

The creative tension between distance and commitment in the historiography of one's own discipline

P.J. van Strien

Zusammenfassung: In diesem Beitrag zu einem Symposium, das aus Anlaß des 25. Dienstjubiläums seiner Tätigkeit als Professor an der Reichsuniversität Groningen, Niederlande, durchgeführt wurde, berichtet der Verfasser über seinen "Weg zur Geschichte". Neben allen guten Gründen für das Studium der Geschichte der eigenen Disziplin, wird ein besonderer detailliert diskutiert: Die Einsicht in den intellektuellen und sozialen Kontext und die historische Relativität von Theorien und Begriffen, wodurch der empirischen und theoretischen Diskussion innerhalb einer Disziplin eine dritte Dimension hinzugefügt wird. Der letzte Abschnitt behandelt das Verhältnis des professionellen Wissenschaftshistorikers zu der kreativen Spannung zwischen Distanz und Commitment, die zur Abfassung der Geschichte der eigenen Disziplin stets dazugehört.

Abstract: In this contribution to a symposium organized, at the occasion of the 25th anniversary of his professorate at the State University at Groningen, Netherlands, the author gives an account of his "way to history". Of all good reasons, for studying the history of one's own discipline one is discussed in more detail: the insight into the intellectual and social context, and the historical relativity of theories and concepts, which, as a third dimension, is added to the empirical and the theoretical discussion within the discipline. The last section focusses on the relationship with the professional historian of science, and the creative tension between distance and commitment involved in writing the history of one's own discipline.

Shortly after I exchanged the chair of Work and Organizational Psychology for the chair for the Introduction, Foundation and History of Psychology¹, which I now hold, a colleague made a remark to me along the following lines: „history, well, it looks very appealing; it is my fancy to do it myself after my retirement: after having spent a lifetime in the field you have an overview over not less than half a century! Still, I can not understand why you are already putting most of your energy into it now - there still is so much useful research to be done!“ Studying the history of one's own discipline is regarded here as something similar to the writing of memoirs by an elderly statesman who has retired from active politics.

Has my move to the Foundations and History of Psychology, nearly twelve years ago, been a premature departure from active science? I must confess that the true situation, if viewed from the standpoint of the colleague I just quoted, is even worse. As a student I took a strong interest in the historical roots of the opposing psychological and philosophical directions with which I was confronted. When, as a junior faculty member, I was asked by my teacher and mentor Snijders to set up an undergraduate history of psychology course, I enthusiastically started to compile a reader of historical texts as a basis for discussion in my classes². In my PhD research on reporting in personnel selection³ I did not restrict my research to the original question of the influence of the report on the opinion of the future employer, but gave in to the temptation to trace the historical development of the diagnostic approaches involved, even back to the characterizations of persons in seventeenth-century literary salons. My first archival study dates back to that period. In my later investigations and publications in the field of work- and organizational psychology I felt again and again the urge to return to the past and to trace the historical background of present controversies and concepts, whether the subject concerned the professional role of the psychologist (the 'three decades' in my *From psychotechnics to criticism of society*), the successive paradigms of practice in organizational research and practice, the concept of alienation, or the meaning of work from antiquity up to the present time. After my appointment to my present chair, I chose a historical approach in my introductory course in psychology: *comparison of mainstreams*⁴. When a few years later the issue of the integration of the social sciences caused a commotion in the Psychology Departments of the Netherlands, my contribution to the discussion consisted - besides an attempt at systematization - of a historical sketch of the process of discipline formation in the fields of psychology, pedagogics, sociology and related disciplines in the Netherlands. The plan of a study of the historical development of methodological thinking in the Netherlands, which has materialized in the meantime in Trudy Dehue's dissertation-study on the rules of the discipline⁵, originated from the same *sensus historicus*.

I would like now first to analyse what has for me personally been the motive for tracing historical lines of development, and next to pose the more general question of the benefit of historiography for one's own discipline. As indicated in the title of my contribution to this symposium, I will pay special attention to the tension between distance and commitment which characterises the historical study of one's own discipline - as is the case with me - in the role of participant. As appears from the title I see this tension as a creative one.

