CHAPTER 4

PARIS, LEIPZIG, DANZIGER, AND BEYOND[1]

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INTRODUCTION

Kurt Danziger's analysis of the history of psychological research methodology, in his *Constructing the Subject* (1990)[2], is perhaps the most valuable, and certainly the most cited of his many contributions to the history of psychology. In this book Danziger shows convincingly that the presently prevailing research design of statistically comparing the outcomes of experimental and control groups—Danziger calls it the *(neo-)Galtonian model*—is only a latecomer. The pioneers of psychology used quite other methods to explore the "laws of consciousness". Danziger designates the most prominent early research models as the *Leipzig model* and the *Paris model*. He shows that it was not only new scientific insights, but social forces as well that led to the gradual replacement of the Leipzig model, as the leading research paradigm, by the neo-Galtonian model in the course of the first half of the 20th century.

Danziger's account of the social dynamics of psychological methodology not only has opened a new chapter in the historiography of psychology, but also offers a challenge to historians of psychology to critically examine his conclusions, and further explore developments beyond the reach of his own investigation. In this chapter I shall try to take up this challenge with an eye out for the relationship between subject matter and method. It will appear that the "natural science model", of which both the Leipzig model and the Paris model were offshoots, has persisted in various forms up to the present time, and also that new variants of the neo-Galtonian model have developed in the course of time. Often the new variants differ to such an extent from the original model that another name is in place. In line with Danziger's contextual approach, I shall also take account of the external "market forces" that are involved.

LEIPZIG AND BEFORE

19th Century Investigative Practice

Danziger traces the development of his research models back to its 19th century roots. Both the Leipzig model and the Paris model appear to have their origin in mid-19th century physiological and medical investigative practice as ushered in by the French physiologists François Magendie and his pupil Claude Bernard, and by pupils of the German physiologist Johannes Müller. Instead of only describing what they found, they started to study the organism by systematically intervening into the functions of various organs. The (human) body was approached here in the same way as other physical objects, and subjected to invasive probing. A well-chosen case or a crucial intervention sufficed here for establishing a new scientific insight or refuting a rival theory. Others—we could add—mapped the functions of the brain by systematically destroying or extirpating parts of it in animals and noting the effects on behavior. Paul Broca's autopsy of just one patient with a speech defect in 1861 served to convince the scientific world of the localization of the speech center at the base of the third frontal convolution of the left cerebral hemisphere.

Typical of all these investigations is that a single observation or experiment sufficed to demonstrate a particular effect: the action of a poison or a drug, or a deficiency resulting from extirpation of a part of the brain. Replications served only to corroborate or to refine the findings and to remove remaining doubts. This is similar to the practices of the natural sciences. In chemistry, for example, a small quantity of some physical substance suffices to establish its chemical properties, as the object of investigation serves as an arbitrary exemplar of nature in general. Henceforth I shall denote this practice as the *general natural science model.*

When physicians and psychiatrists in Paris started to explore the secrets and aberrations of the human mind, they used this general natural science model for the investigation of human subjects. The assumptions that guided their investigation remained the same, and one or a few subjects were sufficient for explaining the phenomena under study. The new element that was introduced in the *Paris model* (Charcot) was the use of (living) human subjects as subjects (the clinical study of hypnotic phenomena). At this point the *role relationship* between experimenter and subject becomes a crucial factor, and it is here that the roads from "Paris" and "Leipzig" divide. In the Paris model, as in all experiments mentioned so far, the role relationship was typically asymmetrical. The situation was defined in medical terms. In this respect the Paris model was molded on the basic assumptions that guided physiological and clinical research for most of the 19th century (*see* Bernard, 1865/1957). The medical context implied yet another difference: Whereas the Leipzig model was meant "to display universal processes that characterized all normal minds", the Paris model, as a typical clinical model, meant to "display the effects of an abnormal condition" (Danziger, p. 54). As an example Danziger refers to Charcot's demonstrations of hypnosis and grand hysteria.

Danziger does not follow up the further development of the Paris model, but spends only a few pages on it. He points to Binet's experiments on infants (in fact his daughters Madeleine and Alice; *see* Pollack & Brenner, 1969) as an extension of the Paris model, but does not mention Binet's experiments on great calculators and blindfold chess-masters (Binet & Henneguy, 1894). In both cases the social structure of the experimental situation was the same as with hypnotic subjects, but the context was no longer medical. In a later section I shall give some 20th century examples of non-clinical research in which elements of the Paris model can be traced.

Josef Breuer's and Sigmund Freud's clinical case studies of hysterical patients appear also to have be guided by the assumptions of the Paris model. The same is true for the many clinical case studies spawned by the various schools in psychotherapy that emerged in the 20th century. There was, however, a gradual change of emphasis. The "case" originally served primarily as an exemplar of a typical clinical picture, an abnormal variant of human nature, such as hysteria, schizophrenia and dementia. With time, it acquired more and more an interest in itself, the chief value of which was to demonstrate an author's therapeutic method: the general was exchanged for the typical, and structure for process.

In the *Leipzig model* (Wundt) the role of single subjects as data sources is preserved, but a new element is introduced: the *interchangeability* of the roles of experimenter and experimental subject. Prior to Wundt the symmetry between experimenter and subject can already be found in Franciscus Donders' epoch making reaction-time experiments (Donders, 1868/1969). He and his pupil de Jaager took turns as experimenter and subject. Nevertheless it is appropriate to attach Wundt's name to the new investigative style, because it was in his school that it became a new research tradition.

