avambraccio viene adagiato in posizione supina: due morse imbottite abbracciano il polso e lo immobilizzano, due altre fermano l'avambraccio nella zona del gomito. L'indice e l'anulare vengono introdotti in due ditali rigidi il cui fondo può venir spostato secondo la diversa lunghezza delle dita dei soggetti. Il medio viene introdotto in un ditale articolato, ed è questo dito che deve compiere un lavoro sollevando un peso determinato legato all'estremità di una cordicella che parte dal ditale. Le contrazioni e le estensioni si compiono secondo il ritmo dato da un metronomo. La parte scrivente consiste in una slitta scorrevole e in una cordicella alla cui estremità è fissato il peso da sollevare. Contraendo il dito la cordicella si sposta verso il soggetto, sposta la slitta e la penna scrivente a questa collegata. La penna scrivente con l'escursione corrispondente alla contrazione e al rilassamento del dito medio segna la traccia del lavoro compiuto.



Materiali: ferro e ottone. Dimensioni: lung. 50,5; larg. 24; alt. 19. Costruito da: Ch. Verdin, Paris. Ist. Psic. FI, inv. n. 82. Bibliografia: A. Mosso, *La fatica*, Milano, 1891 [cfr. IV. 1.³⁰]

III.34. APPARECCHIO DI WIRTH Progettato da Wirth per lo studio della memoria ritentiva immediata.



È dotato di un meccanismo che, azionato da un contrappeso, consente il movimento di un cilindro su cui viene collocata una striscia di carta di lunghezza variabile, dove sono rappresentati segni, sillabe ecc.. Una finestrella di apertura regolabile, collocata su un pannello posto anteriormente al rullo, consente la percezione degli elementi-stimolo. Mediante interruttore e elettrocalamite si provoca lo scorrimento del cilindro a scatti e a velocità variabile.

Materiali: ferro, ottone e legno. Dimensioni: lung. 40; larg. 47; alt. 47,7. Costruito da: Spindler e Hover, Göttingen. Ist. Psic. FI, inv. n. 6.

III.35. MNEMOMETRO

DI RANSCHBURG Serve alla presentazione di materiale visivo, verbale o no, utilizzato per lo studio della memoria.

È formato da un meccanismo elettrico di rotazione a scatti. È possibile così la temporizzazione degli scatti in decimi di secondo. Fornito di un'ampia fessura consente solo la presentazione di stimoli verbali scritti su un disco di carta. Le parti elettromeccaniche dello strumento sono contenute in una scatola di legno a inclinazione variabile con una finestra laterale che permette di visualizzare, ad apparecchio chiuso, i tempi di esposizione.



Sul retro sono visibili le contattiere per il collegamento all'alimentatore.

36

Materiali: legno e metallo. Dimensioni: lung. 24; larg. 24; alt. 10,5. Costruito da: Zimmermann, Leipzig. Ist. Psic. FI, inv. n. 22.

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III. catalogo

III.36. APPARECCHIO DI DELABARRE

Progettato per studiare i movimenti involontari di un soggetto sottoposto a vari tipi di stimoli.

Può essere pure impiegato per la misurazione della frequenza e dell'ampiezza del tremito della mano.

Il soggetto introduce un dito nell'apposita capsula sostenuta da due fili preventivamente posti in tensione. Gli stessi fili trasmettono, mediante un sistema di carrucole, il movimento ad un insieme di penne scriventi che consentono di registrarlo sulla carta affumicata di un cimografo. Le penne scriventi sono due, è quindi possibile ottenere indicazioni e dati solo per due direzioni del movimento del dito.

Materiali: ferro e ottone. Dimensioni: lung. 9; larg. 28; alt. 45. Costruito da: Ch. Verdin, Paris. Ist. Psic. FI, inv. n. 123.



III. catalogo





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111.37. TACHISTOSCOPIO DI BENUSSI

Vittorio Benussi ha ideato questo strumento per esperimenti collettivi al fine di rilevare le differenze individuali nell'ambito della percezione visiva.