My personal interest in the history of my own discipline is closely connected to my sense for system. At first sight this probably appears strange, because history and theory are - as testified by Van Rappard's contribution to this symposium - often seen as antipoles. Educated in the school of Heymans⁶, one of the last system-builders, I was on the one hand positively impressed by the consistency of the philosophically founded architecture of his ideas, but on the other hand repelled by the reductionistic image of man that was implied. In this situation, tracing the epistemological and anthropological presuppositions of psychological theories became for me a means of finding a basis on which I could develop a personally satisfying thought system.

This urge to uncover the image of man and the ideological presuppositions behind the views with which I had become acquainted, has certainly to be attributed to my calvinistic upbringing. „Examining the good and the evil spirits“ is a principle I was taught at an early age. Browsing through German encyclopedias on my father's book shelves as a grammar-school pupil I became interested in the fundamentals of philosophical systems and currents. When I became acquainted with the 'Wijsbegeerte der Wetsidee' (New critique of theoretical thought) of Herman Dooyeweerd (1935) as a student, I found there too an attempt to identify the basic motives and principal choices in the development of Western philosophical thinking. The lectures of my philosophy teacher Helmuth Plessner⁷ strengthened my interest in both the fundamentals of philosophical and psychological systems, and their historical roots.

My interest in fundamentals became focussed on the practice of psychology when, as an advanced student, I was confronted with the debate between Snijders and de Groot on the basic assumptions of psychodiagnostics⁸. After my graduation Kouwer's inaugural address on *The moral quandaries of applied psychology*⁹ added an ethical dimension to my ruminations on fundamentals: the question of judging and misjudging persons. Interpretative-clinical diagnostics appealed to me in itself, because in spite of all its drawbacks it is the method which pays fullest attention to all aspects of the person. On the other hand, I was also impressed by results of the straightforward predictive psychometric methods that the new generation of industrial psychologists had imported from America in the fifties and the compelling logic of de Groot's methodological writings¹⁰. Again I began to probe the rivalling diagnostic approaches in order to uncover the image of man and the intellectual tradition in which they were rooted. Van den Berg's *Metabletica* (1956), which was published in this period, made me aware of the temporality not only of scientific ideas but also of human society and the people who are part of it. The historical approach which I followed in my

dissertation research and in the book 'Kennis en communicatie' (Knowledge and communication), that grew out of it and was published in 1966, should be understood as a consequence of these various influences.

My experiences and dilemmas as a Work & Organizational psychologist reinforced my pursuit of the historical roots of rivalling approaches, now not only in psychodiagnostics, but also in ideas about the human factor in organizations¹¹. The twofold discourse in which I became involved: with practitioners on the one hand, with representatives of academic science on the other, has made me aware of the interaction between practice and theory, and of the value-ladenness of scientific thinking, not only in professional practice, but also in psychological theorizing¹².

In the historical science studies which I have conducted in the past decade, in a situation in which I could direct my attention fully to the foundations and history of psychology, the urge to trace the roots of theories and the striving for system are again both clearly present. The former appears in the form of identifying the methodological principles, the conceptual tools and notions, and the models, metaphors and images of man and society involved in the way theoretical and practical problems are solved by various schools in psychology¹³. The second interest appears in the attempt to arrange these components under a number of basic, socially embedded, intellectual traditions¹⁴ and in my attempts at developing a general relational model of the dynamics of science¹⁵.

As this piece of personal history shows, there is no lack of commitment. That personal commitment can be a positive factor in scientific work has been generally acknowledged since Polanyi (1958), but in most cases not without a warning against the dangers of too strong a commitment for scientific objectivity: Passion for system may lead to selective handling of facts, and passion for history to criticising theories on the basis of their origin and not on the basis of empirical testing. As we know the critical-rationalistic conception of science, which is predominant in psychology, regards the context of discovery of scientific statements, including their historical genesis, as scientifically irrelevant; the context of justification is the only thing that counts. Against this background we may ask whether history of psychology has a sense that transcends that of a personal hobby.

I will resist the temptation to elaborate further here on all the good reasons which can be advanced for giving the history of psychology its proper place in the curriculum and in psychological research. In a propaedeutic history-text I have advanced six of these reasons¹⁶. Some have also been mentioned by the previous speakers. In my argument I will discuss only one reason in more detail, the one which I have most at heart. Of all the good reasons for paying attention to history this is the one in which the mutual significance of the study of the

Foundations of a discipline and the study of its History shows most clearly. In the conception I have in mind, history is assigned the position of the depth-dimension in scientific work. The other two dimensions of the cube are theory-formation as the height-dimension, and empirical testing as the width-dimension.