At first sight this arrangement seems to be merely a didactical device, ensuing from the principle of the *Einheit der Forschung und der Lehre*, and meant to acquaint students with both the role of experimenter and of subject and—once psychology had begun to emulate the natural sciences—to share the tedium of laboratory work. In fact there was a much more fundamental difference. In the Paris model the data consisted of responses and symptoms that were accessible to an outward observer. In the Leipzig model they consisted of the contents of the subject's own consciousness—phenomena that were accessible only to the subject's own introspection (in the sense of *innere Wahrnehmung*). The best way to do justice to this changed role was to make the subject a *co-researcher*. Making the research relationship symmetrical and the roles interchangeable was the natural next step. In view of the perils of a reliable inner perception, the role of subject (*Beobachter, observer*) even became the most important of both roles. To give a reliable account of one's perceptions required much experience. In the report of his investigations in the *Zeitschrift für Psychologie* the Dutch pioneer of psychology Gerard Heymans legitimated the use of his wife as his principal subject by assuring that she was "*eine sehr geübte Beobachterin*" (Heymans, 1887, p. 132). Elsewhere he defended working with a single subject with the argument that in an absolute sense the data may show personal differences, but that nonetheless the general laws of consciousness will appear in the *relationship* between the data (Heymans, 1896).

As an indication of the importance attached to the role of subject Danziger points to the convention of recording the names or initials of the subjects in experimental reports, as a kind of guarantee of trustworthiness. Not seldom the "great man" put himself in the role of subject. Wundt sometimes fulfilled this role in the experiments of his pupils, particularly in the early ones. In the Göttingen laboratory, the second in rank after the Leipzig laboratory, the *Ordinarius* G.E. Müller also volunteered in taking this role (Katz, 1934). Boring (1953) relates in his *History of introspection* that in Wundt's laboratory no observer who had performed less than 10.000 introspectively controlled reactions was deemed suitable to provide data for published research.[3] The role of experimenter was less fundamental, and consisted solely in administering the stimuli to which the subject had to react and in noting the responses.

The division of roles that became standard in the Leipzig laboratory also served to shield the subject's introspection from distorting influences. Of course, there were situations in which the subject was able to administer the stimuli himself. Fechner's psychophysical experiments are an early example, and Ebbinghaus' research on memory another. But as the task of the *observer* grew more complex, it became increasingly difficult for individuals to experiment on themselves without assistance. In Danziger's words: "The task of simultaneously manipulating the apparatus and playing the role of the possessor of a shielded private consciousness whose precise responses were the object of investigation was not easy, and was sometimes downright impossible." Precondition for a reliable response was to keep the responding individual "in ignorance of the precise short-term variations in the stimulus conditions to which he was to respond." (p. 30)

In the systematic introspection of Wundt's pupils the subject's role became even more important. We find this reflected in Titchener's *Manual of Laboratory Practice* where he admonishes his students: "Introspection is never easy; it becomes doubly difficult when one knows that *E* desires one to reach a predetermined result. Many experiments have been spoiled by some suggestion from *E*, and an answering complaisance on the part of *O*" (Titchener, 1906, p. xvii). This reads like a foreshadowing of Orne's (1962) *demand characteristics*. This meant that a large part of the responsibility for the reliability of the results rested on the shoulders of the experimenter. The ideal of mutual cooperation between experimenter and subject in their search for the secrets of the psyche is phrased in a striking way in the following quotation from Oswald Külpe:

> Thus . . . the typical human relationship between the experimenter and his subjects is the pivotal point of the functioning of the Psychological Institute. Both are contained in a relationship of trust. The social bonds of mutual consideration and self-efficacy and reciprocal understanding and friendly cooperation form the basis for scientific progress. . . . The psychological experiments foster not only our theoretical insight, but also our human worth. (quoted after Graumann, 1952, translation PvS)

In conclusion we can say that in Wundt's Leipzig laboratory nature became human nature, but that the principle of taking the data of a single subject as the basis for establishing general insights into human nature was retained. The research situation changed insofar the contents of the subject's own consciousness now became the central focus of study—phenomena that were accessible only to his or her own introspection. The subject became a co-researcher, and, consequently, the role relationship symmetrical. For the rest the basic premise of the natural science model was preserved: just as the subject of investigation is conceived there as an arbitrary specimen of the whole class to which it belongs, the human subject is conceived here, in the phrasing of Danziger, as an *exemplar of the generalized human mind*.

Other Early Variants of the General Natural Science Model

Before turning to the new methodological model that became prominent in the beginning of the 20th century, two special forms of investigative practice deserve our attention. The first of them is the *demonstrative experiment*. The experimental setting here serves solely to generate a phenomenon that speaks for itself, and the role of subject (or better of *observer*) is fulfilled by anybody who is willing to attend. Phenomenology and Gestalt psychology have become famous in this context, but the 19th century abounds already of other instances. Boring (1950, p. 602) characterizes it as "the convincing single demonstration of some observed generality", and expands on it as follows:

> Purkinje's watching the colors change at dawn is such an instance. Since phenomenology deals with immediate experience, its conclusions are instantaneous. They emerge at once and need not wait upon the results of calculations derived from measurements. Nor does a phenomenologist use statistics, since a frequency does not occur at a given instant and can not be immediately observed.