È composto da una struttura metallica che sostiene un disco girevole di alluminio del diametro di 20 cm. Il movimento di oscillazione del disco è determinato da pesi variabili che vengono applicati al disco stesso. A 90º dal peso, c'è una feritoia. L'ampiezza dell'apertura è resa variabile da due piastrine mobili. Dietro il disco è posto un sistema di lenti di un apparato di proiezione in modo che il punto focale del proiettore cada sulla zona del disco dove si trova l'apertura. Il disco si regola tirando una cordicella che fa scattare la molla di arresto. Il disco, sbloccato, compie un'oscillazione di 280° e viene trattenuto da un freno orizzontale che lo riporta nella posizione iniziale.

Materiali: metallo e corda.

Dimensioni: lung. 34; larg. 12,5; alt. 38. Prodotto artigianalmente nel laboratorio di psicologia sperimentale dell'Università di Firenze. Ist. Psic. FI, senza numero di inventario. Bibliografia: Atti del V Congresso Internazionale di Psicologia, Roma, 1905. V. Berrettoni, Come si istituisce un laboratorio di psicologia sperimentale, Firenze, 1906.

III.38. DOPPIO TACHISTOSCOPIO A CADUTA DI BONAVENTURA

Ideato da Enzo Bonaventura (1924), tende a provocare nel soggetto due successivi atti di apprendimento attraverso la presentazione di due serie di stimoli. Consente di misurare il tempo che deve intercorrere tra due presentazioni di stimoli affinchè questi vengano percepiti in maniera corretta.

L'apparecchio è formato da due colonne verticali provviste di due scanalature (una anteriore e una posteriore) nelle quali scorrono due schermi sostenuti da un sistema di carrucole e cordoncini e da un'elettrocalamita che provoca la caduta dello schermo all'interruzione della corrente. Alla base delle colonne sono



III. catalogo

posti due cartellini, recanti le immagini da identificare, coperti da due tendine che verranno abbattute dagli schermi i quali, attraverso una fessura regolabile nella sua ampiezza (da 0 a 30 mm.), permettono di cogliere l'immagine rappresentata. Il primo a cadere è lo schermo anteriore il quale, dopo aver permesso di vedere il cartellino attraverso la fessura, lo abbatte mediante apposite punte, provocando poi la caduta dello schermo posteriore che consente di vedere il secondo cartelliπο.

Lo strumento ha una funzione fondamentale nelle ricerche sulla misura della durata dell'atto di apprendimento. La trasformazione che conduce dal tachistoscopio di Wundt (v. scheda III. 2) a quello del Bonaventura, è determinata dalla necessità di ricerche sempre più precise sui tempi di apprendimento. Si giunge, col nuovo modello di tachistoscopio, a misurare l'intervallo minimo necessario per apprendere due gruppi di elementi senza interferenze o conflitti. Gli esperimenti svolti da Bonaventura con questo strumento tendono a cogliere pure le variabili implicate nella funzione selettiva dell'attenzione, evidenziando se si tratta di fattori dipendenti dalla personalità e dall'esperienza del soggetto o dalle caratteristiche dell'oggetto stesso (forma - colore - posizione...).

Materiali: ferro, ottone, legno e stoffa.

Accessori: serie di cartellini ideati da Bonaventura e dai collaboratori.

Dimensioni: lung. 35,6; larg. 24; alt. 101,5.

Ist. Psic. FI, inv. n. 2. Bibliografia: Atti del IV Congresso Nazionale di Psicologia, Firenze, 1926. / E. Bonaventura, Il metodo tachistoscopio. «Arch. It. di Psic.» 1928, pp. 180-204. / E. Bonaventura, Il problema psicolo-gico del tempo, Milano, 1929. / R. Calabresi, La determinazione del presente psichico, Firenze, 1930.

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