In present scientific practice the empirical dimension receives most attention. The decisive criterion for sound theory-formation consists in the testing of predictions that can be deduced from it. In the Netherlands Adriaan de Groot's Methodology has established this rule so firmly that it has become self-evident. In his betting model Willem Hofstee¹⁷ invests the empirical dimension with a still more decisive position. In his view theories are no more than heuristic aids in the generation of predictions on the basis of which it is decided which of the parties in a wager has won. In the last ten years, however, de Groot¹⁸ has made a plea for more attention to be paid to theoretical discussion, in order to redress the balance between the theoretical and the empirical dimension. His proposals for the formulation of rigorous concept definitions and of agreement theories (de Groot, 1988, 1990, de Groot & Medendorp, 1988) are consequences of his attempts at enhancing the theoretical dimension. The thesis which I want to put forward here is that in scientific discourse in psychology the historical dimension should become a factor of equal importance to the theoretical and the empirical dimensions.

In taking this stance, I am dissociating myself from the distinction between context of discovery and context of justification that I have already mentioned briefly. Science studies in the last few decades have taught us that the development of a science like psychology is governed only to a minor degree by empirical testing in the traditional sense. Especially when the parties in the discussion are representatives of different schools and directions, the outcome is decided by other factors¹⁹. The reason is, that there are no pure facts which can serve as unquestionable tests in the sense of strict empiricism. As Kurt Danziger (1990a) has shown in a conclusive way in his recent book the „reality“ to which psychological research directs itself is a constructed reality. The experiments, questionnaires, tests and other tools that are used in our research have been developed in the framework of the theories that must then be tested with the help of the same tools. The terms and concepts which are used, and their operationalizations are rooted in intellectual traditions in which different methodological principles and rules obtain, and different basic assumptions prevail. To these basic assumptions also belong the images of man and other fundamental notions which are denoted by Kuhn (1962, 1970) and Laudan (1977) as „metaphysical presuppositions“, „values“ and „world-views“. Scientists belonging to another tradition who have been lured into engaging in a Hofsteean wager on the basis of operationalizations that at first sight appear plausible to

them, will in most cases be ready to produce auxiliary hypotheses in order to be able to sustain their theories. By the same token agreement-theories will be laid on the table when one of the parties realizes that the core notions of their own line of thinking are endangered. Hofstee is right about the damage that loosing a bet tends to bring to a scientist's reputation, and, we may add, someone who lays an agreement on the table, after having discovered that it is disadvantageous for his own position, will suffer a loss of face too. This effect, however, will only be significant when the parties involved share the same „reference system“. In cases in which the parties have been socialized within sociologically and social-psychologically separate scientific sub-communities, they do not have much to fear.

How much the conceptual framework of the same concept may diverge, became clear to me in the course of teaching my introductory course when I chose the concept creativity as the common denominator in the systematic comparison of the major contemporary schools and streams in psychology²⁰. Behaviorists have unusual associations in mind when they speak of creativity, cognitivists transformations within a problem solving space, humanistic psychologists personal growth, and psycho-analysts access to impulses from one's own unconscious. This divergence is not so much a matter of logical inconsistency incommensurability, to use a Kuhnian tongue-twister - but of what I would like to call thought-psychological incompatibility, and - in relation to the scientific networks to which each group belongs - also a strong social-psychological divergence.

I do not hold a brief for divergence here. I only want to establish that in the search for agreement there is more at stake than empirical testing and consistency in deriving theories from facts, and that it is important to take this into account in theory development.

The conspicuous divergence in the way reality is constructed in the behavioral and social sciences becomes understandable, when these constructions are conceived as devices developed by humanity in the last few centuries to better understand and control the human aspects of a changing and modernizing world. The theories, research methods and change techniques current in psychology can be conceived in this light as products of what I have called the historical practice of theory construction (Van Strien, 1993a). In each historical period the members of human societies - in England in another way than in France, Germany or the Netherlands, and in America again in a different way - are confronted with different problems and challenges, and the way psychology is applied in the search for answers is a historically changing affair. Behaviouristic learning psychology, cognitive theories of memory, language, and problem solving, humanistically oriented counselling, psychoanalytic therapy, diagnosis

