

## Introduction

In the history of the social sciences the quest for the correct rules of science followed very diverse routes. The historical development of these sciences has been marked by frequent and fierce debates on their “methodology.” Even the most general criteria, about which there seems to be agreement in the natural sciences, are here emphatically disputed.

In this book that phenomenon is subject to closer study. The existing variability of “methodological styles” in the development of the social sciences is illustrated by detailed historical material. Moreover, that historical material is used in the search for an answer to the main question of the book: How should such a profound flexibility of the standards in the social sciences be understood?

Not just the “cut-and-dried” methodological criteria in manuals are studied, but the ways in which certain rules in particular groups or periods could *become* the undisputed prescriptions are carefully traced. To that end, the historical scope of this study is deliberately restricted to the relatively small domain of psychology in the Netherlands. Psychology has always been strongly methodology oriented, and the Netherlands is not only conveniently small, but because it is wedged geographically between many great powers, its inhabitants traditionally are very internationally oriented. Dutch psychology therefore offers a miniature image of international Western psychology: It is small enough to be studied at a detailed level and nevertheless varied enough to be representative of the discipline’s variability.

Chapter 1 gives a brief historical overview of methodology in Dutch psychology that describes phases of an orientation to the German *Geisteswissenschaften*, a Continental-European phenomenological approach and an Anglo-Saxon empirical-analytical style. This chapter explains why this study does not focus on what researchers *actually do* but what

prominent representatives of the field write down that they *should do*. Also, the use of the concept of methodology is defended to encompass *every* position taken by academically trained and prominent representatives with regard to requirements for qualitatively good psychological work.

Next I discuss the option that methodological changes in the history of the social sciences can simply be ascribed to a steady growth of insight into the proper rules of science. If this is so, psychology in the Netherlands, which is now mostly oriented to a hypothesis-testing empirical-analytical approach, slowly but surely seems to have come to realize how the job must be done if it is to be carried out in a scientifically correct manner. However, I argue that historical work, which shows such a process of growth, generally does so by declaring, implicitly or explicitly, the contemporary state of affairs as its criterion. In the presentist or finalistic accounts that arise in this way, an objective linear growth of insight from the past to the present is suggested, whereas in fact the line is actually drawn by the historian from the present to the past.

This leads to the question of whether the image of cognitive progression can be maintained in a historical study where the contemporary state of affairs does *not* function as a criterion. In principle, in a nonfinalistic approach, too, constellations of now-antiquated rules might turn out to have been inadequate and a new methodological style could turn out to have been more suitable. However, such a conclusion would imply that there were periods in the history of Dutch psychology when academically trained and experienced practitioners of the field, who furthermore were aware of the better methods via the professional literature at home and abroad, persisted for decades in making errors that were against their own interests. Because such an unlikely picture is convincing only when it has turned out to be inevitable, I decided first to develop an alternative option and to check its plausibility against the most important episodes and groups in the history of Dutch psychology.

On the basis of an analysis of four characteristics of social scientific methodology, I develop an alternative hypothesis in the last sections of Chapter 1, namely, that in the course of its history Dutch psychology affiliated itself with different social groups having different ideas on social relationships and that this sociocultural pluralism found its expression in its variable methodology. Thus, in other words, it is assumed that in a methodological style, apart from the *scientific* identity of the discipline, a *social identity* is at the same time also given expression and that studying the process in which a discipline or a group disposes of an old scientific identity and assumes a new one demands the discovery of the changing social identity of that discipline or group. The first chapter concludes

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with a discussion of the differences between these presuppositions and those of “contextualist” historiography, which searches for the external (and internal) “influences on” science. I present reasons why the question posed in this book is not what factors influence methodology, but what urges a scientific community to create or subscribe to a specific scientific identity.

Chapter 2 concerns the methodology of psychological assessment for personnel selection and vocational guidance. Emerging under the name of “psychotechnics” in the 1920s, it remained the dominant branch of Dutch psychology until the 1950s, and to this day it is an important field of psychological activity. The crucial example in this chapter is the debate about characterological interpretation of handwriting (“graphology”) as a psychodiagnostic instrument. This debate, which blazed fiercely in the fifties and sixties, confronted two distinct methodological styles. The first style, known as *geisteswissenschaftlich* – which included graphology – lost the battle. From the mid-sixties onward, the “empirical-analytical” style became almost commonplace.

To answer the question of how the rules came to change in this case, first the issue is raised of why the former empathizing methods, which now sound so unconvincing, were once able to seduce people and institutions into spending money and effort on psychological diagnostics. Following from that question, the second asks: What led to the empathizing methods *loss* of persuasiveness and to the acceptance of quantifying empirical-analytical methodology?