Of course one can dispute whether this investigative practice still can be regarded as experimentation or solely consists of the demonstration of particular effects. Albert Michotte, undisputedly the most famous 20th century Belgian psychologist, once remarked to a visitor "Don't take me for a Gestalt psychologist—I do real experiments on subjects!"[4] In fact, the investigative practice that is at stake here is not far removed from the demonstrations of patients as practiced within the French tradition. But we should keep in mind that prior to the demonstration there was the *discovery* of an interesting phenomenon. So I am inclined to regard the demonstrative experiment as a (weak) form of experimentation. Because of the potential interchangeability of the roles of "experimenter" and of "subject" we could speak of a hybrid somewhere between the "Leipzig model" and the "general natural science model".

The *case study* is another instance of an investigative practice rooted in the natural science model—at least in so far as the case serves as a basis for theory building. In their book on *Single case experimental designs* Barlow and Hersen (1984) point to Paul Broca's localization of the speech center as an early example of the case study methodology, that acquired such a prominent place in the social sciences later on. The authors also place Ivan Pavlov's later conditioning experiments on dogs in this single-case category, and perhaps we could also take Wolfgang Köhler's experiments on apes and on his infant daughter as an example. Particularly in sociology the case study has acquired a prominent place, but its significance for psychology should not be underestimated, as, for example, appears from Yin (1989), a book that no one less than Donald Campbell has provided with a foreword. Just as in the experiments mentioned above, the case serves as a basis for general theory building. It is not the place here to further expand on this method. For clarity's sake I only must warn that this use of single cases as a basis for general insights should not be confounded with the single-case N = 1 methodology to which I shall return later.

THE "TRIUMPH OF THE AGGREGATE"

The roots of this new model lay not in the laboratory but in differential psychological practice, as initiated by Francis Galton in his anthropometric studies in the 1880s. It differed in two respects from the Leipzig model. In Leipzig the roles of experimenter and subject were symmetrical and interchangeable, while in the new model the role relationship shows a clear hierarchy. And, still more importantly, single subjects serve no longer as data sources in their own right, but merely as anonymous representatives of some statistical class. To allow generalization subjects have to form a representative *sample* of the category they represent. What previously counted as error variance became now, as William Stern (1900) observed, a matter of interest in itself.

Galton too had his forerunners: Gustav Theodor Fechner for example also used group data. In his famous aesthetic investigations into the golden section he did this, and also in his attempt at polling the preference of visitors for one of both Madonnas of Holbein at an exhibition at the Dresden *Zwinger* in the early 1870s (Fechner, 1876). Woodworth (1950, p. 371) calls Fechner a forerunner of the census method, and Sprung and Sprung (1988) call him a precursor of modern sampling methodology. In his posthumous *Kollektivmaßlehre* (1897) Fechner has further elaborated this methodology. He cites here the famous Belgian statistician Adolphe Quetelet, whose probability theory and ideas on *l'homme moyen*—the average (or better: standard) man—also served as a source of inspiration to Galton. Thus, we can say that Fechner, in fact, belonged to the initiators of two experimental models. But it is true, that the model owes its success to the work of Galton.

With Galton measurement still served primarily to satisfy his inquisitiveness about the distribution of the physical and psychical characteristics among natural and social groups within the population, and subsequently as a basis for promulgating his eugenetic ideas. It was, as Danziger shows convincingly, the interest that educational administrators in America took in the method that gave the impetus to the further elaboration and diffusion of his model. Here the first step was made towards comparing *artificial* "treatment groups" with control groups: the "neo-Galtonian model". In this form the model had an enormous success, also outside the "primary market" of education. Under the Roosevelt administration it became, as Trudy Dehue (2001) has shown, the method *par excellence* of social science policy research. Here it gained the status of a watertight warrant of scientific objectivity and impartiality. In the first half of the 20th century the use of control groups, at first limited almost exclusively to applied research, gradually gained the upper hand in laboratory experimentation as well. The fact that the use of controls and randomization as such was part of experimental methodology already from the 1870s onward (Dehue, 1997, 2000) certainly has facilitated adoption of the control-group model.

Danziger concludes to an "eclipse of the Leipzig model" (p. 64), and a "triumph of the aggregate" (title of chapter 5). This triumph occurred first in the Anglo-American world and eventually conquered also the European continent. Developments in the second half of the 20th century confirm Danziger's findings. In most present-day research hypotheses are tested by examining the outcomes of some experimental or quasi-experimental design.

Particularly striking is the way the *relationship between the experimenter and the subject* further developed after the period covered by Danziger. Where Galton, Cattell, and other early differential psychologists still had a genuine interest in the natural or social group of which the subject was a representative, "neo-Galtonians" were neither interested in subjects nor in groups, but only in the significance of their experimental interventions. Present-day subjects are, to use the words of Klaus Holzkamp (1972), who examined the presuppositions of the classic and the modern research styles before Danziger, stripped of their individuality and transformed into abstract “norm-subjects”. In this situation it is no wonder that psychology became more and more the science of the behavior of undergraduates, paid either in money or in credit points. Subjects cheat, or respond to the demand characteristics of the situation. In our market oriented society the experimenter-subject relationship increasingly reflects, as Argyris (1968) has pointed out, the management-employee relationship in industry.