with the help of tests, attitude measurement and opinion-polling, etcetera, are all in their own way means of explaining, and if necessary changing, the surrounding world. Each makes use in its own way of all kinds of devices and artifices, such as experiments, questionnaires and tests, to let reality speak. The conceptual tools and notions of behaviorism, and the image of man and society they imply were developed in another time and another social context from those of cognitive psychology. Clinical psychodiagnostics is rooted in a professional and social tradition that differs radically from that of assessment on the basis of statistical formulas. Empirical testing of the theories implied, and of the efficacy of the methods and techniques used, takes place within the framework of quite differently constructed settings. In this light it is no longer amazing that it is so difficult to reach accord on an agreed upon theory and on mutually accepted operationalizations.

From this perspective, one initially surprising phenomenon becomes more easily understandable, namely, that theories and approaches which were very popular with one or more generation of psychologists, frequently suffer a rapid eclipse without having been previously refuted by convincing empirical evidence. A good example is the extinction in the sixties of the many characterological and typological systems which had enjoyed an enormous popularity on the European continent in the previous decades. After several popular books in this field still had appeared in the Netherlands in the fifties, and an evergreen like Rümke's *Introduction to Characterology* (first published in 1929) had even been reprinted two more times, interest disappeared almost overnight in the sixties. In the Netherlands this sudden disinterest can be attributed to the appearance of Kouwer's *'Het Spel der persoonlijkheid'* (The staging of personality) in 1963²¹, in which the futility of all attempts at capturing personality in a system was shown in a spirited way. But elsewhere, where no Kouwer appeared, the topic died as well. Characterological thinking had filled a place in the historical practice of a society in which psychologists could establish their value by uncovering some „true personality“ below the surface. When after the Second World War and the period of reconstruction that followed psychologists in Europe were confronted with other questions and another image of man and society began to prevail, characterological systems and typologies became obsolete as conceptual tools²².

In the history of psychology there are numerous examples of theories and systems becoming obsolete, not as a consequence of negative empirical evidence, but as a consequence of changes in the intellectual and social climate in the society in which they fulfilled a function. Thus there has been in Personality Psychology, as a consequence of forces similar to the ones just discussed, a coming and going of trait- and situational approaches. In the IQ-debate hereditary

and environmental explanations of differences in intelligence have alternated in the course of this century in a way that can not be attributed to the accumulation of relevant empirical evidence. There are many other examples that point to a similar conclusion.

The purport of my argument is not that after twenty-five years as a professor I have come to the conclusion that the empirical dimension does not count any more in psychology, and that only theoretical and world view considerations really matter. Without their regularly being put to empirical test, traditions and streams would fossilize and get a sectarian character. Diverging approaches have to remain mutually open to arguments. But in this discourse there can only be progress when due attention is paid to the wider historical frames of reference within which the clashing views have been developed. This is not as easy as it sounds, because the intellectual, social and metaphysical notions that are involved are to a large degree subconscious, and cannot be brought to awareness without sincere effort.

It is exactly here that the historian of psychology can contribute to greater clarity. Debates like the one between computationalists and connectionists, and the controversy about the modularian versus the unitarian organization of the cognitive system, can be clarified and put on a more fundamental level, when the scientific traditions in which the views at stake are rooted are laid bare. The context in which certain seemingly self-evident concepts like association, perception, drives, intelligence, socialization, attitude, repression etcetera have originated, a context of which nobody is aware any more, can be uncovered. This clarification of concepts and theories in the light of their historical origin can be called recontextualization. This is the depth-dimension which I would like to add to the scientific discussion within psychology. When the discussion stagnates, paying attention to this dimension can contribute to greater insight into the possibilities for reaching agreement on the basis of further empirical work and theoretical analysis. When it becomes apparent that there is a division on the level of fundamentals, the discussion can best be carried on at that level, i.e. reconstruction of the conceptual context as the starting-point of a discussion of first principles. In fact, this is what was done in the books on General Psychology (*Allgemeine Psychologie*) of the pioneers of the discipline²³. It would be wise to take up that line again, though making full use of the advances of a century of empirical psychology.