I demonstrate that the methodological rules of the former psychotechnicians were tailored to an interpretation of the question of professional eligibility as a request for guidance in finding life fulfillment. This interpretation was developed in the welfare institutions where psychotechnicians were employed, and it was viable as long as the so-called pacification democracy, with its image of harmonious labor relations, persisted in the Netherlands.

With the politicization and polarization of Dutch society in the fifties and sixties, the role of the assessment psychologist as a social worker could not be maintained. In place of the model of the social worker, the psychologists presented that of the businesslike, detached middleman in manpower. The profession became governed by a new set of methodological rules that proved to be appropriate for this new role.

So this history contradicts the view that the former psychotechnicians did not yet properly understand how their profession should be “scientifically” practiced, whereas the postwar psychologists gradually came to understand this better. I conclude that the changes in the methodology of assessment psychology were not the consequence of an accumulation of

insight into “the” independent criteria for the correct approach, but rather were the result of the fact that the profession itself changed as a component of a changing society. The change of the *scientific identity* of the discipline pointed to a change of its *social identity*.

Chapter 3 deals with phenomenological psychology at the University of Utrecht in the period of 1945–70, which became known as “the Utrecht School” in psychology and attained a strong position in the international “phenomenological movement.” First, the methodological aspects of the Utrecht phenomenological psychology are worked out. Next, I demonstrate that this methodology can be understood properly only if one realizes that it was not Husserlian phenomenology but Max Scheler’s personalistic philosophy that was the main intellectual basis of its viewpoints. That point raised the question why the members of the school named their approach phenomenological psychology and not personalistic psychology. The fact that *personalistic socialism* appears to have been the source of the school’s social ideals answers that question and elucidates how it was possible that this psychology could flourish precisely in the first postwar period in the Netherlands, whereas in the 1960s it was quickly forced back by the statistical empirical-analytical approach.

Personalistic socialism thrived during the Second World War in the Netherlands and found a strong following in some circles after the war, but it soon became controversial and almost completely lost its cogency in the sixties. Thus, the thesis is illustrated again that subscribing to particular professional rules involves choosing specific positions on the level of social relations. I show, this time on the basis of the Utrecht phenomenological psychology, that the degree to which a methodological style is accepted and maintained varies with the contemporary adequacy of the social identity expressed in it.

In Chapter 4 the example of clinical psychology from the 1920s to the present provides a more complicated case. Along with a gradual closure of the debates on what is to be called science, there is a *remaining* uneasiness. This uneasiness was expressed by Johan T. Barendregt, the main character of the chapter, in the metaphor of “the neurotic paradox.”

Barendregt entered the field of clinical psychology in the 1950s and in the 60s became a professor at the University of Amsterdam. The central dilemma throughout his career was that if he regarded a research project as methodologically correct, he believed it was not relevant for life outside of science and hence for clinical practice, but if he considered a project to be relevant, he criticized it for not being methodologically correct. To him this dilemma, more often expressed in the international social sciences, became so serious that living up to methodological pre-

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scriptions became a neurotic paradox: “a romantic image which ruins your life.”

As Barendregt with his paradox described a problem of Dutch clinical psychology as a whole, he is presented as the personification of the dilemma of the subdiscipline. The central question of the chapter is how clinical psychology in the Netherlands fell into this extreme dilemma of rigor versus relevance and why its doubts did not go away.

I argue that the dilemma is not the inevitable outcome of an essential and universal incompatibility, but the consequence of an *incompatibility of social identities* that emerged historically in clinical psychology's laborious process of emancipation with regard to psychiatry. In the course of time, clinical psychologists fought for their independence from the psychiatrists who employed them, not only by introducing their own methodology borrowed from industrial assessment psychology, but also by assuming their own social identity in the context of mental health care. And *that* social identity was very different from the identity traditionally bound up with assessment psychology. I conclude that the neurotic paradox of clinical psychology in the Netherlands is another example of a methodological issue that cannot be properly understood without taking into account the social identity that is expressed in a methodological style.

The various themes of the book come together in Chapter 5, and a next step is taken. I raise the objection that showing the rationality of several methodological styles in their own social setting does not necessarily mean that all of these approaches can also be called *scientific*. It is still possible perhaps to speak of Dutch psychology as gradually becoming scientific in the sense that it has finally linked up with the image of science that just happens to be the *internationally current* one. Looked at in this way the history described in the previous chapters did not in fact refute the image of a steady growth of insight into the proper rules of science, but showed only that certain social circumstances provide the necessary conditions for the real scientific approach to flourish.

However, first I argue that this position simply denies the many methodological styles in the international social sciences that even today distance themselves from empirical-analytical thinking. Empirical-analytical rules are not as internationally current as some may wish them to be. Moreover, this position overlooks the fact that even *within* the empirical-analytical realm, the unanimity it assumes is not to be found.