Outside the laboratory the Galtonian approach also made headway, notably in its original domain: *psychometrics*. In personality psychology there has been a strong current in favor of exempting the individual from quantification, and reserving the understanding of individuals in their uniqueness for the historian or the clinician. In this view the unique case is the very opposite of both the exemplary case of the Paris model and the Galtonian aggregate. Drawing on the German philosopher Wilhelm Windelband, Gordon Allport speaks here of the *idiographic* stance, as opposed to standard *nomological* science. The latter is not able to penetrate to the core of someone’s personality: *societia non est individuorum* (Allport, 1937, p. 3). The individual case is no longer conceived here as an exemplar of a general syndrome but as a matter of interest in its own right. In this sense the case study method became the favorite approach in clinical psychology in the first half of the 20th century (Bolgar, 1965).

Yet the clinical study of the individual case eventually became also a matter of Galtonian measurement. In modern psychometrics the subject is conceived simply as the “point of intersection of a number of quantitative variables” (Eysenck, 1952, p. 18). In reply to Allport, Eysenck, after having acknowledged that Professor Windelband is absolutely unique, adds: “So is my old shoe.” When practitioners make predictions about the future behavior of their clients, he argues, they do so by applying the general laws of behavior to the specific case—laws that are based on aggregate knowledge. *Factor analysis* developed as another branch of the Galton-tree.

Galtonian thinking has even penetrated into the *intra*subjective statistical study of individuals. An early example was William Stephenson (1935; 1953) who applied factor analysis in the study of dimensions of individual persons, the so-called *Q-technique*. Baldwin (1942) made a “personal structure analysis” of one woman on the basis of a statistic analysis of her personal letters. Osgood and Luria (1954) analyzed a case of multiple personality with the help of the *semantic differential* (“The three faces of Eve”). Monte Shapiro, Eysenck’s colleague at the London Maudsley Clinic, started a program directed at the “experimental study of single cases” (Shapiro, 1951; 1957). The subject’s behavior was seen here not as a sample of similar behavior in a wider population, but as a sample of his or her own behavior in similar situations. By studying this behavior *individual laws* were formulated from which the subject’s behavior in future situations was predicted. The findings were subsequently used as a basis for therapeutic interventions (see

Davidson & Costello, 1969, for examples of this N = 1 approach). Similar attempts were made by the Dutch clinical psychologist Johan Barendregt in the psychiatric clinic of the Wilhelmina Hospital of the University of Amsterdam in the 1960s (see Dehue, 1995, Ch. 5). Measurement refers here no longer to the statistical universe of human behavior in general but—to use a term coined by Saul Rosenzweig (1951)—to the *idioversum* of one person. In the same vein the Dutch methodologist Adriaan de Groot (1969) speaks of the "total field of the subject's behavior" within which lawful relationships are formulated on the basis of behavior samples.

Whereas the first N = 1 studies still suffered from growing pains, the experimental study of clinical cases increasingly gathered methodological power, particularly as non-random quasi-experimental designs became available (Campbell & Stanley, 1963; Cook & Campbell, 1979). Examples of sophisticated research in this line can be found in Chassan (1979), Barlow and Hersen (1884) and Kazdin (1982; 1992). Time-series analysis appears to be a favorite method here.

The inevitable conclusion from our extended review seems to be that the "triumph of the aggregate" has become even more complete in the second half of the twentieth century than Danziger suggests. As stated already, Danziger attributes the radical transformation of research practice to market forces: the young discipline of scientific psychology "had to contend with the divergent demands from an expert and a lay public." Within the first the "Leipzig" laboratory conventions prevailed. The wish to provide potential clients—initially primarily in the educational domain—with useful practical knowledge gave rise to a new Galtonian approach, in which individuals were ordered in terms of their standing in a statistical aggregate. In this quandary an approach in which a treatment or experimental group and a control group were compared offered itself as a compromise that enabled the discipline "to have it both ways" (Danziger, Ch. 5). This neo-Galtonian model became the new standard. Once it had conquered scientific psychology, the model was transferred back to differential practice, even, as we just saw, at the level of the single case.

LEIPZIG AND BEYOND

Nevertheless, this "triumph of the aggregate" does not necessary imply a *complete* eclipse of the older models. If it is true that the forces of the markets of educational and social administration had such a great influence on psychological methodology then we also must allow, on the one hand, for domains within the discipline where these forces were less influential, on the other hand, for new market forces that lead to new investigative models, or at least modifications of existing models. An exhaustive survey of these possibilities is, of course, not feasible within the scope of this chapter. I confine myself to the further development of "non-Galtonian" models beyond the period to which Danziger's study pertains.

Psychophysics and Psychophysiology

Kurt Danziger himself recognizes one field in which the Leipzig model is still pertinent to the object of investigation. On p. 70 of his book he writes: "In such areas as the psychology of sensation results from even a single subject can be claimed to have general significance, because of the presumed similarity of the underlying physiology in all human individuals." The "hardware" of our mental apparatus, or the "architecture" of our system, as present day experimental psychologists call it, is at stake here, and its characteristics can be ascertained from any healthy organism.

In fact this was the domain in which I first was alerted to the persistence of the Leipzig research style in contemporary experimental research (van Strien, 1993b, 1995, 1996). In a survey of doctoral dissertations at various Dutch universities I found that, indeed, in line with the international trend, the use of the Leipzig model did decrease after the Second World War. However, contrary to my expectations, about 50% of the key-figures of post-war experimental psychology in the Netherlands—all future professors—still followed it in their doctoral dissertation and in subsequent experimental studies. Among them were Willem Levelt, later to become director of the internationally renowned *Max Planck Institute of Psycholinguistics* in Nijmegen, and John Michon, who later became president of the Dutch *Psychonomic Society*, and first editor of an international *Handbook of Psychonomics.*

Levelt used only a few subjects in his dissertation *On binocular rivalry* (1965). In line with the Wundtian tradition he called them *observers* and identified them with their initials, one of them (W.L) being Levelt himself. In some of his investigations he used only two or three subjects. Michon stuck in his dissertation on *Timing in temporal tracking* (1967) still closer to the Wundtian tradition. His six subjects were, he assured the reader, well-trained; he identified them by initials and Michon himself (M) was one of them. In some experiments so-called "naive" subjects were used, but he threw out—just as Titchener did!—the results of those who appeared to be unreliable.