The relevance of placing theoretical controversies and divergent approaches to professional practice in their historical context should be sufficiently substantiated by now. As I have already pointed out this recontextualization is only one of the many good reasons for giving history a proper place in the psychology curriculum and in scientific discussion. Though the history-and-

theory perspective is probably the one that concerns me most, finding out about the world in which our predecessors lived has become for me in the course of the years more and more an end in itself. I can wholeheartedly endorse the conviction of most professional historians that history is just fun, i.e. has meaning in itself and does not need external justification.

Having called in the professional historians, we are confronted with the question whether the study of the historical dimension, just because of its significance for the discipline, is not better, because of their special training, entrusted to them. It is certainly true that good historiography demands special schooling. But psychologists who apply themselves to the history of their own discipline could familiarize themselves with the rules of the historical trade as well. The reverse obtains too, in the sense that historians of science have to familiarize themselves with the subject matter of the discipline they study. A similar remark can be made about sociologists of science and others in the field of science studies. Good mutual contacts and cooperation matter more here than specific disciplinary background.

But there is yet another, more fundamental reason why we must question whether members of a discipline are the right persons for studying its history, especially when the historical context of positions within actual controversies is at stake, debates in which, as has been the case with the present author, they have perhaps taken a stand themselves. Does this strong involvement not endanger the objectivity which is called for? Would not an outsider like a professional historian be much better able to maintain the required distance?

The issue distance versus commitment has a wider scope than the historiography of science. It is also possible to question whether a woman is the most appropriate person to write the history of women's emancipation, or whether a Dutch architect can write an impartial history of architecture in the Netherlands or - still one further step - whether within the guild of historians a foreigner like Schama probably would have been better fitted than a Kossmann to write the history of the Low Countries²⁴.

In my attempt at answering this question, I want to stress first that a special attitude is required for writing a reliable history of one's own science. Emeriti, though seasoned in their discipline, are not necessarily good historiographers. In spite of its many merits, Boring's well known *History of Experimental Psychology* ((1929)1950) has too much the character of a Whig History, in which the experimental line in the discipline which he represented himself was provided with a respectable genealogy - the fault of much preface history, i.e. historiography in which the present state of the art is seen as the provisional summit of an ascending line, and in which the past is presented, from this perspective, in a selective or even distorted way.

Yet, apart from these dangers, involvement in the subject also has apparent advantages. A scholar who has followed the issues which are at stake from within is in many cases better equipped to recognize crucial continuities and resemblances than most outsiders. A historically well versed psychologist like Scheerer (1985) was able, precisely because of his familiarity with German psychology, to uncover the organic *Weltanschauung* and political ideology behind *Ganzheitspsychologie* in Nazi-Germany. Though presented in a detached, objective tone, his analyses contributed to the critical historical discussion, and perhaps to the *Vergangenheitsbewältigung* in German psychology. A theory - let there be no misunderstanding - is not refuted when its dubious origin has been demonstrated. Newton's mechanics was part of a curious half-religious alchemistic thought-construction²⁵. Yet nobody will dispute the relevance of his *Principia* for that reason. Still, reflection on the principles and notions involved in the scientific tradition to which one's own theories and methods, or those of others in the same field, belong can become a reason for critical reflection and criticism.

Yet the fact that they are more conversant with the subject matter is not the primary ground for my case for entrusting the study of the historiographical depth-dimension of the cube of science primarily to members of the discipline at issue. The principal motive is the creative tension that is caused by the necessity of rendering an account of the findings to a double forum: that of historiographers and that of other members of one's own discipline. The presentation of their own work at conferences and in historical journals to others engaged in historiography compels historiographers of their own discipline to observe the necessary distance and objectivity. The presentation within their own discipline, and the ensuing discussions with colleagues who are primarily focused on the other dimensions of the cube keeps the commitment alive. The tension involved with being part of a multiple system of reference-relationships constitutes a situation that usually enhances creativity²⁶.

That there indeed is a tension between distance and commitment - and should be, I would like to add - is a corollary of the resistance of most representatives of „positive science“ towards a critical examination of the presuppositions of their own position, implied images of man and society, the modish character of metaphors serving as heuristic, the resemblance to ideas produced by predecessors, etcetera. The relativization that is inherent in historical analyses unavoidably meets with opposition. I am even inclined to say a good historian of his own discipline should be experienced as a flea in their bed by the other members of the discipline. If the historian remains unnoticed, he or she has failed to disturb their dogmatic slumbers.