In this chapter, the stance of international agreement on empirical-analytical rules turns out to be untenable too. On closer philosophical consideration the mainstream of Dutch psychology appears to adhere to a particular individual variation within the international empirical-

analytical framework. Specifically, the rule that the Dutch themselves consider to be the *key* notion of scientific methodology is not to be found in either the closest philosophies of science (logical empiricism and falsificationism), or international handbooks of social scientific methodology.

Furthermore, this Dutch variation appears to be related directly to the changing social identity of psychology in the Netherlands. The methodological rule of Dutch psychology that is most taken for granted, as shown at the end of this chapter, is the product of the problems, unique to a certain extent, with which Dutch psychologists were confronted in the course of history and with which they would not have been confronted if their discipline had not had its own changing social role to play in its own changing society.

In this way, finally, the standard view of a growing insight into an abstract general rationality is exchanged for a different picture. That is the picture of social scientists who, though using methodology that is available elsewhere, nevertheless participate actively in their own society and in accordance with this define the rules of their discipline. I also conclude that the former deviations from today's standards should not be ascribed to intellectual confusion in the past. The alternative image is that psychology in the Netherlands affiliated itself to many different social groups and that a sociocultural pluralism is expressed in its variable methodology.

The epilogue discusses the corollary that even if researchers in everyday practice always behaved according to the rules, that would not guarantee that their work would be culturally independent. This, of course, is by no means the last word on methodological matters. On the contrary, in the end, the symmetrical approach in this study was only a means on behalf of the contemporary methodological debate itself. In the epilogue, some consequences for this debate are briefly indicated.

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## The variability of methodological standards in the social sciences

In the empirical science studies of recent decades, a completely different picture has been drawn of the development of scientific knowledge than that portrayed by philosophers of science and the majority of scientists themselves. Both groups ascribe scientific progress to following the correct *methodological rules*. However, on the basis of empirical investigations, researchers in the field of science studies claim that this picture has little descriptive or empirical adequacy. They argue that agreement with regard to methodological criteria in fact exists at best on an extremely general level and that in practice these general criteria are interpreted in diverse ways, or even that researchers do not at all keep to methodological rules in carrying out their work. The development of scientific knowledge, many argue, is actually a social process.<sup>1</sup>

If the contribution of methodological rules to the development of scientific knowledge is negligible, as some have reasoned, it must be explained which objectives drawing up methodological criteria actually does serve. Various *rhetorical* functions of methodology are revealed: Methodological arguments are said to reinforce retrospectively one's rightness, to impress the outside world, or to legitimize preconceived and culturally determined differences.<sup>2</sup>

Apart from this, through the years studies have been carried out on the historical development of the rules of science.<sup>3</sup> Methodological rules do not determine the most important theoretical and empirical developments, but were actually determined by them, it was argued.<sup>4</sup> Others

Parts of this chapter have been published in English in T. Dehue, "Why Methodology Changes: Transforming Psychology in The Netherlands I," *History of the Human Sciences* 4 (1991):335–51; T. Dehue, "Why Does Methodology Change Over Time? A Theoretical View and its Implications," in H. J. Stam, et al., eds., *Recent Trends in Theoretical Psychology, Vol. III* (New York: Springer Verlag, 1993), pp. 495–502.

advanced a sociological approach to the historical multiplicity of criteria, which consisted not of scientific theories, but the culturally steered consensus of researchers or their image of the ideal mutual relations that determine which rules have the most value ascribed to them.<sup>5</sup>

Once methodology had been reduced to the “fifth wheel on the wagon,” however, not *much* interest remained in the characteristics and development of the rules. The number of empirical studies into the functions of methodology, or into the question of how a particular conglomerate of rules *is constructed or established*, lags far behind the number of investigations that only focus on falsifying the functions methodology is said to fulfill by philosophers and scientists themselves. In addition, the existing studies are almost exclusively oriented to diverse quantitative research styles. Little attention is paid to the social and behavioral sciences (henceforth for the sake of brevity to be referred to as “the social sciences”).<sup>6</sup> And when methodology in these sciences is empirically and historically investigated, the focus is mostly on those methods and techniques that are derived from the natural sciences.<sup>7</sup>

However, in disciplines like sociology, psychology, and political science, the quest for the correct working method led to even more diverse routes than was the case in the natural sciences. Also widely discussed in these sciences are general criteria such as the preference for quantitative statements over qualitative or predictive accuracy, about which, according to some, there is agreement in the natural sciences.<sup>8</sup> It has by no means always been, nor is, a matter of course for all academic practitioners of the social sciences to believe that one is allowed to speak of “science” only when general laws are sought, predictions are made, and quantitative values are ascribed to observable phenomena. Such conditions were, and continue to be, emphatically disputed by phenomenologists, hermeneuticists, ethnomethodologists, constructivists, and the like. They reject the “empirical-analytical” way of thinking, as I will call the broad category under which various quantifying approaches fall.