One explanation for the so much higher percentage in the Netherlands—even in the 1960s—could be an enduring European orientation in the Dutch laboratories. It is true, indeed, that after World War II it took a considerable time before Dutch psychologists were won over to the Anglo-American perspective (van Strien, 1997). Is the saying, ascribed to Heinrich Heine, right, then, that Holland is the best place to go at the Day of the Last Judgement, because everything happens there thirty years later? For the young researchers just mentioned this time-lag does not hold, however. And, what is more, depending on the subject matter, the (neo-) Galtonian approach was used by Dutch researchers just as often. This led me to investigate in how far the Leipzig practice of using one or only a few subjects as the knowledge base was—and perhaps still is—part of the standard practice in certain quarters of the scientific community, particularly in the domain of psychophysics and psychophysiology.

An inspection of representative publications showed that in nearly all investigations in this field the number of subjects was much smaller (less than ten or even five) than in studies of higher levels of human or animal behavior. Often they were identified by numbers or by their initials. Even where the subjects were anonymous and were treated as a statistical aggregate their number was usually small. In a relatively recent volume of the interdisciplinary journal *Vision Research*[5] over 80% of the studies dealing with human subjects followed the "Leipzig" canon. The number of subjects was small (rarely more than five); they usually were called "observers"; and in three-quarters of the cases they were identified by their initials. In the majority of the cases (60%) one or more of the authors served as observers. The subjects often were described as either "well trained" or "naive". The results were nearly always presented in graphs of figures for each separate subject, often accompanied with remarks on the status of his or her vision. We must conclude, that the research style in this field has changed little since the era of Helmholz and Hering, or, to cite a more recent example, since the days that Boring (1943) conducted his experiment on the moon illusion—also mainly based on the data of two long-standing observers!

Striking examples of small N research can also be found in the area of *motor control and skill acquisition*. In an authoritative reader in this field (Stelmach, 1978) the model is applied in seven out of the ten empirical articles. The same holds for applied psychophysical research, such as the *ergonomic* design of displays and research in motor control. Space technology is only one of the many examples. A related area in which evidence usually is derived from only a few subjects or cases is *brain-and-behavior research*: studies of, for instance, aphasia or amnesia in relation to brain injuries or other traumata. Thus, Damasio's (1994) study on emotion, reason and the human brain is based on only a few clinical cases of brain damage.

Of course it would be incorrect to conclude from these examples that the Leipzig model has persisted up to the present day in the form in which it was practiced under Wundt. The use of interchanging experimenters, for instance, has not survived. What really survived was the classical natural science tradition of according general significance to the results from very small numbers of subjects.

Operant Conditioning: Skinner

Another example of research in which this tradition is still alive is operant conditioning in the line of B.F. Skinner.[6] Unlike most behaviorists Skinner held group data in low esteem, and was an ardent advocate of *single case* operant conditioning. Perhaps his approach is closest to Pavlov's conditioning of single dogs. In an address to the Pavlovian Society of America in 1966 he acknowledged the strong bond between himself and Pavlov (*See* Barlow & Hersen, 1984, p. 5).

Just like the 19th century physiologists, Skinner experimented on single subjects. However, where they followed an invasive approach, his method consisted of repeated objective measurement in intact, healthy subjects over a long time under highly controlled conditions. The principles of this approach are laid down in Sidman (1960), a book that, according to Skinner (1983, p. 266), "became a kind of Bible among operant conditioners."

How far Skinner's method deviates from the neo-Galtonian tradition appears from the following quote: "Operant methods make their own use of Grand Numbers: instead of studying a thousand rats for one hour each, the investigator is likely to study one rat for a thousand hours" (Skinner, 1966, p. 21). In his autobiography (1983, p. 123) he relates how, after having reported at an APA-conference in the 1950s on the results of an experiment on one rat, he provoked his audience by continuing: "in deference of the standards of this Association I will now report on the other rat". On the other hand, this quote gives evidence of the degree to which the neo-Galtonian model already had become standard practice in American psychology at that time.

The reservations journal editors had toward small-N studies were a major reason for the founding of the *Journal of the Experimental Analysis of Behavior* (JEAB) in 1958, edited by the Skinnerian *Society for the Experimental Analysis of Behavior.* In 1968 the Society started publishing a sister journal, the *Journal of Applied Behavior Analysis* (JABA). Both journals carry hosts of single-case and small-N studies. David Krantz (1971) compared the design of animal experiments in the JEAB and the JABA with that in the *Journal of Comparative and Physiological Psychology*. He found that in 1969 87.5% of the articles in both Skinnerian journals followed an "*intra*-individual" design, against 88.5% cases of an "*inter*-individual" design in the JCPP. Quite appropriately Krantz speaks of "two separate worlds". Many examples of small-N research can also be found in the Skinnerian reader *Operant Behavior* (Honig, 1966).