By the same token historians of their own discipline should not confine themselves to the study of other centuries and far away places. And as far as they do this, they should do it in a way that is critical of misrepresentations that might have originated from the dubious forms of historiography referred to above. Studies in this sense have for instance shown that associationism can only with great reserve refer to Aristotle as a forerunner (see Danziger, 1990b), and that Wundt has fathered, apart from laboratory psychology, other children as well (see Danziger, 1983). To keep the tension with one's own discipline at the desirable voltage, I have directed the Groningen History of Psychology program towards psychology in the Netherlands. I am pleased that there are both in this forum and in the body of this gathering enough historians of science from outside the Netherlands and outside psychology to watch over the necessary distance.

Footnotes

1. The author held the chair of work & organizational psychology at the University of Groningen from 1976-1980 and was then appointed to this present chair: Introduction, Foundations and History of Psychology at the same university.
2. Not published.
3. Most publications to which I refer in this paper are in Dutch. For the convenience of the reader the titles have been translated into English. In the references only internationally available titles are listed. In that case the year of appearance is entered in brackets in the text or in the notes. For those who are interested a full bibliography is available on request.
4. I presented a poster of this theme at the First ISTP-conference at Plymouth (1985), published in 1987 in the German *Nachrichtenblatt* (van Strien, 1987).
5. In 1993 Dehue's book will appear in an English translation, published by Cambridge University Press.
6. See van Strien & Verster (1987) and van Strien (1933^b).
7. Helmuth Plessner's major work is: *Die Stufen des Organischen und der Mensch* (1928). A tribute to my debt to Plessner can be found in van Strien (1987).
8. Because the publications which are relevant in this context are nearly all in Dutch I will not list them. References can be found in my *Kennis en Communicatie*, Utrecht: Bijleveld, 1966.
9. Though in a recent poll Kouwer was mentioned as one of the three most favorite authors among the generation of psychologists who entered the field before 1970, none of Kouwer's publications has been translated.
10. See a. o. de Groot (1954) and de Groot (1961, 1969).
11. See van Strien (1978) and van Strien (1982).

12. An English summary of my book *Praktijk als wetenschap* (Practice as science) (1986) appeared in the European Cheiron Newsletter (winter, 1988).
13. See van Strien (1987^b).
14. See van Strien (1990).
15. The first publication in which this model appears in Van Strien & Dehue (1987); it is further developed in my contribution to the long overdue Volume VIII Theory and History of the Annals of theoretical psychology (Van Strien, 1993^a in press).
16. These six reasons can be briefly circumscribed as: (1) historical knowledge widens or cultural and intellectual horizon; (2) it makes us aware of relevant earlier work and it can help us to trace continuities and changes in the history of problems, concepts and ideas; (4) it sharpens our eye for the historical relativity of contemporary theorizing; (5) by placing contemporary theories in their historical context, it deepens our understanding of the theoretical and methodological presuppositions, images of man and further notions involved; (6) it gives us a better understanding of the dynamics of the development of science in its social and intellectual context. In the present paper the fifth argument occupies a central role.
17. W. K. B. Hofstee (1980). *De empirische discussie; theorie van het sociaal-wetenschappelijk onderzoek*. Meppel: Boom. A succinct presentation of the thrust of the betting model can be found in Hofstee (1987).
18. De Groot's first emphatic plea for the theoretical discussion can be found in his Groningen inaugural address: 'Over regels en taken van het Forum in de gammawetenschappen'. Meppel/Amsterdam: Boom (1980). For a presentation in English, see for instance De Groot (1984).
19. This argument is further developed in van Strien (1993^a). After the conference Hofstee has raised his voice against this 'undermining of the factual basis on which science rests'. The debate which followed (in Dutch), in which T. Dehue has participated too, is not published.
20. See note 4.
21. See note 9.
22. See Derksen & Van Strien (1992).
23. For instance W. Stern's *Allgemeine Psychologie*. Haag: Nijhoff, 1935.
24. Schama (1987) wrote a perceptive study on 17th century's Dutch prosperity; Kossman's history of The low countries appeared in an English translation in 1978.
25. See Manuel (1974).
26. See van Strien (1991 and 1993^b).

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Author: Pieter J. van Strien is professor of psychology at the University of Groningen.

Address: Foundations and History of Psychology. Psychologisch Instituut Heymans, Grote Kruisstraat 2/1, 9712 TS Groningen, The Netherlands.