In this book the phenomenon in the social sciences that there are differences in thinking among groups or between time periods on even the level of the most general rules will be subject to closer study – not only because it is a relatively unexplored topic, but also for reasons to be extensively elucidated later in this chapter. The existing variability of methodological “styles” in the development of the social sciences is illustrated on the basis of detailed historical material. Moreover, that historical material is used in the search for an answer to the main question of the book: that is, how such a profound flexibility of the standards in the social sciences should be understood, or more specifically, how a



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particular scientific community comes to create or endorse a particular methodological style.

*Psychology in the Netherlands as a case study*

In such a project, it is not enough to study the cut-and-dried methodological criteria in manuals. As Latour put it, it is necessary to work back from the law book to the parliament when the laws were still bills.<sup>9</sup> It must be discovered how in a particular group or period certain rules could *become* natural conditions. This requires a level of detail that is achievable only by restricting one's research to a small domain.

Psychology in the Netherlands is a field particularly suitable for my purpose. From its beginning as a university discipline, psychology has been strongly methodologically oriented. Moreover, Dutch psychology is particularly suitable if only because the Netherlands is so conveniently small (some 34,000 square kilometers – 13,000 square miles – of land, with at the moment approximately 14.9 million inhabitants,<sup>10</sup> including around 20,000 psychologists).<sup>11</sup> The Dutch, moreover, wedged in between German-, English-, and French-speaking great powers, are traditionally very internationally oriented. They have always been aware of what is written in other countries, and academics in particular did not have to wait for Dutch translations of foreign-language materials. As the Dutch labor historian A. J. C. Rüter remarked about his countrymen:

We sit and we have always sat at a crossroads in Europe, and our borders have always been open to traffic – intellectual traffic, too. Our civilisation has therefore continually absorbed elements which it borrowed from the great cultural and social currents traversing Europe. If the labour movement borrowed many features from abroad, this does not make it un-Dutch. On the contrary, I view this ability to absorb as a typical Dutch characteristic, at least to the extent that the labour movement refashioned the borrowed elements into an entity which, as an entity, carried a specific Dutch imprint.<sup>12</sup>

This volume will show that these characteristics hold not only for the labor movement but for the methodology of Dutch psychology as well.<sup>13</sup> From the profession's beginning as a university discipline, methodological standards from Germany, England, the United States, and to a lesser extent also from the less linguistically close France, easily got through to the psychologists in the Netherlands. They assumed those standards directly, or placed them beside their own convictions and adjusted them

according to their requirements. Thus, in a small geographical area, over a period of hardly a century, many different methodological styles were in competition with each other. In contrast, in a study of psychology in, say, the United States or England, the historian mostly encounters methods oriented to the natural sciences,<sup>14</sup> whereas historically Dutch psychology, as well as French and German psychology, were also open to Continental-European philosophy. Therefore the Netherlands offers a miniature image of international Western psychology, small enough to be studied at a detailed level and nevertheless varied enough to be representative of the discipline's variability.

In his publications on the "phrenology disputes" in early nineteenth-century Edinburgh, the historian of science Stephen Shapin concentrated on just one town.<sup>15</sup> These studies graphically illustrate that restriction to a small geographical area also has some extra advantages. As the studies show, spatial nearness more easily leads to controversy and controversies in particular are very instructive for science studies. Because traits are never as clearly revealed as they are in mutual clashes, they offer an opportunity to obtain clear pictures of all the parties involved. Moreover, the "reopening" of what are now closed debates makes us realize that what nowadays seems obvious was once by no means so evident and clear.<sup>16</sup> For example, the sociologist of science Harry Collins has compared currently known self-evident truths in the sciences to ships in bottles. Just as a ship was not always there in its completed form, but was once nothing more than a pile of sticks, scraps of cloth, and dabs of glue *outside* the bottle, scientific facts and insights are not givens that have been discovered but the final result of a series of operations, decisions, and events. The description of periods of instability and uncertainty in the history of a scientific field leads us, to begin with, to *questioning* how the ship got in there. With an analysis of the process whereby the debate reached its present outcome, it can then be shown how the sticks, the rags, and the lengths of rope ended up as a ship in the bottle.

Particularly the fact that spatial nearness leads to controversies is why psychology in the Netherlands is a very suitable case for my aims. Indeed, many strong confrontations will be reported in the coming chapters. Meanwhile, I will not attempt to give a complete historical overview of everything that has been presented in Dutch psychology on the level of methodology. Rather, the most important episodes and themes from its history will be used to test the premises on which methodological changes and consensus formation take place, a theme to be developed in the last section of this chapter. Let me first give a brief and broad