Würzburg and the Cognitive Revolution

I now come to a domain of research that Danziger did not recognize, probably because it came to prominence only in the second half of the 20th century, and thus lies beyond the scope of his examination of investigative practices. It is the field of the cognitive psychology of thinking, and the rapidly developing study of Artificial Intelligence, including the building of expert systems that issued from it.

The cognitive psychology of thinking has its roots in the *systematic introspection* of the Würzburg School around Oswald Külpe, an approach in which the introspective stance of the Leipzig model is further extended. In his book (pp. 42–44) Danziger characterizes the systematic experimental introspection of Külpe, Titchener, and other Wundt-pupils as "a dead end" and "a rather odd episode", and its fate as "a debacle". It is certainly true, that the debate between the principal protagonists about the proper conduct of introspection ended in a blind alley in the first decades of the century. However, this should not make us forget that a specific variant of introspection or retrospection, to wit *thinking aloud while fulfilling a particular task*, has opened up a very prolific line of investigation. The most important representatives of this line of investigation were Külpe's pupil Otto Selz (1913, 1922), Gestalt psychologist Karl Duncker (1926, 1935) and the Swiss psychologist Edouard Claparède (1917, 1932).

The *Selbstbeobachtung* they asked of their subjects differed fundamentally from the simple inner perception required by Wundt, and was, for that very reason, rejected by him. But fundamental characteristics of the Leipzig model, like the reporting of individual data and interchangeability of experimenter and subject, were preserved. One could even say that these characteristics were amplified in the Würzburg-style experiments: being a well-trained subject became a still more crucial factor here.

The Nazi regime has nearly eradicated this line of research (both Duncker, an open anti-fascist, and Selz, a Jew, were removed from their posts, and Selz even met his end in a concentration camp), but after some time a revival occurred. One of the links in this development has been the study on *Thought and Choice in Chess* by de Groot (1946/1965). Inspired by the work on productive thinking of Selz, who came as a refugee to Amsterdam shortly before the outbreak of the Second World War, de Groot used the thinking-aloud method to trace the thought processes of grandmasters and other chess-players. They were identified by their initials—a typical feature of the Leipzig-Würzburg approach. His example was followed by the Dutch psychologist Jongman (1968) and the Swiss psychologist Gobet (de Groot & Gobet, 1996). The thinking aloud method has been also applied to the thinking of neurologists by Snoek (1989), and by Hamel (1990) to the thinking of architects.

Drawing on de Groot, but also on Selz and Duncker and on Bartlett's (1958) study of thinking, Allen Newell and Herbert Simon (1972) used the thinking-aloud protocols of only a few experts as a basis for the construction of mathematical models of problem solving and chess-playing. Subsequently Newell, Shaw and Simon (1958, 1963) implemented these models in computer programs. Simon's pupil Ericsson has developed the method of protocol analysis further (Ericsson & Simon, 1984). In the last decades *Artificial Intelligence* (AI) experienced a veritable boom in the construction of expert systems, instantiated in computer programs. They were based on the problem solving procedures of top experts, that were traced down by means of the thinking-aloud procedure (e.g. Kidd, 1978; McGraw & Harbinson-Briggs, 1989; Ericsson & Smith, 1991). In these recent advancements the goal is no longer the development of psychological theory, but of *technology*: knowledge engineering (Feigenbaum, 1984).

In these studies the relation between experimenter and subject is elevated to the supreme level of exchange between experts—knowledge experts on the one side and task experts on the other side—exploring together the possible reaches of human cognition and performance. Although as experts they are of equal standing, the symmetry and interchangeability, which were typical of the classic Leipzig experiment, have disappeared here. In this sense, elements of the Paris model have crept in, not so much in its original clinical use of demonstrating hysterical patients and hypnotic phenomena, but in the form in which Binet used it in his investigations into the characteristics of singularly gifted subjects, such as great calculators and blindfold chess-masters. The continuing influence of the Paris model can also be found in studies of creative individuals, such as Bahle's (1936) study of composers.

NAMING THE MODEL

The references made to *elements* of the Leipzig and Paris models that were preserved, imply that it would not be appropriate still to maintain the original nomenclature. How, then, should we name the various modalities under review? I would suggest only designating a model by the name of a special geographic site or historical person if it is a matter of a *school* within which the model originated and from where it further was disseminated. This was clearly the case with the Paris of Charcot and the Leipzig of Wundt. So these designations conform to our criterion. Würzburg qualifies for a position as a model of its own: the way introspection was used by Külpe and his pupils deviates to such an extent from the way envisaged by Wundt, that it seems better to speak here of an separate school. Titchener's introspection deviated also from Wundt's, but one can hardly say that in this respect he established a school. In this sense he represented a "dead end", indeed. In the later cognitive *thinking aloud studies*, cited in section 4, the focus shifts from the normal to the exceptional: special talents or competencies. In this sense elements of the Paris model creep in. In the absence of a specific geographic origin of this new paradigm I propose the term *expert model*. Skinner at first sight seems to be a case in itself, but at a closer look his approach is not more than the resumption of the classical natural science model.

In the case of the Galtonian model Danziger uses not the birthplace but the founding father of the new model. This is disputable, because Galton, just as his predecessor Fechner, certainly was a great source of inspiration—to Karl Pearson and James McKeen Cattell above all—but hardly can be called the founder of a *school*. Danziger acknowledges this himself in the first article in which he proposes his three models. He identifies Clark University as the major center where (under Stanley Hall) the new research practice was first systematically employed and, consequently, speaks of the *Clark model* (Danziger, 1985, p. 137). But in his next publication about research practice (Danziger, 1987) he abandons *the Clark model* and ends up with "Galtonian", after toying with the "Galton-Pearson model". In this situation I personally prefer to part with both geography and paternity and—as I did already in proposing the term "expert model"—to opt for a name in which the quintessence of the approach is expressed, namely *Differential model.* The Galtonian approach of the single case, as discussed in section 3, already has assumed an appropriate name: the *N = 1 model*.

The neo-Galtonian introduction of artificial groups represents such a fundamentally new step in the logic of experimentation, the importance of which—as Danziger (p. 85) emphasizes—is "difficult to overemphasize", that a new designation is in place. I propose to speak here of *Control group model* in contradistinction of the original Differential model.[7]

However, the real divide lies not between "Leipzig" and "Galton" but between the classical natural science tradition that informed both "Paris" and "Leipzig" and its derivatives on the one hand, and the stochastic way of thinking that came up at the turn of the century on the other hand. This is the pivotal difference in "constructing the subject" around which Danziger's book turns. The fact that this transformation appears to be less complete than Danziger's survey of developments in the first half of the twentieth century suggests, confronts us again with the reason of the persistence of basic elements of the natural science tradition.

METHODOLOGY IN ITS CONTEXT

The Social Context of Investigative Practice

Danziger conceives of science as a social activity, in line with modern sociology of scientific knowledge. Researchers not only have to appeal to their own research community and representatives of related disciplines for acceptance of their results, but also have to deal with an external environment that furnishes (or withholds) financial support and provides a market for knowledge products.[8] In Danziger's view the methodological shift that was discussed above is the outcome of the contest between these two opposing forces: On the one hand the methods that had brought such impressive successes in the natural sciences were seen by the aspiring discipline as the royal road to discover the *laws* of consciousness, and thus to gain scientific respectability. On the other hand, the external market—the educational market to begin with—soon began to ask for useful products. It was the "pull" from this market that finally led to the adoption of Galtonian differential thinking, first in applied psychology, then also in basic research, in the form of control group methodology. The fact, we could add, that the use of controls and randomization as a methodological precaution had a respectable tradition in experimental psychology certainly has favored the reception of the new approach within the scientific community.

However, accepting the control-group model as a valid tool for particular research problems is not the same as adopting it yourself. Psychophysics psychophysiology, and the other domains examined in our section 4 had good reasons to adhere to the methods of the "harder" natural sciences, because this was the accepted style in the professional environment in which they operated. My analysis of papers in the journal *Vision Research*, showed that many of the authors were employed in institutes with an interdisciplinary staff. Psychologists in this field don't see themselves as social scientists, but as representatives of the natural sciences. Their publication channels are not controlled by social scientists, but by natural scientists, for whom experiments on single subjects are current practice, because of the invariance of the physical substrate. And where they did operate in a social science environment, as was the case with Skinner and his group, they could resort to entrenching themselves in a sub-community, with its own journals and its own external contacts.

The external market for the products of psychophysical and psychophysiological research was quite different from the social science market in which the differential approach developed. From World War II onwards, studies on vision and motor control have proved to be of great practical interest to the military and to industry (in such things as navigation, design of displays, handles and control panels). Present-day research institutes in this field have close affiliations with the defense system, industry, or both. Machine recognition of speech and handwriting belong to the more recent examples of applied research in this field. To be an attractive partner in this market, psychologists do better to affiliate with the "hard-nosed" natural scientists, and to follow their methods and manners, and not those of the "softer" social scientists. This was another reason for adhering to the "good old" natural science model.

In the cognitive study of problem solving and artificial intelligence similar contextual factors apply. Research is conducted in an interdisciplinary arena with a strong natural science identity, and published in journals in which the methodological conventions of other psychologists play only a secondary role. In the U.S.A., the Netherlands and some other countries psychologists belonging to this category even have formed a *Psychonomic Society* of their own, outside the professional association of psychologists. As to the external market, strong technical and economical forces appear to play a part here as well—particularly in the applied branches of the field. The prevailing practical interest here is in making a specific *expert performance* marketable. This has led to the new variant of the natural science model for which I proposed the name "expert model".

The "Probabilistic Revolution"

If the natural science model still holds in these prestigious sub-fields of psychology, why is it that "Galtonian" thinking could attain such a strong position? Would the sheer pull from the market for applied research be sufficient to lead to the "triumph of the aggregate?" To my mind the fundamental methodological turn that took place in the first half of the twentieth century could come about only thanks to an additional factor. This was the "probabilistic revolution" in science that occurred in the same period, and that radically changed the intellectual climate in Western thinking. This revolution has its roots in attempts at "taming chance" of 17th and 18th century gambling aristocrats, calculating merchants, entrepreneurs and rulers (Hacking, 1975), and gradually spread from commerce, insurance and governmental sadministration to science and everyday practice. Chance became a leading principle in scientific research and in rational decision making. Statistics became a *Lebensgefühl*, and a way of life (Gigerenzer et al., 1989, p. 289).

Studies into the impact of the probabilistic revolution on the various sciences are of recent origin (Gigerenzer et al., 1989; Krüger, Daston & Heidelberger, 1987, Krüger; Gigerenzer & Morgan, 1987). The most conspicuous implication is, of course, the subversion of the deterministic worldview of the classical natural sciences. I shall pursue this aspect only briefly, and concentrate on the epistemological aspect. A full appreciation of the implications of probabilistic thinking for the "construction of the subject" would require a chapter of it own.

The natural science approach that informed both the Paris and Leipzig models and their derivatives was characterized by a Laplacian mechanistic, deterministic worldview. Typical in this respect is the assertion of Claude Bernard (1865/1957, p. 136) that ". . . scientific law can be based only on certainty, on absolute determinism, not on probability." In this worldview variance in scientific observations was ascribed to the imperfection of our registration of reality, not to the haphazardness of nature. In spite of their fascination with variance, nineteenth century authors like Quetelet and Galton were still very much impressed with the order in nature (Porter, 1983, p. 36). For them statistics was an instrument for making this order visible.[9] This orderly view on nature had gradually to give way to a probabilistic worldview. Within the natural sciences evolutionary biology and quantum physics led the way in accepting it, whereas research in physiology was much less affected.

As we saw, Leipzig laboratory research was inspired by the natural science tradition. In this line, variability was considered as error around a true value that characterizes a natural process. Probabilistic thinking was tolerated solely as an expression of the experimenter's ignorance. In the control-group model statistical probabilities acquired a much more fundamental role. Though probabilistic thinking here too, as Gigerenzer (1987) notes, was enlisted in the service of determinism, the essential tenet of determinism: establishing true causal connections, was sacrificed here. Causality in nature was assumed, but not traced down. Metaphysical determinism was exchanged for pragmatic determinism, and certainty for probability: a methodological probabilism. Probabilistic statistics, based on sampling and randomization, became the standard tool of inductive inference – quite a different form of orderliness than the order presupposed in the natural science model. This transition ran parallel to the transition from essentialism to pragmatism in applied psychology which I have described elsewhere (van Strien, 1998).

In the second half of the 20th century statistical inference, first only a tool, became a model of the human mind: the theory of the mind as "an intuitive statistician" (viz. Gigerenzer & Murray, 1987). A discussion of the implications of this extended revolution for cognitive psychology lies outside the scope of this chapter. In the present context it may suffice to conclude, that the epistemological "*re*-construction of the subject" described by Danziger could only become so profound thanks to the probabilistic revolution.

CONCLUSION

An examination of investigative practices of psychologists beyond the period reviewed by Kurt Danziger showed that the research style of basing inferences on the experimental data of one subject or just a few subjects—first initiated in Leipzig—continued to play a vital role in several domains. This was not only the case in psychophysical and psycho-physiological research—the areas to which Danziger drew attention himself—but also in the realm of operant conditioning and in some parts of cognitive psychology. In the study of artificial intelligence and expert performance it even witnessed a revival in the form of what I called the expert model. Both Danziger's Leipzig model and his Paris model appeared to be variants of an encompassing *natural science model* that gained prominence in the course of the 19th century. The most fundamental methodological divide appeared to lie not between the Leipzig model and the Galtonian differential model, but between the classical natural science model and the stochastic approach that, as a consequence of the probabilistic revolution, increasingly gained ground in the twentieth century. This led me to a proposal to re-label the current research models. These qualifications, however, do not detract anything from Danziger's explication of the radical transformation in "the construction of the subject" that took place in the course of the past century.

A contribution of equal rank has been the way he has placed this transformation in its social context. Of particular interest for the historian of science is the insight that the methodological shift that took place was not solely due to new scientific insights, but also to developments in the market of psychological services in which psychologists operated. Psychological practice set the tune, and the laboratory followed. In the survival of small-N research in the second half of the 20th century similar market forces appear to have played a significant role.

NOTES

[1] The author wishes to thank Kurt Danziger for giving insight into his criteria for categorizing research articles according to investigative practice and his former colleagues Bert Mulder†, Willem Levelt, John Michon and Justus Verster for information on psychonomic research methods. I also owe much to the stimulating comments and suggestions of Trudy Dehue and the anonymous referees of

a previous version, and to Adrian Brock and Johann Louw for their encouragement and painstaking editorial work.

[2] When not stated otherwise, the Danziger citations hereafter refer to this book.

[3] Most probably Boring's statement is based on Leipzig laboratory lore, cited by Titchener—himself a pupil of Wundt—to impress the importance of the role of subject upon his disciples.

[4] This visitor was the Dutch psychologist Willem Levelt (personal communication). It should be noted, though, that Gestalt psychologists did not confine themselves to demonstrative experiments, but also applied the traditional Leipzig research style (Ash. 1995, 220–24).

[5] Volume 25, 1985. Because of the bulk of the volume (more than 2000 pages!) I analyzed only the even issues. Though the professional affiliation of the authors could not be ascertained in all cases, it can safely be assumed that at least half of them were working in a psychology department.

[6] Although he does not mention Skinner's name, Danziger appears to be aware of the exceptional position of the operant psychologists, but disposes of this anomaly in only a few words (p. 154).

[7] In introducing these designations, I retract my proposal in my Passau paper (van Strien, 1996) to use the term *competence model* for both the original "Leipzig model" and its later derivatives and the term *sample model* for both the "Galtonian model" and the "neo-Galtonian model".

[8] There is a close similarity between the social context as conceived of by Danziger and the *relational field* of investigative practice in my "relational model" (van Strien, 1991, 1993a, 1993b).

[9] This common element should not make us loose sight of the difference between both authors: to the astronomer Quetelet the average was something of an almost Platonic order, and error was error; to Galton the average was subject to continuous (eugenetic) amelioration (Hilts, 1973, Porter, 1986, Gigerenzer et al., 1989).

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